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," Doki. 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

November 18, 1988

Professor Steven E. Jones
Department of Physics and
Astronomy
Brigham Young University
Provo, Utah 84602

Dear Steve:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #1.

It will help us in deciding whether or not to support the proposal if you could provide us with your comments on the rebuttal. Do you believe, based on the totality of the arguments offered in the proposal and in the rebuttal, the proposed project should be supported?

Your response, by return mail if possible, will be greatly appreciated.

Sincerely,

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

Enclosures

Reply to Reviewer #1:

We will reply to the reviewer using the numbering of his paragraphs.

(1) The statement on page 2 of our proposal was merely intended to illustrate that IF the expression (particle density x temperature x volume x lifetime) applies to our system and if the chemical potential of the dissolved D in the lattice is converted into an equivalent pressure, then it is not unreasonable to expect significant fusion processes to take place.

The reviewer should note that the processes at the surface of the Pd electrode are

$$D_2O + e = D_{ads} + OD^- \qquad (A)$$

$$D_{ads} + D_2O + e \rightarrow D_2 + OD\text{-} \text{ (slow)} \qquad (B)$$

Because of the slowness of reaction step (B) the chemical potential of the adsorbed D is raised by the electrode potential difference at the interface and,as the adsorbed D is in equilibrium with D in the lattice

$$D_{ads} = Pd/D \qquad (C)$$

the chemical potential of the dissolved D is in turn raised to the value corresponding to the applied overpotential. If one wanted to raise the chemical potential by increasing the pressure of D_2 (and if step (B) could be made to go to equilibrium) then one would require a pressure given by

$$RT/2F \ln [P_{D2}] = 0.8 \text{ Volt}$$

i.e., about 10^{27} atmospheres. Such a pressure clearly cannot be achieved on earth but it is a simple matter to raise the chemical potential of D in the lattice by applying an appropriate potential to the electrode. This is the substance of our proposal.

(2) An increase of (beta + gamma) radiation was detected in the vicinity of the experiment. The measurement was made with a Mini-Monitor 442 sandwich GM-scintillation type counter. The background count in the laboratory, and in adjacent laboratories measured with this meter had remained at 175 counts per minute prior to the last day of experimentation when the rate rose to 256 counts per minute near the Dewar. The rate at remote parts of the lab and in the adjacent labs remained normal. To our knowledge, no radioactive materials had been brought into the lab. This increase must presumably be attributed to the reactions of thermal neutrons with components of the Dewar. This is a complication which we would clearly have wanted to avoid! Please also see reply to (3).

(3) The main methods to be used will be as follows: (a) detection of any tritium generated by the reactions and correlation of the rate of generation of tritium with the excess energy production. Samples will be withdrawn and analyzed using scintillation counting equipment. (b) Detection of thermal neutrons and use of energy discriminative gamma-ray analysis. The reviewer should note that under the conditions of our experiment neutrons will be rapidly thermalized in the palladium rod (indeed the experiment was designed with this in view for safety reasons) so that it is not possible to correlate the energy of any neutrons produced with any particular nuclear reaction. Our strategy therefore will be to detect thermalized neutrons and in particular the gamma radiation generated by the reaction of these neutrons with species present in the Dewar (the electrodes, electrolyte and components of the borosilicate glass).

To be more specific, we will initially use the simplest possible means to search for thermalized neutrons. For example, we may compare results for potassium deuteroxide electrolyte with those for potassium borate using photographic plates as a detection medium. Gamma rays will be detected using sodium iodide crystals for low resolution measurements; if necessary we will use intrinsic germanium detectors.

(4) The reviewer should note that this is why we terminated our experiments. If this project is funded, then one of our first objectives will be the quantification of any radiation produced and all appropriate steps to contain and shield the experiment will be taken. The Department is well equipped with radiation-safe laboratories and various forms of radiation counting equipment. Samples will be monitored daily with scintillation counters, and the apparatus with Geiger-Müller counters. In the case of obvious generation of radiation, we plan to reassemble the experiment in laboratories containing equipment suitable for discriminating the energies of gamma rays and equipment for detection of thermalized neutrons (see also reply to (3) above). We are thoroughly familiar with the rules and regulations of our University Radiation Safety committee, and have discussed with them their requirements for radiation experiments in our laboratories. The reviewer will wish to know that we have informed the Vice President for Research at this University (a well-known physicist) of our plans.

The reviewer will wish to note that if we are correct in assigning the excess energy to a fusion process, then the source would be classified as one of low energy. We intend to keep the experiments in this category. Thus if we get a marked increase in the excess energy with change of the system parameters (overpotential), bath temperature, rod dimensions, poisoning conditions) then we will scale down the experiment appropriately (thinner and shorter rods).

See also last paragraph of our reply to question (7).

(6) The main alternative explanations for excess enthalpy generation are:

(i) generation of D_2 at voids in the lattice (see also comments by reviewer #5). However, if this explanation applies, the excess energy generated during 331 hours of polarization at the highest current density would have required formation of D_2 bubbles at a higher rate than that corresponding to the applied current, i.e., there would have been a loss of dissolved D. Such a loss is inconsistent with the observation of the generation of a constant excess enthalpy during three successive periods of 75, 155, and 101 hours.

Moreover, at least 0.5 cm^3 of bubbles at 2000 atmospheres (the tensile strength of Pd) would have been formed which would almost certainly have disintegrated our sample of Pd. The structural integrity of the sample was preserved and, indeed, it is well known that electrochemical equivalents of Pd diffusion tubes can be used indefinitely. The easiest way to discount this possibility of bubble formation is to increase the experiment times. However, we do have it in mind to search for any D_2 or, more likely, He bubbles.

(ii) Participation of the reduction of O_2 and/or ionization of D_2 i.e. a shift off the Joule heating term towards the upper bound. However, our experiments showed that the Joule heating exactly balanced the Newton's law cooling at low current densities (where the effects of any O_2 reduction on D_2 ionization should have been at a maximum) while the excess enthalpy increased with the current density. Such behavior (as well as the other points we have set out in the application) is not consistent with the participation of O_2 reduction/D_2 ionization.

The reviewer may also like to know that in an earlier series of experiments periodic catalytic contamination of the Pd surface led to loss of dissolved D which was associated with cooling not heating presumably because of the cessation of the fusion process.

(7) We have considered doing this but unfortunately it would not reduce the experiment time. The important point is that the high chemical potential of dissolved D is established by diffusion so that one cannot "beat" the diffusional relaxation time.

We have also considered an electrochemical variant of the reviewer's suggestion, namely, the electrochemical saturation of Pd by polarization at a high temperature and subsequent cooling. As the dissolution of D in Pd is endothermic, this would produce even higher chemical potentials of the dissolved D! We do not wish to do this in our initial experimental experiments as the expulsion of excess D from the lattice on subsequent cooling would lead to spurious excess enthalpy generation (but see our comment above). The reviewer may wish to note that if we can prove that the concept works, then we intend to saturate rods at high temperature and to try to find suitable diffusion barriers. This would in effect produce Pd-D "hot rods".

The considerations set out in the above paragraph are also important to the safety of this project which has been referred to by some of the other reviewers.

As the dissolution of deuterium is endothermic, a marked rise in temperature of the rods will lower the chemical potential of the deuterium and will therefore self limit any fusion process.

(8) We have not yet read these references, but have ordered them and will do so as soon as possible. We would welcome any other useful references the reviewer may be able to supply. We have read similar documents and have not found information pertinent to this work.

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Further Comments on the Proposal: "The Behavior of Electrochemically Compressed Hydrogen and Deuterium"

Frankly, I was disappointed by the response to my original comments on this proposal. The contention that neutrons from fusion will be "rapidly thermalized" and that an "increase of (beta + gamma) radiation ... must presumably be attributed to the reactions of thermal neutrons with components of the Dewar" indicates, I fear, a lack of understanding of the penetrating power of 2.5 MeV neutrons, and of nuclear reactions in general. For example, energetic neutrons are much more penetrating than beta particles of comparable energy, and fusion neutrons are not difficult to detect. (There are numerous papers on this subject in papers on muon-catalyzed fusion, for instance.) And why are not gammas from proton-deuteron fusion considered? Furthermore, a background rate of 175 counts per minute in a small scintillation counter points to a dearth of shielding and a rather cavalier attitude toward detecting radiation associated with nuclear fusion. I also feel strongly that jumping from current results to experiments involving large and expensive palladium rods, requiring "about one year to charge" with deuterium, would be premature. First, smaller scale experiments of an exploratory nature are clearly needed to establish the phenomenon of fusion in metals.

However, in spite of these glaring defects, I do not recommend that all support for this project be denied. I find that the proposers have demonstrated expertise with electrochemistry and calorimetric methods. Although the proposed experiments clearly fail to demonstrate the existence of fusion processes in metals, there indeed exists some evidence that such does occur.

I think the proposers should be informed that exploratory research on fusion in metals (and other compounds) has been pursued under the auspices of the Advanced Energy Projects Division since 1985. (See our annual report dated May , 1986.) Our initial interest in the possibility of fusion in minerals stemmed from our related work on muon-catalyzed fusion in which fusion is induced as isotopic hydrogen nuclei are held closely together by a negative muon, and the correlation of this research with observations of anomalously large heat and helium-3/helium-4 ratios associated with earth's geology. We realized both could be explained by the occurrence of proton-deuteron and/or deuteron-deuteron fusion in the earth. (In particular, water is entrained in minerals in subducting zones, where excess helium-3 relative to helium-4 is common. Internal Brigham Young University reports by Profs. S.E. Jones and E.P. Palmer dated March-April 1986 discuss our early thoughts on this process. We now call the alleged process "piezonuclear fusion" in contradistinction to thermonuclear fusion, or "metal-catalyzed fusion" by analogy to muon-catalyzed fusion.) In discussing our idea with geochemists (H. Craig and A. Nier), we learned that they had seen inexplicable excess helium- 3/helium-4 ratios in a number of minerals–they were considerably intrigued by our possible explanation, which they had never before heard of. Finally, we uncovered a paper by Mamyrin, Khabarin and Yudenich which formally reports the occurrence of high helium- 3/helium-4 in metals and semiconductors (Sov. Phys. Dokl. 23:581 (1978). Since then, our research has accelerated. We have looked for p-d and d-d fusion in a number of compounds, including palladium foils, under various conditions since Spring 1986. Our methods involve both neutron and gamma detectors, followed by measuring helium-3/helium-4 ratios. It would not be appropriate to discuss our results here. However, there is enough evidence to warrant further studies, in my view.

The subject proposal approaches the measurement with calorimetric methods, which complements our methods outlined above. I think there is room for the proposed work in addition to the ongoing effort and would encourage funding. Indeed, I recommend a joint effort, with cooperation between the presently-funded project and the complementary work now being proposed. Such a joint effort would be facilitated by the close proximity of two of the universities involved (Brigham Young and Utah).