

Encounter with a Cover-Up: Examining a Forbidden Report

*Proceedings of the EPRI-NSF Workshop
on Anomalous Effects in Deuterated Metals*
October 16-18, 1989—Washington, D.C.

by Eugene F. Mallove

For years I had been hearing about these now seemingly ancient proceedings of a restricted access cold fusion meeting that was held in October 1989, but I was not sure a finished document had been compiled. I certainly knew that the meeting had occurred (see pp. 181-185 of *Fire from Ice*),¹ but that a detailed report had finally issued had been kept very quiet. That's how secret the existence of this massive report had been. Even this "cold fusion ferret" did not see it until late 2000.

It transpires that the EPRI-NSF joint meeting Proceedings were finally printed years later, in 1993, which may qualify as a world record for delayed proceedings. EPRI (Electric Power Research Institute) is the private research arm of many electric utility companies within the U.S. and NSF is the National Science Foundation, a science funding agency within the Federal government. These Proceedings are an astonishing, secret account of a private but *publicly funded* meeting that bears on a topic of overarching importance in the cold fusion saga: *who knew what and when did they know it*. Consisting of formal scientific papers, plus transcriptions of scientific dialogue following each of the thirty presentations, the report is of much greater significance than I had imagined before reading it. The meeting occurred in Washington, less than two weeks before the DOE ERAB Cold Fusion Panel was to cast its final negative vote against cold fusion on Halloween, October 31, 1989. The public would not see the evidence for cold fusion laid out with such clarity until much later, until the First Annual Conference on Cold Fusion in Salt Lake City, in late March 1990. By then, of course, the chorus of cold fusion critics had long since succeeded in poisoning the public mind with press manipulation and ad hominem attacks against cold fusion researchers.

Prominently stamped in two places at the beginning of the EPRI-NSF Proceedings is the stern admonition: "Note: This document has been prepared for workshop attendees and is not intended for wide distribution." Mind-boggling! We have been lectured for years that secrecy in protecting commercial interests was one of the main problems with cold fusion research, and here some of the enemies of cold fusion are seen trying to classify it retrospectively! What were they trying to hide?

The "Editorial Perspective," which introduces the Proceedings, was written by one Thomas R. Schneider, a miscreant within EPRI who was known to be bent on disproving cold fusion to the world—more remarkable, because he is in

complete opposition to the thrust of the EPRI-sponsored work at SRI International and elsewhere, which confirmed the work of Fleischmann and Pons.² Schneider wrote, "This workshop, held October 16-18, 1989, was the first forum that succeeded in achieving a frank and open scientific discussion on the controversial findings reported by Pons and Fleischmann and the attempts to explain them." (It is telling that he left out Steve Jones' neutron work.) In a summary statement (presumably written in 1993) that bears extremely poor relation to the evidence and discussion in this compendious Proceedings or what evidence built up later, Schneider wrote, "Some of the measurements reported herein appear to be artifacts, unreproducible results, or mistakes. At this time, no clear evidence exists that 'excess heat' is a result of a nuclear

process." He ended with this disingenuous refrain: "My hope is that this Proceedings will help those interested in the history of this controversial subject better understand both the degree of scientific uncertainty and the chaotic state of knowledge that existed at the time this workshop was held."

Schneider, the so-called "Managing Editor," made sure that these proceedings were stamped "not intended for wide distribution," a mockery of his earlier phrase, "frank and open scientific discussion."

If a cold fusion colleague hadn't thought to violate the ban against dissemination, I would not have been able to prepare this account. Since learning of the publication, I have heard from a government official, who attended this meeting, that there had been threats of lawsuits if these proceedings were widely circulated. I do not know precisely who these unhappy parties might be, but they could not be those interested in the honest investigation of cold fusion or its history. This is the bottom line: The data and discussions included in this report lead to nothing less than proof that the then imminent DOE killing of cold fusion research was utterly wrong-minded. There was simply too much evidence presented—nuclear scale excess heat, tritium, and low-level neutron emissions—to leave any doubt that the matter deserved continued, intense investigation. With the passage of one more year the *public record* would leave virtually no doubt about the reality of the cold fusion phenomena. Those who took part in the official DOE rush-to-judgment later in October 1989, including at least one individual present at the EPRI-NSF meeting, Professor Alan Bard of the University of Texas, are guilty of egregious malfeasance in their civic and scientific responsibilities.

Curiously, though fifty attendees are listed in the report (sixty were invited), only one member of the DOE's cold fusion panel, Alan Bard, attended. Prof. Mark Wrighton of MIT's Chemistry Department (another DOE Cold Fusion panelist) was invited, but did not attend. MIT Plasma Fusion Center's (PFC) Dr. Richard Petrasso *did* attend, and presumably later filled in Wrighton with his negatively biased observations. Though the report transcribes the scientific exchanges that occurred after each presentation, not one public comment from Petrasso appears on the record. There was evidently no need to do so—

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the “fix” was in. Wrighton and Petrasso, it may be recalled, have their names on the infamous “Phase-II Calorimetry” report from the MIT PFC, which was touted as a null result but wasn’t.³ The Proceedings show that Bard offered the MIT PFC result and Caltech as two of five negatives for excess heat. Two other of the “negatives” for excess heat, University of British Columbia and Sandia Laboratory, have not claimed major significance against cold fusion since that time. And another, the experiment of Dr. Melvin Miles of the Navy, was cited as negative by Bard, which was then certifiably out-of-date information, as it turns out. Miles initially got negative results in his calorimetry, and subsequently brought to the attention of Bard *et al.* that he had, indeed, achieved excess heat runs. Miles’ results were also incorrectly cited as negative in the final DOE report.

Why Wade Through This Muck?

It is a pleasant winter morning here in New Hampshire and I really should be out in the elements enjoying life. Instead, I find myself digging through evidence from a grizzly crime scene of years past—the rush-to-judgment against cold fusion of 1989. There is more than enough evidence in this 600-plus page tome to condemn the perpetrators, but the American people, who paid for much of the work documented in the Proceedings (the specific contract numbers under which the reported research was carried out are given), are not allowed to read the evidence or even to obtain a copy of it—except illicitly. You will not be allowed, except by this summary, to see for yourself the true state of knowledge about cold fusion that existed just prior to its official killing by Huizenga *et al.* That makes the crime so much worse. It is infuriating.

There were many important players at this meeting from those who were then and subsequently on the “pro” side of the cold fusion debate—John Appleby, John Bockris, Scott Chubb, Martin Fleischmann, Robert Huggins, Yeong E. Kim, Mike McKubre, Howard Menlove, George Miley, Richard Oriani, Stan Pons, Ed Storms, Carol Talcott, the late Kevin Wolf, David Worledge, and the late Ernest Yeager, to name the more prominent ones. Well-known critics included of course Alan Bard, but also Nathan Lewis of Caltech, the silent Richard Petrasso, EPRI’s Thomas Schneider, and excess-heat opponent Steven E. Jones. Also attending the meeting were two science luminaries, who had not previously been in the public fray over cold fusion: nuclear weapons physicist Dr. Edward Teller and Dr. Paul Chu, from the University of Houston and the Texas Center for Superconductivity. Chu was a respected name in the then new field of high-temperature superconductivity, another solid state enigma which had fared much better than cold fusion due to its ease of reproducibility. The record shows that Teller and Chu were not convinced by the evidence, but they were struck by it—enough for one of them, Chu, to issue a joint press statement with Texas A&M’s John Appleby at the time: “Based on



Paul Chu
Courtesy University of Houston



Edward Teller
Courtesy Lawrence
Livermore National Laboratory

the information that we have, these effects cannot be explained as the results of artifacts, equipment, or human errors. However, the predictability and reproducibility of the occurrence of these effects, which are common for accepted established scientific facts, are still lacking. Given the potential significance of the problem, further research is definitely desirable to improve the reproducibility of the effects and to unravel the mystery of the observations.”⁴

Teller subsequently told the press that he recommended “in recognition of the high class work that yielded surprising results, that the efforts be supported to obtain clarification, whether the results are due to sophisticated difficulties in the experiments, or whether a new phenomenon is involved.”⁴ His transcribed words, now revealed by the Proceedings, were even stronger: “I am arguing for a continuation of an effort, primarily for the sake of pure science. And, of course, where there is pure science, sometimes, at an unknown point, applications may also follow.” (p.1-1). Weeks later, the negative DOE Cold Fusion Panel report would fly in the face of this sentiment.

In his introductory remarks at the beginning of the conference (p. 1-1 and 1-2) Teller further noted: “. . .the history of science and experimental physics is full of examples of predictions that things are impossible and yet they have happened. I remember what Ernest Lawrence once said about me: ‘When Teller says it is impossible, he is frequently wrong. When he says it can be done, he is always right.’ But what if we are presented with the fact that the results are correct? Then we will have to ask ourselves what are the minimum changes which we would need to make in

nuclear physics to explain the facts. If the giraffe exists, how does his heart pump blood into his brain? If the results are correct, then you must assume that nucleons can interact not just when they touch. We need to be able to explain how the nucleons interact at distances as great as 1/10 of an angstrom. . .This would be a scientific discovery of the first order, the kind for which we are willing to spend 5 x 10⁹ dollars (Superconducting SuperCollider). I therefore applaud the National Science Foundation and the Electric Power Research Institute for maintaining enough interest and enough support so that a real clarification of the apparent contradictions can be pursued.”

Later in the meeting, during a talk on “Catalytic Neutron Transfer,” in which he whimsically proposed an imaginary neutral mediating particle, the “Meshuganon,” to facilitate cold fusion phenomena, Teller said this about the importance of the overall effort (p. 23-2): “My conclusion is that the experimental work on ‘cold fusion’ should continue until a decision is obtained. The Meshuganon may give a possible but not a probable explanation. In any case, the experimental observations, if they are confirmed, must be explained by means that are outside of conventional nuclear physics.”

Taking a different tack on theory, Scott and Talbot Chubb of

the Naval Research Laboratory later in the session opined (p. 29-1): “‘Cold Fusion’ can be explained in terms of known quantum mechanical effects that become physically realizable in a well-defined limit within a solid. Furthermore, the extraordinary circumstances associated with the prolonged overcharging of Pd electrodes by Fleischmann and Pons suggests that in fact this well-defined limit may very well have been obtained during the experiments.” Both the Teller and Chubb lines of theorizing have persisted to this day.

Someday, an enterprising group may succeed in getting this full EPRI-NSF report sprung into the public arena, perhaps by a Freedom of Information Act (FOIA) request. Good luck! Though in many cases federally funded research is being reported, the Proceedings are marked, “Copyright 1993, Electric Power Research Institute. All rights reserved.” For now you’ll just have to be satisfied with some additional fair use extracts and observations, to wit:

- Critics Alan Bard and Nathan Lewis think that modern heat measuring calorimeters can only to be trusted at the 5% error level! Lewis (p. 2-9): “The measured excesses are actually very small (10-30% of total input power) in most instances, and in most cases, the observed heating power is less than the total input power. Only if no recombination is established, and accurate calibration is demonstrated, can these values be considered trustworthy. It would be far better to build a calorimeter in which the claimed 50 W of excess power yielded a result that was a factor of 2 to 3 higher than the calibration curve, not a mere 5 to 10% higher.” Error bounds on carefully done calorimetry are far better than this—an order of magnitude or more better. Later came this inane and preposterous statement from Bard, (p. 14-5): “Calorimetric data must always be shown graphically, including error bars. These errors are often much larger than we would normally expect. When we performed some calorimetric measurements in our laboratory with less care than usual, we obtained errors of $\pm 5\%$. In my view, this would not be an unusually large error in many calorimetric experiments.” So, Bard (or his students) performs a deliberately inept—“with less care than usual”—experiment and then states that this $\pm 5\%$ would not be an unusually large error! Since helping to kill off cold fusion with this kind of irresponsible stupidity, Bard has been observed traveling on the lecture circuit against cold fusion as “pathological science,” in speeches from Boston University to Illinois.

- The late Kevin Wolf of Texas A&M’s Cyclotron Institute and Chemistry Department clearly asserted that his tritium evolution experiments could not be due to contamination—quite a different story than what he would say in the public arena during the furor over tritium at Texas A&M provoked by science journalist Gary Taubes in June 1990.⁵ From Wolf’s paper (p. 8-1): “The sudden appearance of tritium activity in the cells requires the tritium to be loaded in a component prior to the beginning of cell operation in a contamination model. Release is assumed to be caused by deterioration of one of the materials used in the 0.1 M LiOD solution. In an extensive set of tests, no contamination has been found in the starting materials or in normal water blanks.”

- Perhaps one of the most remarkable papers at the meeting was by Debra Rolison and William E. O’Grady of the Naval Research Laboratory, “Mass/Charge Anomalies in Pd After

Electrochemical Loading with Deuterium.” It was a strong hint of things to come in the cold fusion field—the discovery of heavy element transmutations on and in CF cathodes. Their abstract, in part (p. 10-1): “Our approach has been to explore the surface character of Pd foils after extensive electrolysis of H₂O or D₂O solutions. Our experiments with the electrolysis of Pd foil in D₂O did not produce large neutron fluxes, and due to the small volume of the foil, calorimetric measurements were precluded. However, surface analyses by time-of-flight secondary [ion] mass spectrometry (TOF-SIMS) of the electrolyzed Pd revealed anomalies with electrolysis. TOF-SIMS was used to survey the effect on the Pd isotopic distribution with electrolysis in D₂O and H₂O. We report here an enrichment of m/z 106 and a diminution of m/z 105 in the surface/near surface layers of Pd electrolyzed in D₂O. No enrichment of m/z 106 was observed for the starting Pd material or for Pd electrolyzed in H₂O.” The researchers proved that the anomaly was a surface and not a bulk phenomenon. After this high quality research, the tritium results from Storms, Talcott, and Wolf, not to forget the various high quality nuclear-scale excess heat measurements reported at the EPRI-NSF meeting, alarm bells should have gone off in the minds of the critics, but this did not happen. The critics, bent on killing cold fusion, not exploring a scientific frontier, were allergic to the facts. Moreover, the meeting was not open to public scrutiny by science journalists and others. The critics did not have to answer to the facts; these remained hidden.

- The paper of C. Talcott, E. Storms, R. Jalbert, and M.A. David, all of Los Alamos National Laboratory, was most impressive (p. 13-1). Storms was emphatic about the group’s results right up front: “Let me begin this note with a conclusion: I see no way that the large (10^3 to 10^6) tritium values reported throughout the world could be in error. The smaller values (10^2) type range requires more scrutiny, but even those are unlikely to be in error.” Interesting and impressive are the credentials of Roland A. Jalbert, whose background, presented in a prominent slide by Storms, is seen to be deeply rooted in the tritium measurement establishment of the hot fusion program. “Roland A. Jalbert: twenty-five years working with tritium and tritium detection; involved in development, design, and implementation of tritium instrumentation for fifteen years; for twelve years he has had prime responsibility for the design, implementation, and maintenance of all tritium instrumentation at a major fusion technology development facility (Tritium Systems Test Assembly); Consultant on tritium instrumentation to other fusion energy facilities for ten years (Tokamak Fusion Test Reactor at Princeton).” Where were the minds and ethics of the hot fusioners of that time as they marginalized the tritium results performed, in part, by one of their own?

Bashing the Meeting

It is bad enough that the EPRI-NSF meeting was closed-door and that the Proceedings were delayed and then covered up. But the circumstances surrounding the meeting now acquire a more sinister aspect. This is what I wrote in 1991 in *Fire from Ice*¹ about the context of this meeting:

“Ironically, at the same time the DOE cold fusion panel was finalizing its harsh, negative report. It met on October 30 to produce the final dark score, but before that meeting, co-chairman Huizenga was already telling the press that he didn’t

expect any changes from the July interim report. He told Jacobsen-Wells of the *Deseret News* in referring to the EPRI-NSF meeting, 'That was a very minor group of people who had been getting positive results for some time.' Even within NSF there was dissent. Marcel Brandon, director of the NSF physics division sent an e-mail message to many NSF colleagues: 'It seems unfortunate that an NSF office [the engineering division] is now appearing to encourage such discredited work. . . ' By contrast, co-chairman Norman Ramsey of Harvard dearly wanted to see the results of the EPRI-NSF meeting. Paul Chu would have obliged, but he said it would be a few months before the report was due to come out. (Unfortunately, the EPRI-NSF report has still not been published.) In guiding the Panel to its conclusions, Huizenga had shown the members purported evidence that Fleischmann and Pons had broken their agreements with BYU. (Question: What did that have to do with science?) One panel member, H. Guyford Stever, characterized Fleischmann and Pons's behavior as 'bad science.' Within a year, Stever would be appointed the head of a federal panel charged with plotting the future course of hot fusion. . .

"The negative ERAB report undoubtedly provided a convenient justification within the DOE bureaucracy for reducing federal funding for cold fusion research. Two million dollars were summarily removed from fiscal year 1990 funding of the Division of Advanced Energy Projects led by Dr. Gajewski. This, in effect, put a damper on cold fusion research, but did it in a way that was not directly traceable to the report. At the EPRI-NSF meeting, Gajewski had made it known that he would support cold fusion projects. But now the 'modest support for carefully focused and cooperative experiments within the present funding system' favored by the ERAB report went up in a cloud of bureaucratic smoke fueled by the negativists. . .

"Though the DOE panel had let cold fusion down, positive results from the DOE's own laboratories, Oak Ridge and Los Alamos, were beginning to surface. Los Alamos was strong on anomalous tritium and neutrons. ORNL had gotten tritium bursts, excess heat, and low level neutrons. Yet without winking, Huizenga claimed that the DOE panel had considered ORNL and LANL findings. How so? Pons, for one, reacted angrily to the DOE report in the *Deseret News*: 'They have made their judgment; they have passed sentence on fusion and they've been proven wrong. They were wrong from day one. The DOE appointed a bunch of negative people to give a negative decision. They will continue to be proven wrong—even by their own laboratories.'"

Now that I see the level of detail on the above-mentioned positive findings that were reported by ORNL and LANL in the EPRI-NSF meeting, Pons' words ring ever more true. This entire affair at DOE was a pre-ordained rush-to-judgment by a bunch of closed-minded bureaucratic scientists. None of them have had the integrity or decency in the intervening years to change their inept, wrong, and now it can be seen so very clearly—*immoral*—conclusions.

References:

1. Mallove, E. 1991. *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, John Wiley & Sons.
2. SRI International and EPRI. 1994. "Development of Advanced Concepts for Nuclear Processes in Deuterated Metals," McKubre,

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3. Mallove, E. 1999. "MIT and Cold Fusion: A Special Report," *Infinite Energy*, 4, 24, 66-119.

4. Mallove, E. *Fire from Ice*, p. 182.

5. Mallove, E. 2000. "The Triumph of Alchemy: John Bockris and The Transmutation Crisis at Texas A&M," *Infinite Energy*, 5, 32, 9-24.

