#1

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Dear Steve:

This will acknowledge, with thanks, the receipt of your comments on the proposal entitled, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium."

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

Ryszard Gajewski, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

REVIEWER #1

REVIEW OF PROPOSAL: "The Behavior of Electrochemically Compressed Hydrogen and Deuterium", by S. Pons and M. Fleischmann

COMMENTS ON THE PROPOSAL

1) Statements such as "the resulting calculated pressure is on the order of the measured rise in chemical potential, approximately 10^{27} atmospheres" (page 2) demand support: where are the calculations? In general, theoretical calculations are strikingly absent in the proposal.

2) The authors tantalizingly claim an "increase in the background radiation count in the lab" (page 6) during an experiment, suggesting the occurrence of nuclear fusion. What kind of radiation was observed? How was the radiation detected? Was the radiation consistent in type and energy with p-d or d-d fusion? These points should appropriately be addressed to permit evaluation of the merits of the proposal.

3) The proposed work includes "radiation measurements" (page 10). Unfortunately, the method of making these measurements is not discussed although it is central to the investigation, since detecting neutrons and/or gamma radiation of the proper energy would be a clean signature for fusion reactions.

4) If significant radiation is anticipated in the research, safety measures must certainly be elaborated.

5) If a paucity of theoretical justification and information on radiation is a weakness in the proposal, certainly the electrochemical/calorimetric approach is amply defined and explained. The researchers appear to be well-qualified in this area.

6) "We believe that the results we have obtained so far are a strong indication of a progressive increase in the fusion of D nuclei in the Pd-lattice with increasing chemical potential (= compression). While there are alternative explanations of the excess heating effects, their possibility does not seem to be very likely." (p. 6) Please, what are the other explanations and why are they unlikely?

7) "The experiments will take longer than our previous experiments in view of the greater thickness of the rods compared to the sheet electrodes. It will take approximately 12 months to charge a 2cm diameter rod to saturation with deuterium." (p. 7) Could not the time required be drastically reduced by heating the rod in a pressurized deuterium environment?

8) Since no references are cited, one wonders if a thorough

literature has been done. In particular, publications by C. Van Siclen and S. E. Jones (J. Phys. G, 12 (1986) 213-221) and by B. A. Mamyrin and I. N. Tolstikhin (Developments in Geochemistry 3: Helium Isotopes in Nature, New York: Elsevier, 1984) could be relevant.

In conclusion, I find the proposed research to be very intriguing and consistent with the direction of the Advanced Energy Projects Division. The personnel are evidently well-qualified and competent in electrochemical techniques. However, the proposal has a number of weak areas as delineated above that should perhaps be addressed.

Statement regarding my review of the proposal: "The Behavior of Electrochemically Compressed Hydrogen and Deuterium," by S. Pons and M. Fleischmann

I have made every effort to be objective and thorough in reviewing the proposal described above. I must make it clear, however, that I have been doing research in the subject area, which I call piezonuclear fusion, since 1985. Our research group at Brigham Young University is using neutron and gamma radiation detection techniques, along with measurements of helium-3 / helium-4 ratios (which will be performed on our samples by Alfred Nier of the University of Minnesota). We load hydrogen and deuterium into metal strips using electrochemical means as well as by heating the metal in a pressurized hydrogen-deuterium environment. We began experimental research in this area in Spring 1986 as an offshoot of our cold nuclear fusion research program supported by the Advanced Energy Projects Division of the Department of Energy. The work was discussed with Dr. Gajewski in this time period and was formally reported in our 1985-1986 Annual Report to the DOE (see attachments).

The roots of our work in this area may be traced to my efforts in 1985 to enhance fusion in isotopic hydrogen molecules without the use of short-lived muons. Early work on this was published in the paper: "Piezonuclear fusion in isotopic hydrogen molecules," by Clinton Van Siclen (who performed the detailed calculations) and myself in **Journal of Physics G: Nuclear Physics, 12: 213** (1986, paper received 12 June 1985). In addition to initiating the study, I coined the term "piezonuclear fusion" in analogy to the term "thermonuclear fusion", to indicate that our approach is to induce fusion by "squeezing" the hydrogen nuclei together rather than by heating them to very high temperatures. (The idea is to reduce the width of the Coulomb potential barrier and thereby to enhance barrier penetration leading to nuclear fusion.) It later occurred to Prof. Palmer and myself in discussions at BYU in March 1986 that this end might be achievable by loading hydrogen isotopes into minerals (in particular into metals), leading to the current study. We were totally unaware of any work on this concept by Dr. Pons, Dr. Fleischmann or indeed of **anyone else** at this time. (Prof. Johann Rafelski had suggested the possibility of slow fusion in gaseous HD molecules in December 1985, but the Van Siclen/Jones paper indicated that this would be exceedingly slow. Prof. Rafelski became very intrigued by our idea of piezonuclear fusion of hydrogen isotopes in metal lattices when we told him about it; he is trying to establish a theoretical basis for calculating rates for this effect.) In doing a literature search, we subsequently found that B. A. Mamyrin, L.V. Khabarin and V. S. Yudenich had mentioned the possibility of hydrogen fusion occuring in metal foils in their paper "Anomalously High Isotope Ratio $^{3}He/^{4}He$ in Technical-Grade Metals and Semiconductors," **Dokl. Akad. Nauk. SSSR, 237:1054** (1978), but they had no proof that fusion was occurring. We have found no further publications by these scientists on this subject, except for a reference to this short paper in Mamyrin's book "Helium Isotopes in Nature,"

New York: Elsevier, 1984.

I feel that Pons' proposed work nicely complements the ongoing cold fusion research previously initiated by us with the support of the Advanced Energy Projects Division of the Department of Energy.

Steven E. Jones

9/30/88