

If skeptics wish to make a contribution, they should explore rational explanations; otherwise, they should just remain quiet while letting the rest of us find ways to explain the effect.

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#### REFERENCES

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### RESPONSE TO "FACTS BEING DISTORTED IN COLD FUSION CONTROVERSY"

Storms falls prey to exactly the same fault he finds in others; i.e., he defines anyone who is skeptical of "cold fusion" claims as someone who distorts the facts, is dishonest, or is careless with logic. We did not derive our conclusion that "failure to rule out prosaic explanations probably invalidates all currently available reports of excess heat in both light water Ni/Pt and heavy water Pd/Pt cells" from a "single experience" as Storms asserts, but rather from a careful review of all of the literature available at the time we wrote the article. Our conclusion was and is consistent with the results of several experiments reported in our paper. We did not propose that "reports of excess heat result from using 'bad' calorimeters and negative results . . . from using 'good' calorimeters." Rather, we pointed out that "bad" calorimetry (which can be done with a "good" calorimeter) definitely accounted for some of the claims of excess heat. It is also certainly true that most claims of excess heat, including those of Pons and Fleischmann, come from studies using calorimeters of unproven design and with minimal calibration and verification. The measurement of heat, i.e., calorimetry, can be subject to many errors and is not something that should be assumed to give correct answers without careful and thorough verification of the results. Storms does not give any references to the "10 studies using closed cells and 9 studies showing no recombination to which the prosaic explanation does not apply," so we cannot respond. Indeed, which "prosaic" explanation does he refer to, recombination or bad calorimetry? No references are given to support his assertion of work that used "calorimetric studies as good as theirs."

One of the requirements for a phenomenon to be accepted as scientifically valid is that it be reproducible. Storms argues that we cannot criticize a result just because it is not reproducible. To what experiments does Storms refer in which "so much power has been produced on several occasions to completely overwhelm any 'prosaic' explanation?" We devoted a paper to the claims of Miles et al. showing that neither excess heat nor  $^4\text{He}$  production had been established by their work.<sup>1</sup> Storms

fails to mention this paper although it was published together with his reference.<sup>2</sup> Such omissions are definitely "not in the spirit of proper scientific debate."

The challenge for Storms is to prove that cold fusion does exist. In our opinion, work done to date does not provide compelling evidence for cold fusion. We suggest that Storms study the history of genetics research in Russia during the period of Lysenko if he really wants to know what happens to science when the skeptics follow his dangerous request to "just remain quiet."

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#### REFERENCES

1. S. E. JONES and L. D. HANSEN, "Examination of Claims of Miles et al. in Pons-Fleischmann Type Cold-Fusion Experiments," *J. Phys. Chem.*, **99**, 6999 (1995).
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### RESPONSE TO "FACTS BEING DISTORTED IN COLD FUSION CONTROVERSY"

When I was asked to write this summary of "cold fusion," I knew I would receive enormous flak because both sides of this controversial subject have turned to bitter rhetoric when discussing the scientific capability of anyone who disagrees with them. I thought that my book would especially draw the ire of the "anti-cold-fusion" establishment because I maintained that the workers in cold fusion were competent indeed and doing interesting, scientific work. Much to my surprise, almost all the bitter attacks came from the "pro-cold-fusion" faction.

One point in particular has been raised again and again. We found that the surface of palladium cathodes often showed a surface, some hundreds of angstroms thick, highly enriched in mass 106. Auger analyses showed this layer to be  $\text{ZrO}^+$  producing (90 + 16) and not palladium isotope 106. The question then arose, "Why such widespread contamination by zirconium in the pH 13 LiOD electrolyte?" The tritium/deuterium (T/D) ratios of solutions that deposited out zirconium were higher than literature values for heavy water. Because heavy water reactors often use zirconium cladding for fuel rods (and all cladding have measurable fuel contamination on the outer surface of the cladding), a hypothesis was put forward by one character in the dialogue that the high T/D ratio, the presence of zirconium, and a possible slight increase of suspended particles with alpha-emitting surfaces could all be explained by "spiking" of natural heavy water with slight amounts of used moderator water from a heavy water reactor. The second character in the dialogue asks

if there is any proof that such spiking has occurred, and the first character answers, "No, we have no proof that used moderator heavy water is actually present in commercial heavy water, just circumstantial evidence such as the ultra-high tritium-to-deuterium isotope ratios. . . ."

Why this hypothesis is so troublesome to Storms and others is puzzling to me. The possible confusion of beta from tritium versus beta from  $^{210}\text{Pb}$  is associated with radon decomposition, not heavy water spiking. Storms's wife, C. Talcott Storms, is quoted in my book on her checklist to make sure that tritium and  $^{210}\text{Pb}$  are not confused. I think Storms misinterpreted my concern about making sure that a beta emission is from tritium and not  $^{210}\text{Pb}$ . This is a general concern, not linked to zirconium and high T/D ratios. Storms criticizes my comment that Russia is now selling palladium from reactor fission products. He says he inquired as to whether the palladium producers could include Russian palladium, and they answered, he says, "this possibility does not exist." When I questioned the palladium producers if their palladium could include Russian palladium, they told me that they buy palladium from many sources and could not guarantee "no Russian palladium." The problem with fission fragment palladium is not the tritium, as somehow Storms has interpreted my concern, but possible radioactive species within the palladium. On the other hand, palladium that has been contaminated with tritium during a previous use can be a problem because it can dissolve in molecular form within the electrolyte as well as escaping as TH or TD evolved gas.

Because I anticipated blasts from the pro- and anti-cold-fusion scientists in equal numbers but received the "excess heat" on my book only from the "pro" crowd, I wondered if I had been as evenhanded and objective as I thought I had been. Luckily, I received a very supportive "your book is objective" comment from an ardent pro-cold-fusion scientist, Professor Bockris. Perhaps to be sure that I was not "intellectually dishonest and should be censured by any competent scientist" (as my friend Storms describes me), I need to receive a blast from the anti-cold-fusion establishment!

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## COMMENTS ON "CALORIMETRY, EXCESS HEAT, AND FARADAY EFFICIENCY IN Ni-H<sub>2</sub>O ELECTROLYTIC CELLS"

Concerning Ref. 1, which follows our paper<sup>2</sup> in *Fusion Technology*, Vol. 28, No. 4, we have several independent reports from excellent researchers documenting thermal energy release from our light water electrolytic cells; consequently, we are somewhat irritated by the Bose report in the confusion that it causes because our referenced experimental protocol was clearly not followed. Why?

The Bose scientists report that they did not produce energy in their light water electrolytic cells. They reference Mills' first paper<sup>3</sup> on electrolytic cell studies but surprisingly do not reference the second paper<sup>4</sup> even though Ref. 4 was published 5 months before the indicated submission date of Ref. 1.

Unfortunately, the Bose researchers did not follow either the published protocols from Refs. 3 and 4 or Ref. 5, which they also cite—all of which describe how to ensure the proper nickel surface necessary for the electrolytic cells to produce the catalyzed transitions to fractional-state hydrogen atoms and the consequent thermal energy release from those transitions. Everyone knows how important surface preparation is in catalytic chemistry. Shkedi et al. were doomed from the start. If they had contacted us, as they did others (see Refs. 12 and 14 on p. 1730 of Ref. 1), we would have faxed them the proper protocol.

Reference 4 specifically states (p. 109, right column, first full paragraph), "As usual in electrochemistry, measures were taken to avoid impurities in the system, especially organic substances," and further states later in the paragraph, "The nickel cathode was removed from the container with rubber gloves, and cut and folded in such a way that no organic substances were transferred to the nickel surface."

The Bose scientists may indeed have acquired some information from Ref. 4 because they used (Ref. 1, p. 1722, left column, third full paragraph) "... cold drawn nickel wire. . . ." See Ref. 4, p. 109, left column, last full paragraph. But, instead of following our protocol, they prepared the nickel electrode surface by cleaning it with not one, but two different organic solvents, and then proceeded to anneal the electrodes at 1100°C. Such a treatment would have left trace organic residue on the cathode and changed the "cold drawn" surface to an annealed surface. For these two reasons, we would have predicted the null event.

Having assembled a nonworking (nonenergy-producing) electrolytic cell, it is no surprise that the Bose scientists also failed to detect hydrino atoms: Hydrino atoms must be made before they can be detected. I would suggest a reading of Ref. 2. Fractional quantum energy level  $n = \frac{1}{2}$  hydrogen atoms are clearly demonstrated in Figs. 4 and 6 through 9. We have studied several hundred spectra (and samples) since the data reported, and I believe that the *direct evidence* of their existence speaks for itself.

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3. R. L. MILLS and S. P. KNEIZYS, "Excess Heat Production by the Electrolysis of an Aqueous Potassium Carbonate Electrolyte and the Implications for Cold Fusion," *Fusion Technol.*, **20**, 65 (1991).