November 29, 1988

Dr. Theodore Beck
Electrochemical Technology
Corporation
1601 Dexter Avenue, North
Seattle, WA 98109

Dear Dr. Beck:

This will acknowledge, with thanks, the receipt of your comments on Professor Pons' rebuttal 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

**Department of Energy**
Washington, DC 20545

November 18, 1988

Dr. Theodore Beck
Electrochemical Technology
Corporation
1601 Dexter Avenue, North
Seattle, WA 98109

Dear ~~Dr. Beck~~ Ted:

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 #5.

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

11-22-88

Dear Ryszard:

A response to the Pons/Fleischman response is enclosed.

Ted Beck

Response to Pons/Fleischmann Response

I am not satisfied with the proposer's qualitative responses to my questions, but it appears that the contract research is required to answer the questions quantitatively. I am inclined to believe that the process is so potentially important, if it indeed works, that the project should be funded.

Some quantitative estimations of time constants for buildup of a runaway thermonuclear reaction and for the proposed self-limiting decrease in chemical potential of dissolved D and estimations of steady-state conditions would appear to be in order before serious experiments are begun. "Hand-waving" arguments were used in the proposer's response.

Review of the proposal, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium" by Stanley Pons.

The concept is, to this reviewers knowledge, new, and it is most intriguing. If the project were successful, it would constitute one of the most important inventions of the 20th century. The investigators should be encouraged to pursue it.

The project appears to be an extreme limiting case of the high-payoff, high-risk type that AEP funds. The payoff approaches infinity and the probability of success unknown and could be small. The product, $0 < (\text{payoff})(\text{success probability}) < \infty$, is quite indeterminate at this point in time.

On the other hand, this reviewer has serious questions about the reported experiment with $D_2O$ and the process itself.

1. Agreed that 0.8 eV could theoretically produce $10^{27}$ atmospheres equivalent for $D_2$, but what if the reaction, $2(D^+ + e^-) \rightarrow D_2$ nucleates at imperfections like grain boundaries. Since the tensile strength of Pd is only 2000 atm., the material could blow apart mechanically. $Pd_2D$ supersaturated with D probably has a lower tensile strength.

2. Agreed on the method of the thermal balance but not convinced that there are not valid alternative explanations for the excess heating effect. The investigators case would be stronger if they repeated the experiment in $H_2O$ and found no excess heating effect.

3. The alledged increase in radiation count in the lab should be elaborated. Where measured? Is it definitive? Is it attributed to tritium from Reaction 1 at the top of page 2? A more quantitative treatment and correlation with excess heating effect would be in order.

4. Is it possible to get a runaway thermonuclear reaction? A 2 cm diameter, 10 cm long Pd rod converted to $Pd_2D$ could produce an order-of-magnitude 0.1 kiloton explosion by Reaction 1 if detonated. The investigators are proposing to tread in an unknown region. To quote them, "In our view, calculations (such as nuclear force: quantum: molecular dynamic simulations) would be difficult and ambiguous (indeed perhaps impossible at this stage). In these circumstances it is best to resort to experiment." It would be a shame to lose Pons and Fleischmann as well as the University of Utah campus.

Reply to reviewer #5

We will reply to the reviewer according to his numbering system.

(l) and (2) (in part). These points are covered by our reply to question #6 of Reviewer #l. As this reply is lengthy, we attach a copy to these comments.

(2) (in part) We fully intend to make the comparison with saturation using $H^+$. However, the experiment is not unambiguous. One of the more intriguing possibilities is that one might be able to induce an hydrogen cycle: we have pointed out in the application that the cross-sections under the conditions we have in mind may be quite different to those in H-bombs.

The reviewer may be interested to know that we have repeatedly discussed amongst ourselves questions such as: is a part of the heat generation in the planets (especially the earth and Jupiter) due to H cycles in the Ni core? Are supernovas caused by related effects?

(3) The increase in the radiation was measured adjacent to the Dewar. It may have been due to tritium but could also have been due to the reaction of thermalized neutrons with components of the Dewar other than the $D_2O$. The reason we cannot be more specific at this stage is that we considered that we had to terminate the experiments. If the project is funded, then one of our first objectives is to quantify such effects (if any!) so that appropriate safety measures can be taken. We intend to correlate any heat release with tritium production and to look for thermalized neutrons and gamma-ray emission. Our replies to questions (3) and (4) of reviewer #1 are relevant to this point and are attached to this reply.

(4) Our own calculations showed that in the experiment we were conducting we might achieve a 0.042 kiloton explosion. Rescaling to the largest Pd rod we have in mind gives the figure quoted by the reviewer. This is why we approached the project with great care. There is, however, an intriguing aspect to the possible fusion of D in the Pd-lattice namely that it would be self-limiting. As the dissolution is endothermic, an increase in temperature would lower the chemical potential of the dissolved D and thereby limit the reaction. The effect would probably outweigh any acceleration of the reaction with increase of temperature. We might have the unusual situation that the heat release in any practicable device would increase with heat demand.

Our reply to #7 of reviewer #1 is also pertinent to this question and is attached.

Question (3) of Reviewer #1:

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.

Our reply:

(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.

Question (4) of Reviewer #1:

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

Our reply:

(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).

Question (6) of Reviewer #1:

"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?

Our reply:

(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.

Question (7) of Reviewer #1:

"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?

Our reply:

(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.