

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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2. J. E. JONES, L. D. HANSEN, S. E. JONES, D. S. SHELTON, and J. M. THORNE, "Faradaic Efficiencies Less Than 100% During Electrolysis of Water Can Account for Reports of Excess Heat in 'Cold Fusion' Cells," *J. Phys. Chem.*, **99**, 6973 (1995).

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 ^4He production had been established by their work.¹ Storms

fails to mention this paper although it was published together with his reference.² 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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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 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