Department of Energy
Office of Scientific and Technical Information
Post Office Box 62
Oak Ridge, Tennessee 37831

August 10, 2016

Re: OSTI-2016-01064-F

Dear Mr. Ravnitzky:
This is in final response to the request for information you sent to the Department of Energy (DOE), Office of Scientific and Technical Information (OSTI) under the Freedom of Information Act (FOIA), 5 U.S.C. 552 on June 22, 2016.

You requested a "copy of records, electronic, or otherwise, of each letter TO and FROM universities, companies, and organizations, from the OSTI 'cold fusion' documents collection." On July 11, 2016, you were emailed an interim response letter informing you of the need for OSTI to obtain release authorization from the Department of Energy. OSTI received notification to release the letters to you in their entirety on August 8, 2016. As a result, OSTI is releasing 72 cold fusion letters in this mailing on a CD-ROM because of the volume and file size of the PDFs.

In addition, there are approximately 13 letters that are currently being reviewed by the DOE's General Counsel Office (GC) for release or redaction. Upon receipt of guidance from GC, OSTI will release in whole or in part.

This decision, as well as the adequacy of the search, may be appealed within **90** calendar days from your receipt of this letter pursuant to 10 C.F.R. § 1004.8. Appeals should be addressed to Director, Office of Hearings and Appeals, HG-1, L'Enfant Plaza, U.S. Department of Energy, 1000 Independence Avenue, S.W., Washington, D.C. 20585-1615. The written appeal, including the envelope, must clearly indicate that a FOIA appeal is being made. You may also submit your appeal to OHA.filings@hq.doe.gov, including the phrase "Freedom of Information Appeal" in the subject line. The appeal must contain all of the elements required by 10 C.F.R. § 1004.8, including a copy of the determination letter. Thereafter, judicial review will be available to you in the Federal District Court either: 1) in the district where you reside; 2) where you have your principal place of business; 3) where DOE's records are situated; or 4) in the District of Columbia.

You may contact OSTI's FOIA Public Liaison, Charlene Luther, Office of Preservation and Technology at 865.576.1138 or by mail at the Department of Energy, Office of Scientific and Technical Information, 1 Science.gov Way, Oak Ridge, TN 37830 for any further assistance and to discuss any aspect of your request. Additionally, you may contact the Office of Government Information Services (OGIS) at the National Archives and Records Administration to inquire about the FOIA mediation services they offer.

The contact information for OGIS is as follows: Office of Government Information Services, National Archives and Records Administration, 8601 Adelphi Road-OGIS, College Park, Maryland 20740-6001, e-mail at ogis@nara.gov; telephone at 202-741-5770; toll free at 1-877-684-6448; or facsimile at 202-741-5769.

If you have any questions about the processing of the request or about this letter, please contact Madelyn M. Wilson at

Sincerely,

Madelyn M. Wilson
FOIA Officer
DOE OSTI
1 Science.gov Way
Oak Ridge, TN 37830

THE UNIVERSITY OF UTAH

November 14, 1988

U.S. Department of Energy
Dr. R. Gajewski ER-16
19901 Germantown Road
Germantown, MD 20874

Dear Dr. Gajewski:

We are now enclosing our replies to the Reviewers #1 to 5 of our proposal "The Behavior of Electrochemically Compressed Hydrogen and Deuterium". As some of our replies to Reviewer #1 and one reply to Reviewer # 3 are relevant to some of the comments made by other Reviewers, we are attaching spare copies where appropriate.

We were very glad to be able to have the extended conference call with you last Monday. During this call you raised with us the question as to what our intentions might be regarding publication and we equivocated. Perhaps we should explain to you that we approach this project with a mixture of excitement, skepticism, and trepidation. Trepidation because of the following scenario: just suppose that the project is successful, then how would one react to the ready production of neutron sources using easily available laboratory components? Clearly, controls might have to be imposed. However, we are quite sure also that if we are successful, then others may stumble on such a device. Because of this we are naturally concerned that it should be us (us and U.S.) who should get there first.

The questions to be raised are complicated by a further issue. A first assessment of a likely device indicates that there might be no defense application but one can soon see that this is not necessarily so. This problem was approached obliquely by Reviewer #5 in his question #4 and we did in fact allude to the problems in an obscure (deliberately so) way in item #6 of the section "Discussion and Proposed Plans" of the proposal. Clearly this item might raise issues of national security and we would prefer not to carry out any research at this stage on such defense-related topics. However, we are quite willing to discuss the modification of nuclear devices which might be used to test our hypotheses--admittedly under rather different conditions than the ones we have primarily in mind. This would constitute rapid, if expensive, tests of our proposal.

Department of Chemistry
Henry Eyring Building
Salt Lake City, Utah 84112

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We hope that the comments will satisfy the Reviewers so that we may be able to proceed with this work. We look forward to hearing from you. Best regards.

Sincerely yours,

Martin Fleischmann

Professor Martin Fleischmann, F.R.S.
Department of Chemistry
The University
Southampton, Hants. SO9 5NH
(B) 011 44 (703) 559122
(H) 011 44 (747) 870384

Stanley Pons, Professor and Chairman
Department of Chemistry
University of Utah
Salt Lake City, UT 84112
(B) (801) 581-4760
(H) (801) 484-0434

November 18, 1988

Professor Neil Ashcroft
LASSP
Cornell University
Clark Hall
Ithaca, NY 14853-2501

Dear Professor Ashcroft:

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 #3.

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

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REVIEWER #3

I am sorry, but I find it very difficult to accept the preliminary findings of Pons/Fleischmann. Deuteriums in palladium are not significantly closer together than they are in solid deuterium. Thus if they are claiming fusion in Pd at the atomic length scales typical of this alloy, then they should also see similar results from pure solid deuterium. It is a rather obvious test.

The idea that the environment of palladium (as a host) is playing a role similar to the negative muon in muon catalysis of D-T is rather primitive. If the important quantity is the overlap of deuterium wave-functions, then it is not at all clear that a palladium host does any better than the molecule of deuterium.

So far as the so-called experiment is concerned, the investigators seem to have trouble in doing their energy bookkeeping and suggest that some "excesses" on the order of 10% are due to fusion. There is almost no discussion of possible heat leaks. The authors should be held to account for their statement that their experiment was "accompanied by an increase in the background radiation count in the lab of > 50%. The long term experiments were all terminated at about this time." It is scientifically irresponsible to leave things this way: what radiation? Why wasn't this followed up by the University safety people?

I don't think you should proceed with this.

Reply to reviewer #3

We will reply to the reviewer according to his paragraphs.

#1 We are at a loss to know how the reviewer can make this statement. Solid D_2 (where the nuclei are held at positions determined by the s-orbitals of the electrons) is simply not comparable to D+ dissolved in a Pd host lattice--which is in effect a high density, high pressure, low temperature and infinitely long lived D+ plasma contained in a lattice where the electrons are in the d-bands of the metal. We have pointed out in the proposal that there must be collisions between D+ nuclei in this system and that the repulsive potential must be appreciably screened by the electrons in the host lattice but that D_2 is NOT formed in this system.

#2 We also do not know how the reviewer can make this statement. Our experiments are in no sense comparable to muon catalyzed fusion (which is dependent on reducing the D-D distance due to the mass of the muon). We only referred to muon catalysis in order to point out that the conditions in tauri,tocomacs, H-bombs or in laser fusion are not an essential prerequisite for fusion.

#3 Again we are at a loss to know how the reviewer could make this comment. We actually pointed out that we have greater than 25% excess energy released at the highest current density. This occurred in three runs of 75, 155 and 101 hours duration. There was absolutely no possibility of heat leaks as the averaged temperature difference between the inside of the Dewar and the external water bath (which in turn was above room temperature) was 1.33(4), 1.43(6), and 1.44(2)°C respectively. Our reply to the reviewer #1 question #6 is pertinent to the interpretation of the excess energy. As this reply is lengthy, we attach an extra copy.

The radiation was beta/gamma type, possibly due to the reaction of thermalized neutrons with components of the Dewar. The matter was not followed up because it would in fact have been irresponsible of us to proceed with the experiments in their present form. We need the resources asked for to carry out the experiments under properly controlled conditions. However, we fully realized the outrageous nature of our proposals which is why we spent a considerable sum (personal funds) in order to at least get some preliminary evidence that the concepts are worth pursuing.

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.

November 18, 1988

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

Dear Dr. Beck:

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

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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, $0<$(payoff)(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.

November 18, 1988

Professor L. M. Falicov
Department of Physics
University of California
Berkeley, CA 94720

Dear Professor Falicov:

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 #4.

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

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REVIEWER #4

Reviewer's Report to the Department of Energy
Proposal by Prof. Stanley Pons
University of Utah
The Behavior of Electrochemically Compressed Hydrogen and Deuterium

This is a truly maverick proposal; it is also an outstanding one.

It proposes to study the feasibility of obtaining nuclear fusion in Deuterium by electrochemical compression in a Pd electrode.

There is some very interesting and high-class electrochemistry involved here. And, even though the probability of finding the ideal conditions of particle density / temperature / volume / lifetime is very small and the chances of success remote, the possible pay-off is so large that support in small scale to this project should be given.

Both principal investigators seem to have the necessary qualifications to carry out high-quality research and to be able to judge their results coolly and impartially.

It is a long-shot, with small probability of success. But it involves good science and the remote possibility of enormous pay-off.

Recommendation: support the research on a one-time-only basis. (No renewal unless positive results are CLEARLY obtained)

Reply to reviewer #4

We would like to assure the reviewer that we fully understand the outrageous nature of our proposal. This is why we spent considerable personal funds to try to obtain some preliminary evidence that the concepts were worth pursuing.

Thank you for your support!

We need one clear year of experimentation after the apparatus is assembled (about 6 months), basically due to the fact it will take about this long to saturate the large rods with D_2. We therefore think that the feasibility question could certainly be answered within 18 months.

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

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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.

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

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

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

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.

November 18, 1988

Professor Johann Rafelski
Department of Physics
University of Arizona
Tucson, AZ 85721

Dear Jan:

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 #2.

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

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REVIEWER #2

I have carefully studied the proposal submitted by Dr. S. Pons from the University of Utah entitled "The Behavior of Electrochemically Compressed Hydrogen and Deuterium". I am responding as a referee specialized in Nuclear and Particle Physics, and will not comment at the matters related to electrochemical analysis. However I wish to mention that the proposal, even though it refers to pilot experiments, never does clearly commit the author to a certain result.

The proposal addresses the issue pertinent to spontaneous fusion of hydrogen isotopes placed inside a metal lattice. The method of experimental approach selected here is to study excess heat generated by fusion energy. I support in principle the study of the general issue raised in this proposal, but have very grave doubts about the method selected, in particular I am concerned, if it is sufficiently sensitive to find a new effect not formerly observed in an incidental way by nuclear detection methods (fusion neutrons etc).

Since the energy gain from fusion is 10^7 times greater than the chemical energy gain, this method would work if fusion rates are some good fraction, say 10^{-10} of the chemical reaction rates. This implies in turn that fusion rates at the level of 10^{-16}/s may be detectable by this method. What is indeed badly missing in the proposal is a more accurate back of the envelope estimate how a hypothetical fusion rate relates to the excess heat and which range of fusion rates would be accessible to measurement in the proposed set up, considering the usual uncertainties of the method. Without such a discussion of this question it is in my judgement impossible to evaluate the chances of success for the proposed work, since we do not know how the expected result would show in other physical environments.

Neither does the proposal indicate what one does if the effect one is looking for, excess heat, is actually found! One can not simply claim "eureka, fusion" . There are many other sources of energy in a complex system considered for this investigation, and there is no attempt made to identify the source of heat.
I do not recommend that the funding for this project be based on the present submission. I would like to reserve my final recommendation until I see an addendum or a new proposal in which two matters are put straight:

1: which range of fusion rates is measurable in the proposed set up;
2: how will the decision be made that any energy excess is of nuclear origin.

Reply to reviewer #2

We will reply to the reviewer's comments paragraph by paragraph.

#1 We are at a loss to know how the reviewer can make this statement. How much more specific can we be than to say that we had ca. 25% excess energy produced at the highest current density? The reviewer may wish to know that we observed this excess energy in three runs of 75, 155 and 101 hours.

#2 We believe that such effects were not observed previously because physical chemists and physicists simply do not set up experiments of several thousand hours duration to look for small calorimetric effects. A short duration experiment would also not give any detectable radiation.

#3 We would like to assure the reviewer that we have carried out many back-of-the-envelope calculations. Our own calculations showed that fusion rates of the order 3×10^{-16} s^{-1} would be readily detectable by the methods we have outlined. With special precautions and cell design, rates as low as 3×10^{-17} s^{-1} (or even 3×10^{-18} s^{-1}) might be detectable. The fusion rate (if indeed it was that) in our experiments at the highest current densities was about 3×10^{-14} s^{-1}. It is a straightforward matter to confirm these figures taking into account the likely Newton's law of cooling for Dewars, and the temperature differences between the inside of the Dewar and the surrounding water bath readily accessible to measurement. Further, it is our opinion that any meaningful calculations such as those proposed by the reviewer at a minimum would require a detailed quantum-mechanical molecular dynamical calculation; we have talked extensively with several of our colleagues (expert in these types of studies) regarding such a calculation. They have evidently not been made successfully in the past, and would require a major research-computing effort. We would hope to take on (or see others do so) such a project after the experimental verification has been made. We agree that it is difficult to evaluate the chance for success of this work, but we must also question the applicability of the proposed calculations in making such an evaluation easier.

#4 Our reply to the question #6 of the first reviewer and paragraph (3) of the third reviewer are relevant to this comment by the present reviewer and are attached.

1: We have replied to this under #3 above.

2: As we have pointed out in the proposal, we shall seek to correlate any excess energy released with tritium produced; we shall look for thermalized neutrons and for gamma-rays generated by any reactions of these thermalized neutrons with components of the Dewar etc.

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.

Paragraph (3) of Reviewer #3:

So far as the so-called experiment is concerned, the investigators seem to have trouble doing their energy bookkeeping and suggest that some "excesses" on the order of 10% are due to fusion. There is almost no discussion of possible heat leaks. The authors should be held to account for their statement that their experiment was "accompanied by an increase in the background radiation count in the lab of >50%. The long term experiments were all terminated at about this time." It is scientifically irresponsible to leave things this way: what radiation? Why wasn't this followed up by the University safety people?

Our reply:

#3 Again we are at a loss to know how the reviewer could make this comment. We actually pointed out that we have greater than 25% excess energy released at the highest current density. This occurred in three runs of 75, 155 and 101 hours duration. There was absolutely no possibility of heat leaks as the averaged temperature difference between the inside of the Dewar and the external water bath (which in turn was above room temperature) was 1.33(4), 1.43(6), and 1.44(2)°C respectively. Our reply to the reviewer #1 question #6 is pertinent to the interpretation of the excess energy. As this reply is lengthy, we attach an extra copy.

The radiation was beta/gamma type, possibly due to the reaction of thermalized neutrons with components of the Dewar. The matter was not followed up because it would in fact have been irresponsible of us to proceed with the experiments in their present form. We need the resources asked for to carry out the experiments under properly controlled conditions. However, we fully realized the outrageous nature of our proposals which is why we spent a considerable sum (personal funds) in order to at least get some preliminary evidence that the concepts are worth pursuing.