

# MY LIFE WITH COLD FUSION AS A RELUCTANT MISTRESS

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Over nine years have passed since many of us were lured into believing that the Pons-Fleischmann effect would solve the world's energy problems and make us all rich. Things have not yet worked out as we had hoped. Each of us have followed a different path through the labyrinth of this expectation. I would like to share with you my particular path and show you how I came to believe that problems of reproducibility are caused solely by the properties of the materials in which the nuclear reactions are proposed to occur.

When the announcement was made, I was working at the Los Alamos National Laboratory (LANL) on rather conventional materials problems associated with trying to design nuclear reactors for use in space. Suddenly, the possibility of making nuclear energy in a Mason jar was the center of attention and conversation at LANL. Meetings were held and memos flowed freely—typical of a government operation. Excitement was intense, causing previously untapped creative juices to flow and an intensity of communication within the laboratory not seen since the War. Dozens of imaginative experiments were started using funds otherwise destined for the design of better atomic bombs. In my case, a search for tritium production seemed to be the most logical approach, because I was located in a building where worked some of the world's experts in the properties and detection of tritium. They know tritium when they see it. So, with the financial backing of the DOE, my future wife (Carol) and I set about trying to verify the claims for tritium production using the Pons-Fleischmann effect.

Our approach was to electrolyze heavy-water in a closed cell designed to collect tritium produced in the electrolyte in a separate compartment from that present in the evolving gas. Because we planned to do many experiments, in order to explore a wide range of variables, the cells were designed to be cheap and simple. As you can see in Figure 1, the evolving  $D_2$  and  $O_2$  pass through a recombiner and the resulting  $D_2O$  is collected in an IV bag. For a brief time, we were the major user of IV bags in Los Alamos. The method allowed us to keep a complete inventory of all material within the cell including any tritium. Over 250 cells were studied using palladium from many different sources containing many different introduced impurities. Unfortunately, only thirteen cells produced excess tritium and the amount was rather small. Typical results obtained from active and inactive cells run at the same time are shown in Figure 2. Note the delay in tritium production followed by a rapid onset with bursts. In the process, we made an important discovery. When tritium is produced, it always appears first in the electrolyte, not in the evolving gas. This is important because it indicates that tritium is being produced only at the surface and it leaves the sample before it has a chance to dissolve in the metal lattice. The implications of this behavior will be described in more detail a little later. Unfortunately, we could find no relationship between its production and the nature of the palladium, hence the effect was not reproducible. Being highly controversial, the resulting paper<sup>1</sup> was reviewed by twelve people at LANL and by several more reviewers after it was submitted to *Fusion Technology*. Even though this intense scrutiny found

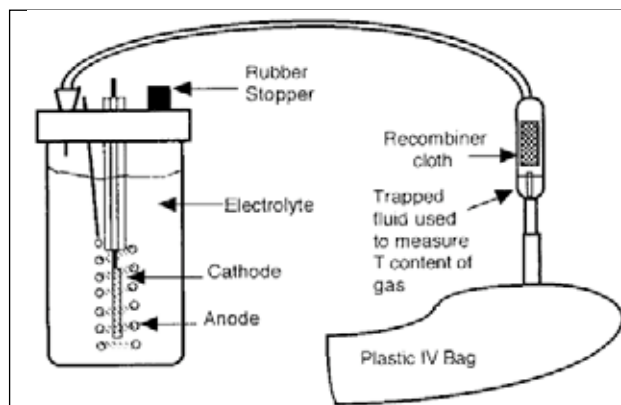


Figure 1. Drawing of closed cell used to study tritium production.

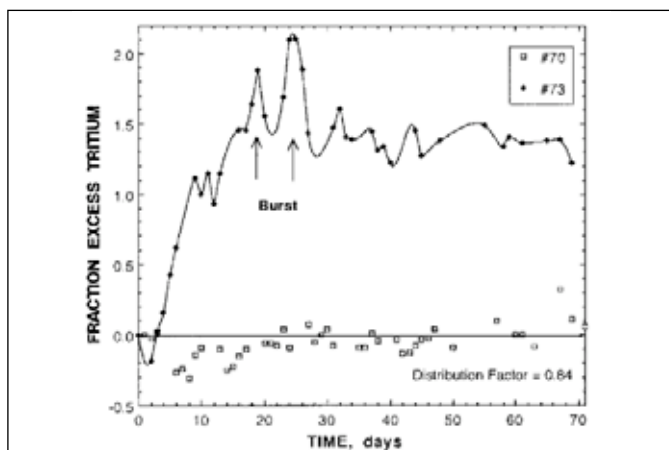
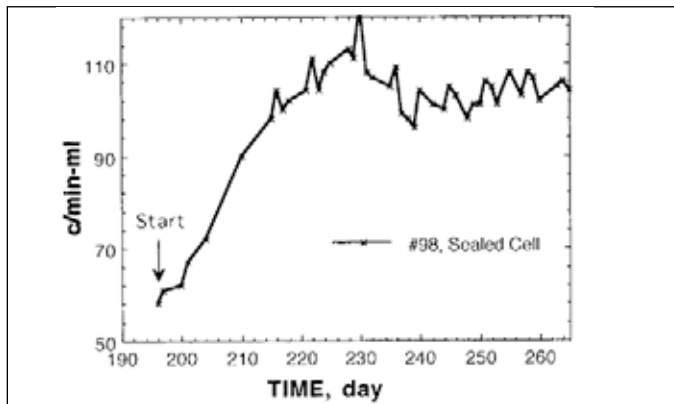


Figure 2. Typical behavior of active and inactive cells in which tritium was measured. Both cells were run at the same time. Note the four day delay before tritium was produced in cell #73.

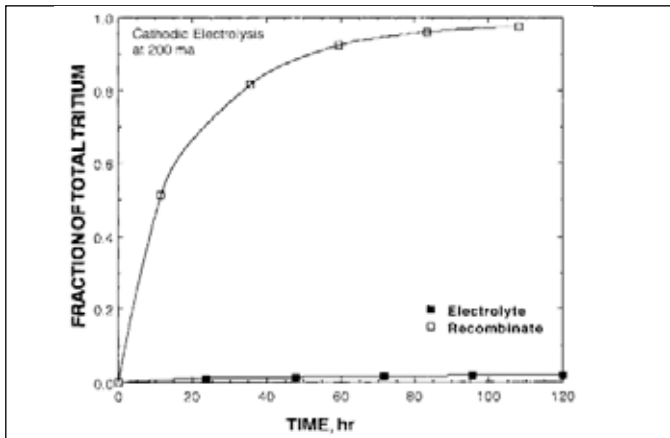
no fatal flaws, the results, although published after peer review, were universally ignored. The papers that skeptics publish should be so carefully analyzed.

One of the major rationalizations used to reject such work was the assumption that tritium was already in the palladium or came from the surrounding environment. Consequently, we set about to test these assumptions. We had already made numerous attempts to find dissolved tritium within the initial palladium and within the environment without success. So we chose the opposite approach. We placed cells in an environment known to contain tritium. In addition, we dissolved a known amount of tritium in palladium destined for electrolytic study. If the behaviors seen during the proposed cold fusion production matched those found using known tritium additions, then an explanation would be obvious.

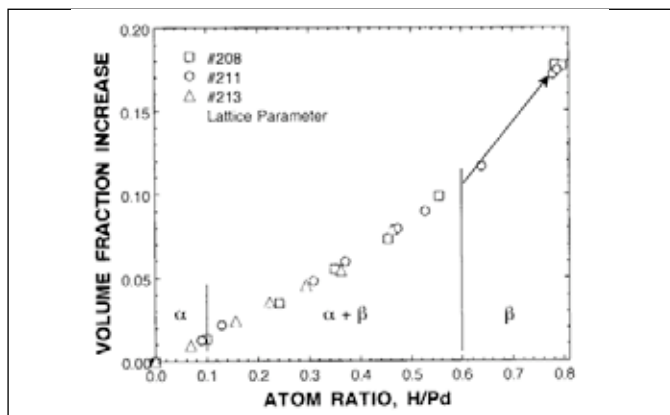
When a cell was placed in a tritium environment, the tritium content of the electrolyte slowly increased at a linear rate, as expected (Figure 3). In contrast, anomalous tritium was seen to occur only after a delay and then was produced at a high rate over a short time with bursts (Figure 2). In addition, similar cells in the same, clean environment showed no effect while tritium was growing in a neighboring cell. Consequently, the observed behavior of cold fusion production of tritium and that obtained from the



**Figure 3.** Pickup of tritium in a sealed cell attached to an IV bag located within an environment containing tritiated water vapor.



**Figure 4.** Growth of tritium in the electrolyte and in the evolving gas as a sample of palladium containing tritium was electrolyzed as the cathode.



**Figure 5.** Increase in volume produced by loading palladium with hydrogen.

environment show entirely different patterns of behavior.

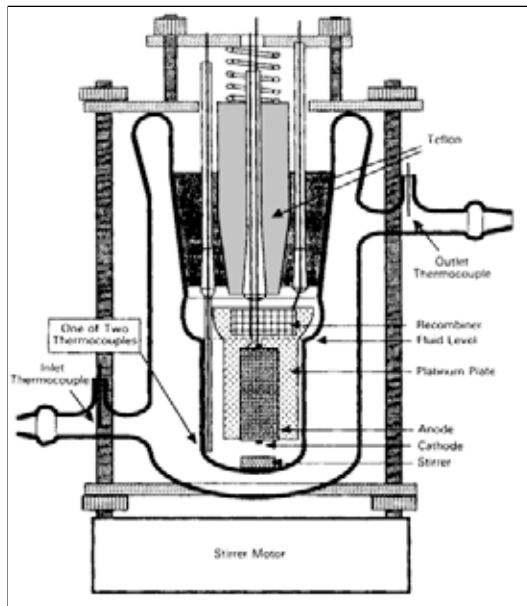
Tritium which was placed in the palladium on purpose always appeared first in the evolving gas, not in the electrolyte, during electrolysis. As can be seen in Figure 4, the contained tritium was released immediately after electrolysis started and continued at a steadily reduced rate as electrolysis continued. Loss into the gas was caused by a first order reaction for which a half-life could be determined. Again, the behavior of tritium claimed to be produced by cold fusion and the behavior of known tritium were different. To us at least, this study demonstrated that neither contaminated palladium nor the environment were the source.<sup>2</sup> Skeptics were forced to propose that tritium released in "cold fusion" cells was not dissolved but was present as isolated impurities to which tritium was tightly bonded. Never explained was the mechanism of its subsequent

rapid release after hours of electrolysis. Unfortunately, this work was also completely ignored.

Individual studies always have errors which rightly introduce doubt. On the other hand, a series of studies using different approaches that produce patterns of behavior are much more difficult to reject. In this case, the observed patterns are completely consistent with anomalous tritium being produced within the cell and, more specifically, in the surface of the palladium cathode. Nevertheless, people of a skeptical mindset would propose terribly unlikely and convoluted processes to explain individual results while ignoring the patterns. Amazingly, this method of rejection seems to satisfy many people in academic science these days.

During this study, we began to appreciate how important crack formation was in determining the local concentration of deuterium. Most palladium grows an increased crack concentration each time it is loaded. This crack concentration is measured as a volume increase over that expected from the lattice parameter change, shown in Figure 5. Only a very few samples did not show this effect. Using tritium as a tracer, we were able to quantify the effect of cracks on the loss rate of hydrogen from palladium. In general, the higher the excess volume, the faster tritium left the sample through the cracks as gas, thereby entering the gas rather than being dissolved in the electrolyte as ions. In this case, tritium was also being used as a tracer for deuterium loss. Thus, both tritium and deuterium, previously dissolved in the PdD, left the structure through cracks as gas rather than by ion exchange at the surface. Only tritium produced by the anomalous reaction entered the electrolyte. Consequently, we could conclude that most palladium acted like a leaky bucket which could never be filled. Since we know that high deuterium loading is a requirement for excess energy production, these cracks are apparently a major hindrance in achieving the required high deuterium concentration. Unfortunately, this work had no influence on the skeptics and very little influence on those people who were also trying to reproduce the effect.

About a year later, I wrote a review<sup>3</sup> which was published in *Fusion Technology*. Using this collected experience as an argument to look for heat production, I convinced my Division Leader at LANL to fund a calorimetric study. The calorimeter was sealed, closed and stirred, all requirements demanded by various skeptics before claims could be believed. A drawing of the device is shown in Figure 6. For our first study, I was given a piece of palladium from a batch made by Tanaka Metals which had been shown to produce excess energy by Prof. Takahashi in Japan. Amazingly, his sample also produced excess energy in my calorimeter. The most dramatic of the various examples of excess heat production from this sample is shown as a function of time in Figure 7. As you can imagine, this unexpected result was analyzed every which way by numerous people to discover the source of the apparent energy. The calorimeter was calibrated using an internal heater many times during the study and three times during excess energy production, with no apparent change. Unfortunately, the study had to be terminated prematurely because the internal recombiner began to fail. After the calorimeter was repaired, some additional excess energy was seen, but the sample soon died. Because we were sensitive to the influence of cracks, we measured this piece and found very few of the little devils (1.7%). A second piece of palladium from a different batch, but one claimed to have been made in the same manner, was delivered from Japan and studied. This piece was found to produce no excess energy, but contained a high concentration of cracks (13.5%). After informing the Japanese of this fact, a third batch was made. But this time,



**Figure 6.** Drawing of the calorimeter used at LANL. The cell was sealed, contained a recombining, and was stirred. Temperature was measured at two positions within the cell. The deuterium content of the palladium cathode was determined by measuring the change in deuterium pressure.

the conditions were as close as those used to make the first batch as was possible. This material was found to make excess energy, although less than the first batch. Also, it was found to contain a crack concentration slightly greater than the first batch. From these results, it is clear that the crack concentration is an important variable. Table 1 compares the behavior of several samples made in Japan. While crack concentration (excess volume) is important, it is not the only variable having influence on excess energy production.

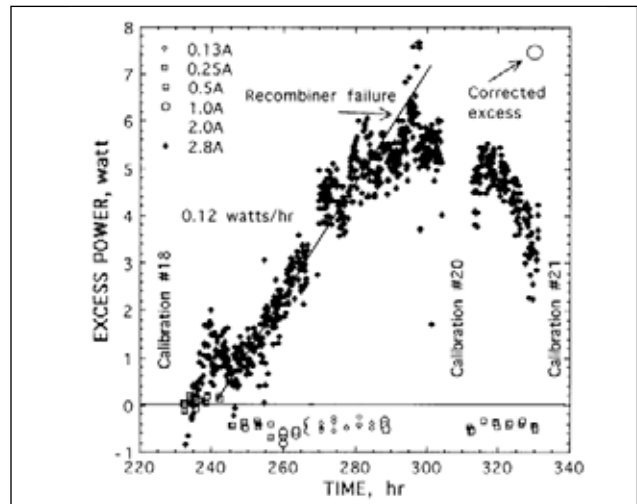
**Table 1**  
**Comparison between measured properties and excess power production.**

Sample Number	Excess Volume, %	Composition D/Pd	OVC*	Excess Power, W (at 3 A)
Tanaka 1	1.7	0.82		7.5
IMRA #38	2.8	0.875	1.03	3.2
Tanaka 3	2	0.84		2
IMRA #42	1 to 2	0.891	1.25	4.6
IMRA #84	6.7	0.752	1.00	1.5
IMRA #58	4.1	0.833	0.60	0.0
Tanaka 2	13.5	0.75		0.0

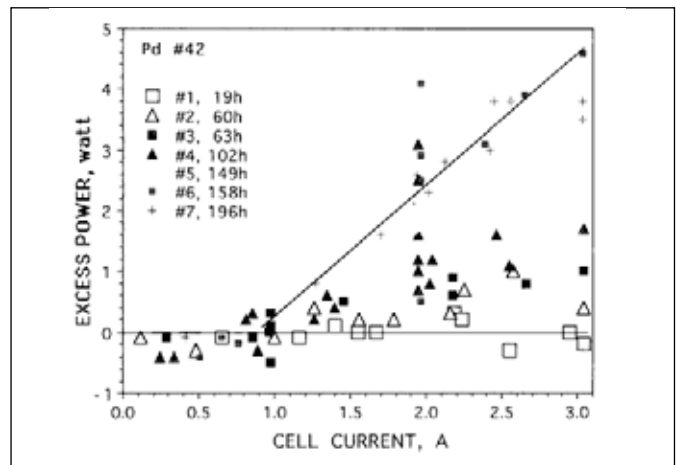
\* Open circuit voltage

Once again, this information, when published,<sup>4</sup> had very little effect on other people's work. It did, however, start me down a path to answer the question, "Why is the effect so difficult to produce?" In addition, this experience started a search for ways to pretest palladium. Meanwhile, conventional science was taking the opposite approach—saying because the effect could not be reproduced at will, it was not real.

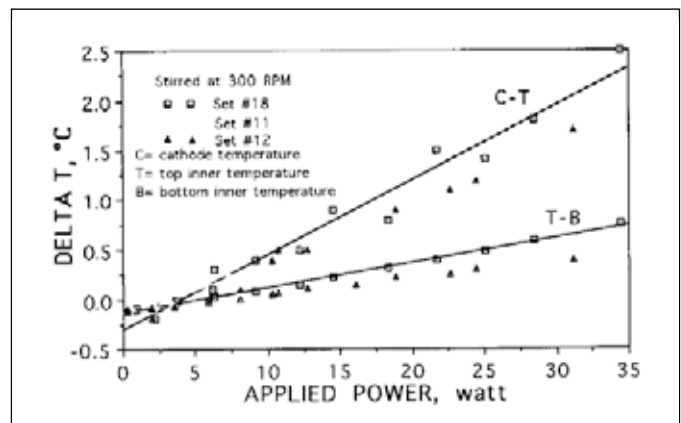
About this time several events occurred to give many of us some optimism. Thanks to Eugene Mallove, Rep. Dick Swett (NH) asked me to testify before a congressional subcommittee which was reviewing future funding for "hot fusion."<sup>5</sup> Unfortunately, this effort had no effect on future actions by the government toward "cold fusion." Then, in spite of increasingly vocal opposition to the whole idea, *MIT Technology Review* took the courageous step of asking me to write an article for them.<sup>6</sup>



**Figure 7.** Time variation of excess power production using Takahashi palladium with various applied currents. The calorimeter was calibrated before, during, and after the study. No significant changes were noticed. Excess power was observed only above a critical current. The "correct excess" value is calculated assuming no recombination is occurring at that time.



**Figure 8.** Excess power as a function of applied current after electrolyzing for various times.



**Figure 9.** Temperature difference between the cathode and the top of the electrolyte and between the top and the bottom of the cell. No excess power was detected during Sets #11 and #12. Excess power was detected during Set #18.

Although this article stirred up some heated debates, it also had no significant impact on the course of the field.

My work in cold fusion at LANL was meeting increased resistance and my new wife and I wanted to build a home in Santa Fe. Retirement began to look very attractive. Amazingly, they were even willing to pay me a bonus to leave. After we

finished the house and a laboratory, I began to study ninety pieces of palladium furnished by IMRA (Japan). Some of this material produced excess energy as shown in Figure 8 and listed in Table 1. In addition, a temperature gradient was found to occur within the cell during excess energy production such that the cathode appeared to be the source of energy, as shown in Figure 9. This sample was found to be unique in that the low excess volume did not grow larger upon repeated deloading-loading cycles. Energy production was very difficult to kill, returning after a short delay even when the surface was removed by Aqua Regia. The study also demonstrated that palladium could be pretested, thereby reducing the growing frustration by eliminating most material from which energy could never be obtained. This study was published in *Infinite Energy*<sup>7</sup> [Ed. note: *Errata* regarding figures in this previously published paper to appear in a forthcoming issue]. In addition, the study showed cracking to be a highly variable property and pointed out a number of other variables, besides cracking, as being important. These included the deloading rate after the current was stopped, the open-circuit-voltage, and the loading efficiency. As my laboratory grew, thanks to Dave Nagel (NRL) and Fred Jaeger (ENECO), computer control could be introduced allowing these variables to be explored in more detail. Unfortunately, the results of this study<sup>8</sup> were held up for about a year by the then editor of *Journal of Electroanalytical Chemistry* (JEAC), apparently because the paper mentioned cold fusion. The work was recently published in *J. Alloys and Compounds* and described at ICCF-7.

These studies provided insights into the nature of the regions in which the nuclear reactions actually occur. These regions I call a special condition of matter containing nuclear-active-states (NAS).<sup>9</sup> It is interesting to examine some of these results in detail. In order to produce "cold fusion" or more exactly "chemically assisted nuclear reactions" (CANR), the hydrogen isotope must achieve a very high local concentration. To do this, the atoms must get into the metal lattice though a surface barrier, and the atoms must stay in the lattice regardless of there being many avenues for escape. The "getting in" and the "getting out" are independent variables having wide ranges of values which depend strongly on the nature of the metal. Once regions of high-concentration are achieved, changes must take place such that the atomic and electronic structures are altered to produce unique conditions—a new chemical phase. This transition is also sensitive to a variety of conditions in addition to the hydrogen concentration. In short, two major conditions must be achieved—a high hydrogen concentration and then conversion to a new phase having a composition well above that of normal  $\beta$ -PdD. Each of these conditions is affected by dozens of variables. It is little wonder that the effect is difficult to reproduce. The "going in" can be examined using the loading efficiency; this quantity being the ratio created when the number of atoms of hydrogen presented to the surface by the applied current is divided into the number which actually dissolve in the metal, each measured over a five minute interval. Figure 10 shows the behavior of an untreated sample and one whose surface barrier was reduced using Aqua Regia. Needless to say, a poor "going in" rate needs to be avoided. The "going in" rate is also sensitive to applied current and temperature. The "going out" rate can be studied by measuring the deloading rate after the current is turned off. This rate is obtained from a plot of average composition vs. square root of time, as shown in Figure 11. The initial slope is used to evaluate samples. The slopes for numerous samples are compared in Figure 12 as a function of maximum average composition-

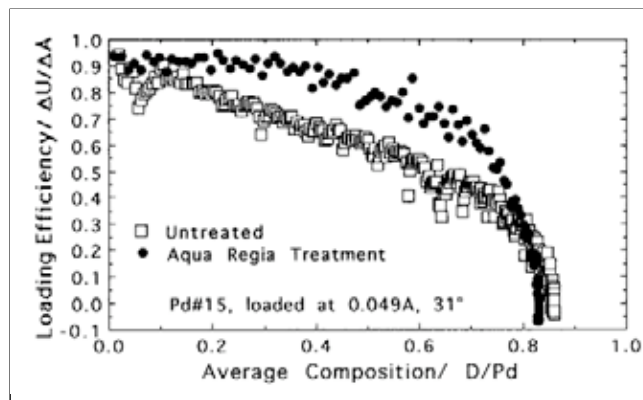


Figure 10. Effect of a surface barrier on the loading efficiency.

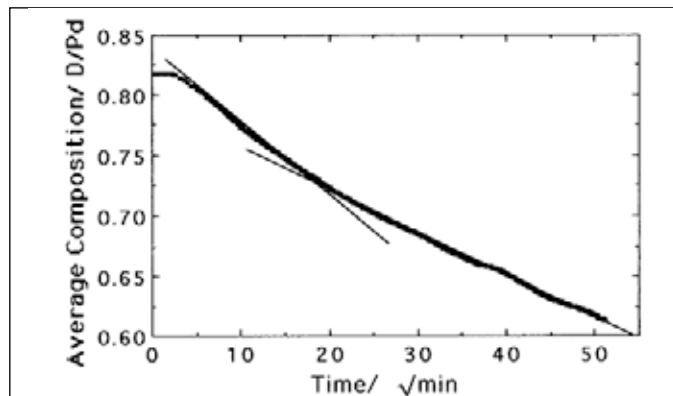
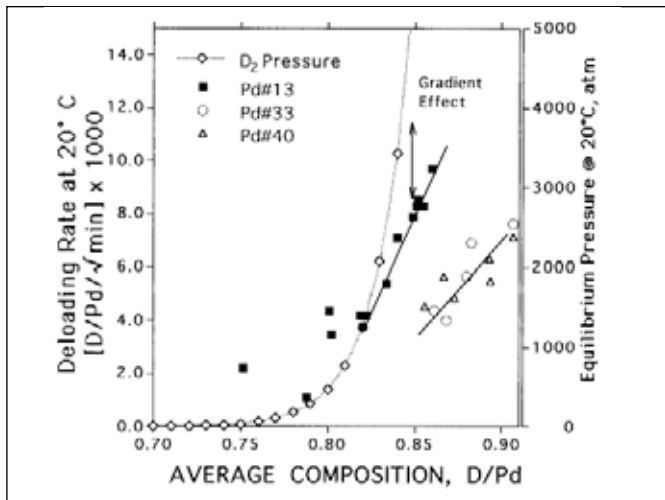


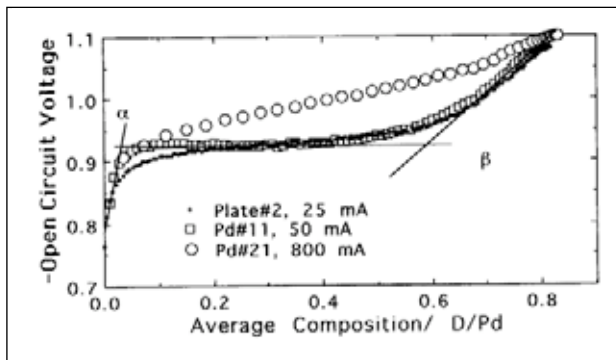
Figure 11. Change in average composition vs square root of time after applied current is stopped. The initial delay is caused by inertia in the composition measuring system. A break in slope occurs when  $\alpha$ -PdD forms on the surface.

tion. Two conclusions are obvious: the loss rate increases as the initial composition is increased and some samples have an abnormally low rate even though they have a high composition. From this and other arguments, I conclude that this same loss is occurring while current is being applied. In addition, the rare piece of palladium which is able to achieve very high average compositions does so because it has a low loss rate, in addition to several other features. Consequently, both the loading efficiency and the deloading rate can be combined to quickly identify potentially nuclear-active palladium, in addition to using material having low excess volume.

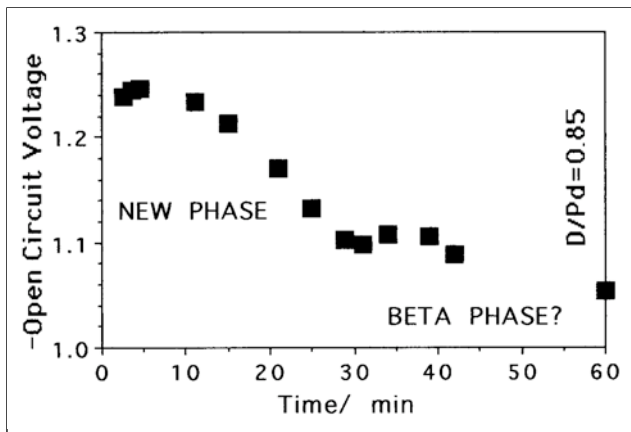
Unfortunately, the average composition used to obtain these two quantities is not the important variable. The highest composition exists on the surface and it is this composition that determines whether a sample will become nuclear-active. Fortunately, changes in surface composition can be estimated using the open-circuit-voltage (OCV), a value which is sensitive to the chemical activity of hydrogen in the surface. A value is obtained by measuring the voltage between the cathode and a platinum reference electrode while the current is turned off for a few seconds. Figure 13 shows how the OCV changes as a palladium sample is loaded with deuterium. A reverse of this behavior is frequently seen when a sample is allowed to deload after the current is turned off. However, occasionally, especially after very high average compositions have been achieved, the OCV shows a different behavior as seen in Figure 14. This behavior indicates that a new phase has formed which slowly decomposes into normal  $\beta$ -PdD as deuterium is lost. The next question needing an answer is: "What is the actual composition of this phase?" A partial answer can be obtained by studying very thin films of palladium plated on to platinum. As shown in Figure 15, the measured composition of such films is highly



**Figure 12.** Variation of deloading rate as a function of average composition. Also shown is the equilibrium pressure within voids for the indicated compositions. The gradient effect is caused by a reduced composition at the surface of a crack produced by deuterium loss.

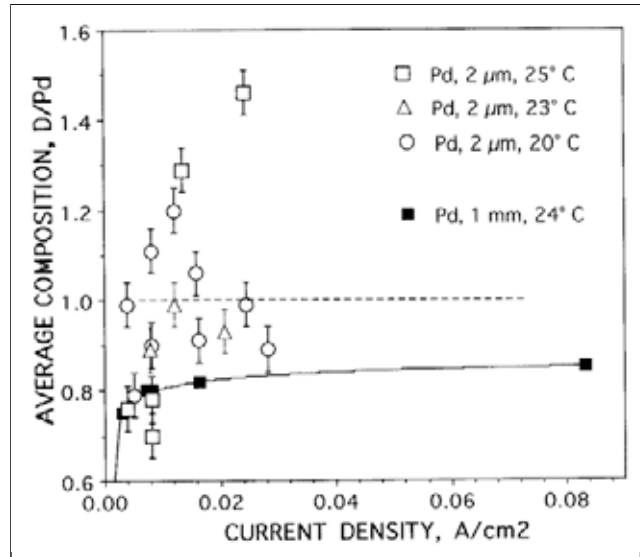


**Figure 13.** Open circuit voltage measurement during loading at various currents referenced to platinum.

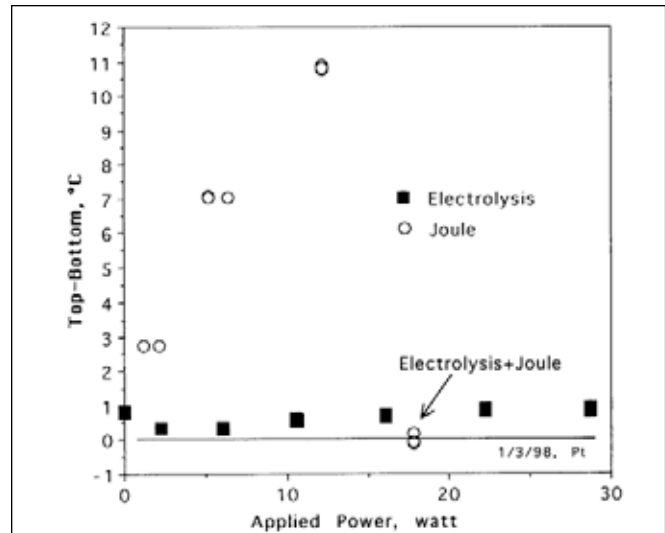


**Figure 14.** Open circuit voltage during deloading after production of excess energy.

variable but can achieve a composition as high as  $D/Pd=1.5$ . In this case, the surface region containing the high composition is not as diluted by the smaller interior composition as would be the case if a thicker sample were used, such as shown by the lower curve. Since this particular film was not nuclear-active, the composition of a nuclear-active surface is probably significantly higher. In addition, deloading from a surface is very nonuniform, as can be seen by examining bubble production. Therefore, the maximum composition of such a thin film, or indeed any palladium surface, is well above the average value in addition to being above  $D/Pd=1.5$ . This observation means that all theories based on the properties of  $\beta$ -PdD are barking



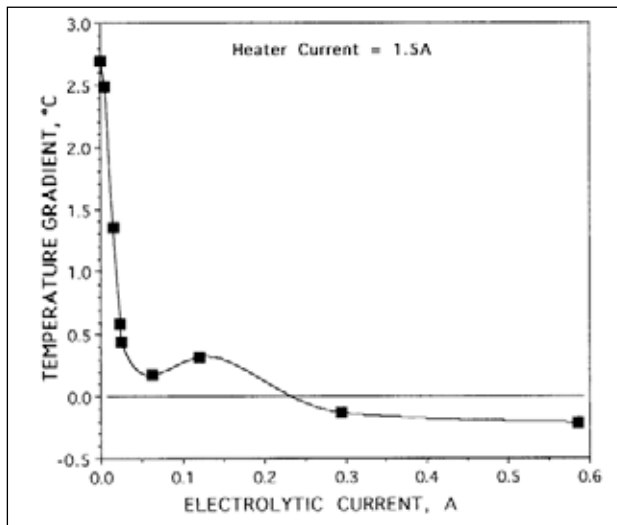
**Figure 15.** Measured average composition of thin films of Pd plated on Pt after being subjected to different current densities. The behavior of a typical thick plate is also shown.



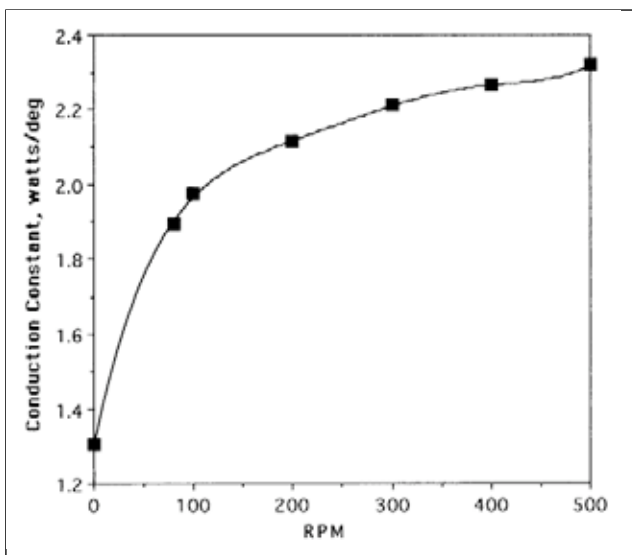
**Figure 16.** Gradient between the top and bottom of the electrolyte as a function of applied power.

up the wrong tree. I have proposed the actual nuclear-active phase to be  $PdD_{2+x}$ .<sup>9</sup>

Meanwhile, the results of this study were used by ENECO in an attempt to convince the Patent Office that the lack of reproducibility was caused by limitations inherent in the chosen material. This argument was completely ignored. In addition, I wrote another review in an attempt to bring all of the better data under one roof so that even an open-mind-challenged person could see the bigger picture. This was published in *J. Scientific Exploration*<sup>10</sup> with brief versions in *21st Century Science and Technology*,<sup>11</sup> and in an earlier volume of *J. Scientific Exploration*.<sup>12</sup> Again, no change was produced at the Patent Office nor in any other government agency. Recently, a third and final review was rejected by the *International J. of Modern Phys.* even though one of the Editors requested the paper. Amazingly, four reviewers rejected its publication for reasons that appeared to me to be based on a bias against cold fusion. This work was published in *Infinite Energy*.<sup>13</sup> You can decide for yourselves whether this review deserved such treatment. My recent experience suggests that many avenues for publication previously available no longer provide the service because reviewers have



**Figure 17.** Reduction in gradient as electrolysis current is increased at a fixed heater current.

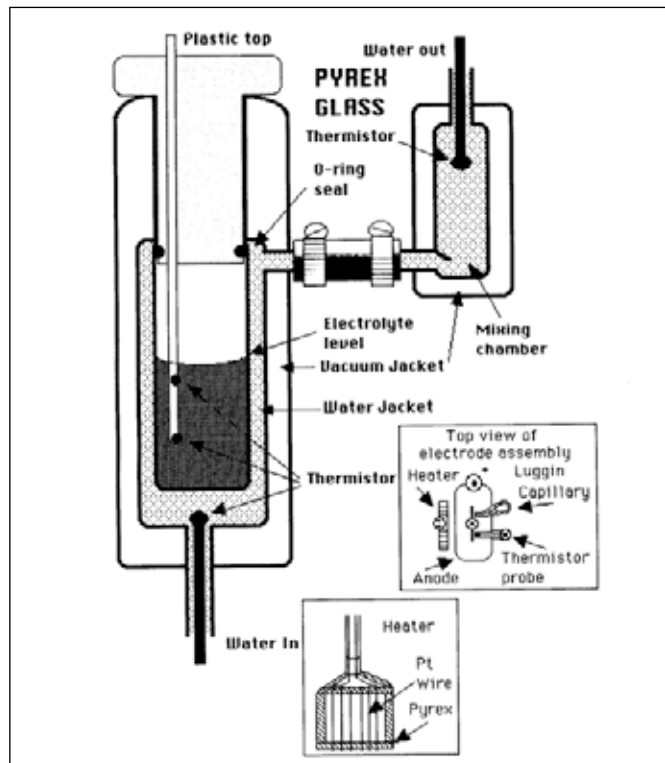


**Figure 18.** Thermal conduction of the cell wall as a function of stirring speed using an isoperibolic calorimeter.

become even more unwilling to give cold fusion the benefit of doubt. It is depressing to see resistance increase as the experimental results and understanding improve.

Last year, Dr. Naoto Asami sent me some palladium that the New Hydrogen Energy Laboratory (NHE) had made at great expense and to their specifications. Unfortunately, they could detect no excess energy using this material. As expected, my tests showed this material to be flawed—material I would expect to be completely inert. After hearing this, Dr. Asami invited me to Japan where we were able to discuss the problem in some detail. Apparently, impurities were being introduced into the palladium during manufacture as well as during subsequent annealing. We were able to eliminate the impurities added by annealing. Unfortunately, insufficient time and money were available to change the method of manufacture. Consequently, NHE closed down without exploring the use of proven, active palladium.

Recently, I have been exploring the various errors besetting calorimetric measurements. I have discovered that stirring is not an important variable when electrolysis is used, in contrast to what has been claimed by some well know skeptics. This can be seen in Figure 16 where the gradients produced by an internal heater and by an electrolysis current are shown. A small



**Figure 19.** Drawing of the dual calorimeter. The cell is sealed, contains a recombiner, and a Joule heater. Temperature is measured at three locations within the cell.

electrolysis current added to the Joule heating reduces the gradient to insignificant values (Figure 17). On the other hand, the stagnate layer of fluid at the cell wall is important when isoperibolic methods are used. This layer influences the thermal conductivity of the wall and is very sensitive to the amount of fluid convection. Figure 18 shows how the calibration constant changes as the stirring rate is increased. In the absence of mechanical stirring, bubble generation serves the same end although not to the same degree. This study shows that claims for small amounts of heat using unstirred isoperibolic calorimeters may be in error. Pons and Fleischmann largely avoided this problem by frequently calibrating their cell, although the effect may still have some influence on their data especially at the lowest claimed excess power levels. Other studies, especially if they use Joule heating to calibrate the cells, are not so fortunate. In my work, calibration was based on electrolytic heating, with Joule heating being used only to determine whether the calibration constant had changed while electrolysis was ongoing.

I have also built a dual calorimeter (Figure 19) which is being used to study electroplated palladium on platinum and search for evidence of superconductivity in nuclear-active-material. This method greatly reduces the likelihood of misinterpreting apparent excess energy. Electroplated palladium appears to have a much greater likelihood of success compared to bulk material, and it is cheaper.

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. It is unfortunate that these brave and creative thinkers had to take so much pain and be denied the rewards of

their discovery by closed-minded colleagues and an incompetent Patent Office. And third, the field is sick and on life support. Major sources of financial support have dried up, many self-funded individuals are moving on, and conventional rejection has solidified. Those of us who would like to see this field grow are encouraged by a few impressive successes, such as Case, Arata, and Stringham, but even these approaches are woefully underfunded and are being studied by a very small number of individuals. We most hope that when the spectacular demonstration demanded by skeptics is found, a way is available to make this fact generally known.

The amazing claims for 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. The most likely possibility is that the entire field, heat and transmutation, will get noticed only after the price of oil skyrockets in the next few years or if there is a major spill of highly radioactive material from a waste dump. Meanwhile, a very objective article in *Wired* magazine<sup>14</sup> will at least wake up the public to this issue. Hopefully, some



Drs. Carol and Ed Storms

awareness will trickle down from the public to the scientific profession, the reverse of the usual procedure. As for me, I will continue to study the effect as time and money permit. However, additional useful knowledge demands sophisticated techniques that I presently do not have available. This limitation makes continued study of this effect a matter of diminishing returns for me. Therefore, I am gradually turning my attention to other problems—such as earning a living and preparing for Y2K. Giving up this mistress is hard for me but very attractive to my wife, as you might expect. Eventually, the field will be discovered by other people, hopefully by someone who can afford to marry her.

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13. E. K. Storms, "Cold Fusion Revisited," *Infinite Energy*, 4, #21 (1998) 16.
14. Charles Platt, "Dirty Science: The Strange Rebirth of Cold Fusion," *Wired*, Nov. 1998, page 171.

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applications. As a mathematical physicist, Sternheimer is developing a new twist in quantum theory, where the particle waves are not simply sinusoidal, as in classical QM, but have harmonics too. So he thinks that in CF, hydrogen electrons might jump to a lower harmonic, thus achieving fractional quantum states (*a la* Mills or Vigier), getting closer to the nucleus and gaining energy.

Alexandre Szames presented his big fat book (500 pages, three years work) on the Biefeld-Brown effect. This twenty-nine year-old writer has an impressive scientific vitae (puts me to shame, easily!) and did fascinating experimental work with help from Naudin and Cornille. They conclude that Biefeld-Brown effect is NOT anti-gravity, but a crucial proof of the existence of aether (thus invalidating general relativity) and stimulated forces, opening the way to over-unity and new propulsion concepts. Just after the Festival, Szames flew to the U.S. to participate in NASA's latest seminar on breakthrough propulsion in Albuquerque, New Mexico. I expect some news from him when he returns.

Lastly, another sign that O/U or FE research is gaining ground in France: we finally have a French language web site on these topics, thanks to Jean and Bernadette Soares and their association, Quant, Homme. For years they did a wonderful job translating articles and information, and now they have a site full of links and references, at [<http://multimania.com/quanthomme>].

Full program is available (in French only) at :  
<http://www.transfert.net/mag/reportag/pages/m1frep.html>



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