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

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

U.S. Department of Energy
Office of Energy Research

PROPOSAL COVER SHEET

8365

DATE: 08/31/88

To: ADVANCED ENERGY PROJECTS
(ATTN: RYSZARD GAJEWSKI)

THE ATTACHED PROPOSAL IS FOR YOUR REVIEW & APPROPRIATE ACTION.

PROPOSER: UTAH, UNIVERSITY OF
SALT LAKE CITY, UT

P.I.(s): PONS, STANLEY

TITLE: THE BEHAVIOR OF ELECTROCHEMICALLY COMPRESSED HYDROGEN AND DEUTERIUM

TYPE OF PROPOSAL: NEW

DATE RECEIVED: 08/29/88

NO. OF COPIES: 07

AWARD NO.: N/A

UNSOLICITED PROPOSAL NO.: N/A

* * *

THIS PROPOSAL HAS BEEN GIVEN A PRELIMINARY ADMINISTRATIVE REVIEW BY THE ACQUISITION & ASSISTANCE MANAGEMENT DIVISION, ER-64.

- **THIS PROPOSAL WAS RECEIVED**
  DIRECTLY FROM THE PROPOSER

- **THIS PROPOSAL WAS ALSO SUBMITTED TO:**

- **OTHER COMMENTS:**

THE UNIVERSITY OF UTAH

26 August 1988

DHL COURIER

U S DEPARTMENT OF ENERGY
ACQUISITION & ASSISTANCE
MANAGEMENT DIVISION
OFFICE OF ENERGY RESEARCH ER64
19901 GERMANTOWN ROAD
GERMANTOWN MD 20874

SUBJECT: New Proposal
University of Utah PID No. 8808032

Dear Sir/Madam:

We have enclosed an original and six (6) copies of the proposal for the project entitled "THE BEHAVIOR OF ELECTROCHEMICALLY COMPRESSED HYDROGEN AND DEUTERIUM" under the direction of Dr. B. Stanley Pons, Department of Chemistry. These documents have been signed by an authorized official of the University of Utah.

This proposal is in the amount of $632,281 for the performance period 1 October 1988 to 30 September 1991. Of this amount, $208,375 is for the first year.

We appreciate your consideration of this proposal and look forward to hearing from you when your review is completed.

Very truly yours,

Richard H. Timpson
Director
Research Administration

kb

Enclosures: 7

cy: Dr. B. Stanley Pons - Chemistry
Dr. Hugo Rossi, Dean - College of Science

Office of Research Administration
309 Park Building
Salt Lake City, Utah 84112
(801) 581-6903

OMB Approval No. 0348-0006

FEDERAL ASSISTANCE

1. TYPE OF SUBMISSION *(Mark appropriate box)*
☐ NOTICE OF INTENT (OPTIONAL)
☐ PREAPPLICATION
☒ APPLICATION

2. APPLICANT'S APPLICATION IDENTIFIER
a. NUMBER
b. DATE *Year month day* 19 8 23 88

3. STATE APPLICATION IDENTIFIER *NOTE: TO BE ASSIGNED BY STATE*
a. NUMBER
b. DATE ASSIGNED *Year month day* 19

Leave Blank

4. LEGAL APPLICANT/RECIPIENT
a. Applicant Name University of Utah
b. Organization Unit Stanley Pons
c. Street/P.O. Box Department of Chemistry
d. City Salt Lake City
e. County Salt Lake
f. State Utah
g. ZIP Code 84112
h. Contact Person *(Name & Telephone No.)* Stanley Pons (801)581-4760

5. EMPLOYER IDENTIFICATION NUMBER (EIN) 187600525A1

6. PROGRAM *(From CFDA)*
a. NUMBER 81.049
MULTIPLE ☐
b. TITLE Basic Energy Sciences

7. TITLE OF APPLICANT'S PROJECT (Use section IV of this form to provide a summary description of the project.)
The Behavior of Electrochemically Compressed Hydrogen and Deuterium

8. TYPE OF APPLICANT/RECIPIENT
A—State
B—Interstate
C—Substate Organization
D—County
E—City
F—School District
G—Special Purpose District
H—Community Action Agency
I—Higher Educational Institution
J—Indian Tribe
K—Other *(Specify)*:
Enter appropriate letter I

9. AREA OF PROJECT IMPACT *(Names of cities, counties, states, etc.)*

10. ESTIMATED NUMBER OF PERSONS BENEFITING

11. TYPE OF ASSISTANCE
A—Basic Grant
B—Supplemental Grant
C—Loan
D—Insurance
E—Other
Enter appropriate letter(s) A

12. PROPOSED FUNDING

a. FEDERAL	$208375 .00
b. APPLICANT	40000 .00
c. STATE	.00
d. LOCAL	.00
e. OTHER	.00
f. Total	$248375 .00

13. CONGRESSIONAL DISTRICTS OF:
a. APPLICANT UTAH 2nd
b. PROJECT UTAH 2nd

14. TYPE OF APPLICATION
A—New
B—Renewal
C—Revision
D—Continuation
E—Augmentation
Enter appropriate letter A

15. PROJECT START DATE *Year month day* 19 88 10 1

16. PROJECT DURATION 36 *Months*

17. TYPE OF CHANGE *(For 14c or 14e)*
A—Increase Dollars
B—Decrease Dollars
C—Increase Duration
D—Decrease Duration
E—Cancellation
F—Other *(Specify)*:
Enter appropriate letter(s)

18. DATE DUE TO FEDERAL AGENCY ► *Year month day* 19

19. FEDERAL AGENCY TO RECEIVE REQUEST DOE Office of Energy Research
a. ORGANIZATIONAL UNIT (IF APPROPRIATE) Office of Energy Research
b. ADMINISTRATIVE CONTACT (IF KNOWN) Ryszard Gajewski
c. ADDRESS Acquisition and Assistance Management Division Office Energy Research ER64 DOE Washington dc 20545

20. EXISTING FEDERAL GRANT IDENTIFICATION NUMBER

21. REMARKS ADDED ☐ Yes ☒ No

SECTION II—CERTIFICATION

22. THE APPLICANT CERTIFIES THAT ► To the best of my knowledge and belief, data in this preapplication/application are true and correct, the document has been duly authorized by the governing body of the applicant and the applicant will comply with the attached assurances if the assistance is approved.

a. YES, THIS NOTICE OF INTENT/PREAPPLICATION/APPLICATION WAS MADE AVAILABLE TO THE STATE EXECUTIVE ORDER 12372 PROCESS FOR REVIEW ON: DATE ____________

b. NO, PROGRAM IS NOT COVERED BY E.O. 12372 ☐ OR PROGRAM HAS NOT BEEN SELECTED BY STATE FOR REVIEW ☐

23. a. TYPED NAME AND TITLE James J. Brophy

b. SIGNATURE James J Brophy

AUG 26 1988

SECTION III—FEDERAL

...ICATION IDENTIFICATION NUMBER

26. FEDERAL GRANT IDENTIFICATION

29. ACTION DATE ► *Year month day* 19

.00
.00
.00
.00
.00
.00

30. STARTING DATE *Year month date* 19

31. CONTACT FOR ADDITIONAL INFORMATION *(Name and telephone number)*

32. ENDING DATE *Year month date* 19

33. REMARKS ADDED ☐ Yes ☐ No

NOTICE FOR HANDLING PROPOSALS

"This proposal is to be used only for DOE evaluation purposes and this notice shall be affixed to any reproduction or abstract thereof. All Government and non-Government personnel handling this proposal shall exercise extreme care to insure that the information contained herein is not duplicated, used, or disclosed in whole or in part for any purpose other than to evaluate the proposal, without the written permission of the offeror (except that if a contract is awarded on the basis of this proposal, the terms of the contract shall control disclosure and use). This notice does not limit the Government's right to use information contained in the proposal if it is obtainable from another source without restriction. This is a Government notice, and shall not by itself be construed to impose any liability upon the Government or Government personnel for any disclosure or use of data contained in this proposal."

OFFICIAL FILE COPY

424-103

STANDARD FORM 424 PAGE 1 (Rev. 4-84)
Prescribed by OMB Circular A-102

ER F 4620.1
(7-85)

U.S. Department of Energy

Grant Application Budget Period Summary

(See Reverse for Definitions and Instructions)

OMB Approval
No. 1910-1400

Please Print or Type

Organization: University of Utah	Period Covering: From: 10-1-88	FOR DOE USE ONLY Proposal No:
Principal Investigator (P.I.)/Project Director (P.D.): Stanley Pons	To: 9-30-89	Award No.:

A. SENIOR PERSONNEL PI/PD Co PIs, Faculty and Other Senior Associates (List each separately with title, A.6 show number in brackets. Attach separate sheet, if required.)	DOE Funded Person-Mos. Cal.	Acad.	Sumr.	Funds Requested By Applicant $	
1. Martin Fleischmann, Co PI, Professor,	4			16000	
2. No Employee Benefits /from England					
3.					
4.					
5.					
6. (1) TOTAL SENIOR PERSONNEL					
B. OTHER PERSONNEL (SHOW NUMBERS IN BRACKETS)					
1. (1) POST DOCTORAL ASSOCIATES	12			20000	
2. ( ) OTHER PROFESSIONALS (TECHNICIAN, PROGRAMMER, ETC.)					
3. (2) GRADUATE STUDENTS	12			20000	
4. ( ) UNDERGRADUATE STUDENTS					
5. ( ) SECRETARIAL-CLERICAL					
6. ( ) OTHER					
TOTAL SALARIES AND WAGES (A + B)				56,000	
C. FRINGE BENEFITS (IF CHARGED AS DIRECT COSTS) Post Doc 14% Grad Stud. 8% only				4350	
TOTAL SALARIES, WAGES AND FRINGE BENEFITS (A + B + C)					60,350
D. EQUIPMENT (LIST ITEM AND DOLLAR AMOUNT FOR EACH ITEM) ~~Calorimeter for temp msmt~~				~~23500~~	
3 Potentiostat-galvanostats-power for cells				17940	
1 waveform generator for potential programs				5790	
Temp tranducers, PC for control and recording				11000	
TOTAL EQUIPMENT				58230	
E. TRAVEL 1. DOMESTIC (INCL. CANADA AND U.S. POSSESSIONS)					
2. FOREIGN Fleischmann to Utah to participate in work				2500	
F. OTHER DIRECT COSTS					
1. MATERIALS AND SUPPLIES Rods, heavy water, other metals,				48000	
2. PUBLICATION COSTS/PAGE CHARGES				500	
3. CONSULTANT SERVICES				18000	
4. COMPUTER (ADPE) SERVICES					
5. CONTRACTS AND SUBGRANTS					
6. OTHER					
TOTAL OTHER DIRECT COSTS				66500	
G. TOTAL DIRECT COSTS (A THROUGH F)				187580	
H. INDIRECT COSTS (SPECIFY RATE AND BASE) 47% Direct Costs, except equipment TOTAL INDIRECT COSTS				60795	
I. TOTAL DIRECT AND INDIRECT COSTS (G & H)				248375	
J. APPLICANT'S COST SHARING (IF ANY)				40000	
K. TOTAL AMOUNT OF THIS REQUEST (ITEM I LESS ITEM J)				208375	

PI/PD TYPED NAME & SIGNATURE Stanley Pons	DATE 8-23-88
INST. REP. TYPED NAME & SIGNATURE JAMES J. BROPHY Vice President for Research James J. Brophy	DATE AUG 26 1988

For further information, other than technical contact Richard H. Timpson
Research Administration 801-581-6903

ER F 4620.1A
(7-85)

U.S. Department of Energy

GRANT APPLICATION
PROJECT PERIOD SUMMARY

(Must be completed for all new and renewal applications.)

Please Print or Type

OMB Approval
No. 1910-1400

Categories	01 Budget Period	02 Budget Period	03 Budget Period	04 Budget Period	05 Budget Period
A. Senior Personnel Totals	16000	18000	20000		
B. Other Personnel Totals	58000	60000	62000		
C. Fringe Benefit Totals	4350	4500	4650		
Total of A, B & C	78350	82500	86650		
D. Equipment	58230	10000	10000		
E. Travel 1. Domestic					
2. Foreign	2500	3000	3500		
F. Other Direct Costs	48500	52000	58000		
G. Total Direct Costs	187580	147500	158150		
H. Total Indirect Costs	60795	64625	69631		
I. Total Direct & Indirect Costs	248375	212125	227781		
J. Applicant's Cost-Sharing (if any)	40000	8000	8000		
K. Total Amount of Request (Item I. Less Item J.)	(1)* 208375 ✓	(2) 204125 ✓	(3) 219781 ✓	(4)	(5)

*This should equal Item K on Budget Period Summary (ER/F/4620.1)

For further information, other than technical contact Richard H. Timpson Research Administration 801-581-6903

ESTIMATE

TOTAL COST OF PROJECT

$ 632,281✓

(add K(1) thru (5))

THE BEHAVIOR OF ELECTROCHEMICALLY COMPRESSED HYDROGEN AND DEUTERIUM

Stanley Pons
Professor and Chairman
Department of Chemistry
University of Utah
Salt Lake City, UT 84112 USA

Martin Fleischmann, FRS
Professor
Department of Chemistry
The University
Southampton, Hants. SO9 5NH
ENGLAND

Abstract

The raising of the chemical potential of low atomic number nuclei (such as deuterium) to values > 0.1eV results in significant probability of fusion of the nuclei. Most attempts to produce methods for controlled terrestrial fusion reactions are based on the raising of the chemical potential of the nuclei by thermal means. This is certainly not a necessary requirement, however, as has been demonstrated by experiments involving room temperature muon-catalyzed fusion reactions, a possibility that has been known for over 40 years. We have preliminary evidence that the required chemical potentials for controlled fusion reactions of small nuclei may be attained by simple electrochemical means. In a metallic host lattice, for instance, there is apparently sufficient shielding of the electrostatic repulsion of deuterium plasma by the electrons in the metal to allow very short approach distances of deuterons. Under these conditions, the cross section for fusion is significant. The chemical potential of deuterium in a palladium lattice, for instance, has been raised to values > 0.8eV. To attain this energy by compression, one would have to generate static pressures of 10^{27} atmospheres. Under these conditions, we have observed continuous excess thermal energy generation of >25% above that expected to arise from the most likely electrochemical reaction occurring in the device, and concomitant increase in background radiation levels. We intend to further demonstrate and extend the scope of these experiments, and we will investigate possible applications to a new type of simply controlled fusion reactor.

THE BEHAVIOR OF ELECTROCHEMICALLY COMPRESSED HYDROGEN AND DEUTERIUM

Introduction and Description of the Experiments

We describe here preliminary results of an experiment designed to investigate the result of compression of deuterium at very high pressures using a simple device.

The starting point of our investigation was a reconsideration of the possibility of forming metallic hydrogen or deuterium, which should occur at about 10^6 atmospheres and which should have applications as a room temperature superconductor, a powerful fuel, in nuclear devices, and as an explosive. Here we will confine attention to the deuterium system alone.

Whereas it would be difficult to achieve the conditions for producing metallic deuterium, alloys of deuterium are readily formed; a well known example is the Pd-D system. The correct description of such systems is still uncertain, but one knows that: a) the deuterium is mobile in the metal lattice (it has a high diffusion coefficient) and exists as D^+ (migration of the species in an electric field, etc.); b) the electrons go into the Pd band structure; c) the D^+ are essentially unbound; d) it is possible to raise the chemical potential of D in the lattice by perhaps 0.8eV by applying cathodic potentials to a Pd electrode. While this may appear to be a modest figure, it is in fact of astronomical magnitude: if one attempted to achieve the same result by compressing D_2, one would require 10^{27} atmospheres! It would be straightforward to develop even higher chemical potentials (see (3) of section Discussion and Proposed Plans).

It is because of the extremely high chemical potentials of D which can be developed within the host Pd lattice by suitable electrochemical polarization, that we have considered the possibility of fusion of D nuclei. It is in fact difficult to predict the collisional behavior of $D^+ + D^+$ under these particular conditions. Clearly the repulsive potential of the nuclei will be screened to some extent by the electrons in the metal, but D_2 is not formed, i.e. the s-character is weak. One can devise plausible arguments in favor or against any fusion reaction such as

$^{2}D + ^{2}D \rightarrow ^{3}T(1.01MeV) + ^{1}H(3.02MeV)$

$^{2}D + ^{2}D \rightarrow ^{3}He(0.82MeV) + n(2.45MeV)$

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. We note that in the well known magic expression (Particle Density x Temperature x Volume x Lifetime) usually associated with conditions for nuclear fusion, our particle densities are astronomically high, our temperatures are low, our volumes low, and our lifetimes infinitely long compared to the conditions in tauri, tocomacs, H-bombs, or in laser fusion. We compare, for instance, the figures for our cell and an "ideal" self-sustaining thermonuclear fusion reactor: such a device might contain a deuterium (reaction cross section about $10^{-24}cm^{2}$) particle density of 10^{14} cm^{-3} at a temperature of $10^{8}K$, a volume of $10^{6}cm^{3}$, and a confinement time of 1s. The pressure of this plasma is therefore about 30 atmospheres. The device described herein has the same temperature as the muon-catalyzed fusion reactors, about 0.025eV, but the particle density is on the order of 10^{23} with a volume of about $10^{1}cm^{3}$ and a confinement time of $>10^{6}s$. The resulting calculated pressure is on the order of the measured rise in chemical potential, approximately 10^{27} atmospheres.

The basic experiment was based on the cathodic reduction of D_2O from the liquid phase at near room temperature using low current densities ($< 1.6mA\ cm^{-2}$). The D formed was initially absorbed into the palladium metal lattice eventually forming β-PdH. When saturation conditions had been reached at any particular current density, D_2 was evolved at the Pd cathode at a steady rate. The electrochemical cell consisted of an evacuated Dewar mounted in a constant temperature water bath and the heat release under steady state conditions was determined from the thermal balances in the Dewar. The temperature inside and outside the Dewar were monitored with Beckmann thermometers. The secondary electrode inside the Dewar was a cylinder of platinized platinum foil about 10cm high and 5cm in diameter. The working electrode was an 8 x 8 x 0.2cm sheet of palladium metal that had been folded to fit inside the platinum cylinder. The solution was 0.1M LiOD in 99.99% D_2O. A gas sparger was placed inside the Dewar. For some experiments, D_2 was replaced in the Dewar by sparging in from an external generator.

The experiment was controlled by a galvanostat (constant current generator). The temperature of the water bath, temperature inside the Dewar, the potential between the two electrodes, the cell current, and rate of D_2 and O_2 evolution in the auxiliary generator were monitored as a function of time. The system was calibrated for the Heavy Water Equivalent, HWE, (g D_2O) and Newton's law of cooling constant, k (joules s^{-1}) by adding m grams of D_2O at elevated temperature T_2 to the LiOD/D_2O solution already in the Dewar at temperature T_1 and measuring the temperature transient; the Dewar here contained all of the components that it did during the constant current experiments. We denote the HWE of the Dewar and contents by M (g D_2O).

Treatment of the Data

We denote the initial temperature of the (m + M) g D_2O in the Dewar by T_3. A thermal balance then gives

$$(m + M)(T_3 - T_1) = m(T_2 - T_1) \qquad [1]$$

so that

$$M = m(T_2 - T_3) / (T_3 - T_1) \qquad [2]$$

We take the heat capacitance of D_2O to be C $J\ g^{-1}(^\circ)^{-1}$. Then the rate of cooling of the Dewar is given by

$$(m + M)C\ d(T - T_1)/dt = -k(T - T_1) \qquad [3]$$

Integration of [3] subject to the initial condition

$$T - T_1 = T_3 - T_1 \ \text{ at } t = 0 \qquad [4]$$

gives

$$(T - T_1) = (T_3 - T_1) \exp(-kt/(m + M)C) \qquad [5]$$

We thus plot $\ln(T - T_1)$ against t and determine k from the slope since we know the values (m + M) and C.

From our data, we derive the HWE of the system to be 292.7 g D_2O, and the parameter

$$Mk/(m + M) = MC / k/(m + M)C = 0.3113\ J\ g^{-1}(^\circ)^{-1}. \qquad [6]$$

Thermal Balances in the Dewar

One objective during some experiments was to balance the evolution of D_2 on the Pd electrode by D_2 ionization at the platinum electrode. In that case the joule heat due to the electrolysis current would simply be the cell current x cell voltage. Note that if the electrode reactions were the evolution and reduction of oxygen from and to water, then the same relationship would hold for the joule heating. Equally, a mixture of D_2 evolution/ionization and O_2 evolution/reduction would give such a joule heating term. However, any other set of electrode reactions would reduce joule heating. In particular, if the reactions were

$$2D_2O + 2e \rightarrow D_2 + 2OD^- \qquad [A]$$

and

$$4OD^- \rightarrow 2D_2O + 4e + O_2 \qquad [B]$$

(or any other comparable pair of reactions representing the electrolysis of D_2O) then we must subtract from the cell voltage a term giving the cell voltage at which the reaction is "thermoneutral" (neither exo- or endothermic). This voltage is given by

$$-2EF = \Delta H \qquad [7]$$

With the value $\Delta H = -294.6$ kJ mol^{-1} we obtain $E_{thermoneutral} = 1.54V$. For the case of reactions [A] and [B] the joule heating is given by (cell current x (cell voltage - $E_{thermoneutral}$). The two cases give an upper and lower limit to the rate of joule heating, respectively, that can occur in the cell.

We estimate the maximum possible iR drop in the cell from the change in the observed cell voltage when we step the cell current between two values. We have determined that after correction for iR loss, the reactions [A] and [B] are the reactions present; i.e. the joule heating is always given by the lower bound calculated above.

Preliminary Results

First we find that the Newton's cooling loss is always less than the lower bound of the electrolytic joule heating when the system is driven at low current densities (0.4mA cm^{-2}) during the early parts of the experiment (approximately the first 300 hours; the Pd cathode was subjected to a prolonged period of presaturation with D before the start of measurements). We believe that the cooling of the system is due to the continuation of endothermic dissolution of D into the Pd lattice. Toward the end of this period and at somewhat higher current densities (0.8mA cm^{-2}), the Newton's cooling loss almost balances the lower bound of the electrolytic heating. On raising the current density further to 1.2mA cm^{-2} at about 600 hours there is a >9% excess heating effect while from 600 to 1000 hours at the final current density of 1.6mA cm^{-2}, we observe a transition to >25% excess heating. This is 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.

Discussion and Proposed Plans

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. We must, of course, investigate all of these effects in detail and carry out the experiments under more carefully controlled conditions and for a much wider range of the experimental parameters. Although it is possible to suggest drastic changes to our experiments, (see (3) - (6) below), we want to stay with the Pd-D system at this stage and our proposal would cover the initial aspects of (1) - (5) below.

(1) In order to achieve better control and to extend the experimental conditions, we want to purchase Pd rods about 10 cm long and in various diameters up to about 2 cm. These will have to be specially cast (the electrode we have been using to date is made of the heaviest gauge material available commercially). We have designed and need to construct new smaller Dewars to enhance ($T - T_1$). In the first place, we will reduce the size of the Pt electrode so as to decrease any effects of the D_2 ionization / O_2 reduction reactions. The smaller surface area of the electrodes

will decrease the electrolytic joule heating (as compared to any fusion) and will allow us to reach more negative potentials, i.e. higher D compressions. Comparisons of results for different diameters will show whether the excess thermal energy is proportional to the volume of the rods. It should be possible to devise conditions where the excess thermal energy is much larger compared to the lower bound electrolytic joule heating term than in our experiments to date; we should be able to make corrections for the effects of D_2 ionization / O_2 reduction and it may prove possible to make the excess thermal energy large compared to the upper bound electrolytic joule heating term.

Nevertheless, we should devise Dewar cells in which we can restrict the anode reaction to D_2 ionization. We would use fuel cell anodes fed with electrolytic D_2. All Dewars will have to contain reference electrodes (α–β–Pd-D) so that we can monitor the potentials of the electrodes. They should also contain resistance heaters so that we can determine the thermal balances more accurately.

The experiments will take longer than our previous experiments in view of the greater thickness of the rods compared to the sheet electrodes. For instance, it will take approximately 12 months to charge a 2cm diameter rod to saturation with deuterium.

(2) Experiments will be carried out at higher temperatures T_1 in the external bath. This should lead to an increase of the excess thermal energy.

(3) We will attempt to increase the chemical potential of the dissolved D further by selective poisoning of the electrode surface. This will reduce the D_2 evolution at the surface and therefore make the experiment more clear cut. We cannot use the cells described in (1) above or with the same electrodes as the whole system will probably be ruined by the poisoning treatments.

(4) We will search for other metals and other materials as substrates. β-NiD is an obvious target. Experiments on surface poisoning may be better deferred until we start work with cheaper materials.

(5) We will investigate the properties of other alloy hosts. Pd-^{7}Li is a clear choice. Such work will be combined with a search for more favorable solution media such as D_2O / T_2O mixtures. In particular, one questions whether it is possible to induce

$$^2D + {}^3T \rightarrow {}^4He(3.5MeV) + n(14.1MeV)$$

$$^7Li + \text{fast } n \rightarrow {}^3T + {}^4He + n(3.5MeV)$$

$$^6Li + \text{slow } n \rightarrow {}^3T(2.7MeV) + {}^4He(2.1MeV)$$

$$^3T + {}^3T \rightarrow {}^4He + 2n + 12.1MeV$$

$$^2D + {}^6Li \rightarrow 2{}^4He + 22.4MeV$$

although screening of the electrostatic repulsion by the lithium nucleus may be poor due to the presence of its 1s electronic structure. On the other hand, note that the capture cross sections of the dissolved D, T, and Li species may be quite different to those which apply in thermonuclear reactors (compare to mu-meson catalyzed reactions).

(6) Work under (5) raises some further questions. A Pd dispersion in, for instance, a LiD/T matrix rapidly heated would be effectively subjected to very negative potentials and the Li/D/T would be driven into the lattice in approximately 1 - 100μs.

Statement of Practical Application

One of the most intriguing applications is, that since the generation of excess thermal energy appears to be continuous, the system could be simply redesigned to accommodate heat exchangers for external energy generation. Additionally, high thermonuclear energy levels may be obtained if devices constructed from dispersions of metal lattices such as those described in (6) above are utilized.

Statement of Work

1) Project Objective

The principal investigators intend to confirm preliminary observations that it may be possible to attain conditions for nuclear fusion of light nuclei in host metal lattices at low temperatures ($< 1\text{eV}$) by electrochemical means. If successful, we intend to extend the scope of the experiments to determine the optimum conditions for construction of simply controlled fusion reactors.

2) The work to be performed consists of the following tasks:

   2.1 Casting of palladium host lattice rod electrodes in a variety of diameters. Construction of the appropriate secondary electrodes, dewar cells, and associated calorimetric equipment. Construction of measuring equipment, and purchasing of constant temperature baths, appropriate temperature and radiation measuring devices.

   2.2 Calorimetric, weight gain, and radiation measurements of the palladium generators as a function of rod volume. Chemical analysis of Dewar contents.

   2.3 Calorimetric, weight gain, and radiation measurements of the palladium generators as a function of bath temperature.

2.4 Extension of the experiments to include other metal lattices known to sorb deuterium. Studies of the increase in reactant particle density (increase of chemical potential) in the metal lattice by poisoning of competitive surface electrochemical processes.

2.5 Extension of the experiments to include potentially more powerful, interesting, or useful reactants such various alloys of deuterium, tritium, lithium, and boron.

2.6 Extension of the experiments to include dispersions of electrodes in various reactants. These experiments would require special facilities, and should be discussed at a later point.

2.7 Preliminary engineering and economic analyses according to the results of paragraphs 2.1-2.6.

3) Deliveries

The principal investigators shall provide the data of experiments performed according to paragraphs 2.1-2.6 along with analyses and conclusions based on this data.

4) Performance Schedule

4.1 Complete construction of the equipment and electrodes within 120 days after start of work.

4.2 Complete thermodynamic measurements as a function of electrode volume and temperature within 24 months after start of work (large electrodes will take about one year to charge).

4.3 Complete measurements as a function of electrode material and nature of reactants within 34 months after start of work.

4.4 Decision to determine the feasibility of proceeding to experiments utilizing dispersions of electrodes to be made in third year.

4.5 Complete data correlation, economic analysis, and final report 36 months after start of work.

Facilities

The experiments will be conducted at the University of Utah. The Department of Chemistry at the University of Utah has excellent equipment and plant facilities. We have recently occupied a new building which has increased our space by 50%. The research described herein will be performed in this new facility. The department owns all of the normal instrumentation found in first rate Departments, including mass spectrometers, x-ray diffractometers, FTIR equipment, Raman spectrometers, ESR and ENDOR facilities, ORD and polarimeter equipment, several superconducting NMR spectrometers, and numerous smaller items for chemical analysis. The equipment services are staffed by a team of 8 Ph.D. chemists. We have excellent shop facilities including machine, electronics, and glassblowing which are staffed by 10 technicians.

In addition to the Departmental facilities, our research group has a vacuum FTIR spectrometer (purchased under the DOD University instrumentation program), reflectance MSRS UV-VIS reflectance spectrometer, EMIRS reflectance spectrometer, several high performance PC computers dedicated to various experiments, and several electrochemical systems set up on various experiments. These are composed typically of potentiostats, waveform generators, transient recorders, integrators, cells, oscilloscopes, and related circuitry and interfaces. We also own the usual items found in most well-equipped analytical laboratories. The major equipment will be available for use in the proposed research. Most of this equipment was purchased under DOD grants and contracts.

Other Support

We certify that we originally sent portions of this proposal to the Office of Naval research for inspection and advice. After correspondence with ONR, it was felt that the most appropriate source of support for this type of project was the Advanced Energy Projects Division of the Department of Energy. We have therefore resubmitted this proposal to that Division. We do not anticipate that we will request other research support if funded by DOE.

August 23, 1988

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1986- Professor, University of Utah, Chemistry Department.
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National Service:

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Curriculum Vitae

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Date of birth: 29 March 1927

Place of birth: Karlsbad, Czechoslovakia. Nationality: British

Education: High School, Worthing, Sussex, England.
1939-1945.

Imperial College, London 1945-1949.
ARCS (Associate of the Royal College of Science) 1947
1st Class Special Honours Degree awarded in 1948.

Imperial College, London, 1947-1950
Degree of Ph.D. of London University, 1951.

Academic Career: 1950-1967

Successively Research Fellow and Imperial Chemical Industries Research Fellow at Kings College, University of Durham, Newcastle-upon-Tyne (now the University of Newcastle-upon-Tyne) then Lecturer and Reader in Physical Chemistry, University of Newcastle-upon-Tyne.

1967-1983

Professor of Electrochemistry at University of Southampton. (Chair originally endowed by the Electricity Council).

1977-1982

Science Research Council Senior Fellow.

1983-present

Honorary Research Professor. Also now hold part-time positions at Harwell and University of Utah.

1986. Elected Fellow of the Royal Society.

Other positions: Ex-Secretary and President of ISE. Ex-member of SERC Chemistry Committee. Consultant to several British, European and U.S. Industrial and/or Government Laboratiries.

Medals: The Royal Society of Chemistry. Medal for Electrochemistry and Thermodynamics (1979).

Olin-Palladium Medal of The Electrochemical Society (1985).

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Revised Budget

THE UNIVERSITY OF UTAH

28 February 1989

AIRBORNE

RYSZARD GAJEWSKI
U S DEPARTMENT OF ENERGY
ACQUISITION & ASSISTANCE MANAGEMENT DIVISION
OFFICE OF ENERGY RESEARCH ER64 DOE
19901 GERMANTOWN ROAD
GERMANTOWN MD 20874

SUBJECT: Revised Budget
University of Utah PID No. 8808032

Dear Mr. Gajewski:

We have enclosed an original and five (5) copies of the budget revision which was sent to you by FAX machine on 27 February 1989. This revision is for the project entitled "THE BEHAVIOR OF ELECTROCHEMICALLY COMPRESSED HYDROGEN AND DEUTERIUM" under the direction of Dr. B. Stanley Pons, Department of Chemistry. These documents have been signed by an authorized official of the University of Utah.

This revision increases the amount requested for the first 18 months to $322,462.

We appreciate your consideration of this proposal.

Very truly yours,

Richard H. Timpson
Director
Sponsored Projects

kb

Enclosures: 6

cy: Dr. B. Stanley Pons
Dr. Hugo Rossi, Dean

Office of Sponsored Projects
309 Park Building
Salt Lake City, Utah 84112
(801) 581-6903
Fax Machine (801) 581-3007

OMB Approval No. 0348-0006

FEDERAL ASSISTANCE

1. TYPE OF SUBMISSION *(Mark appropriate box)*
☐ NOTICE OF INTENT (OPTIONAL)
☐ PREAPPLICATION
☒ APPLICATION

2. APPLICANT'S APPLICATION IDENTIFIER
a. NUMBER
b. DATE *Year month day* 19 89 2 27

3. STATE APPLICATION IDENTIFIER
a. NUMBER
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NOTE: TO BE ASSIGNED BY STATE

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4. LEGAL APPLICANT/RECIPIENT
a. Applicant Name : University of Utah
b. Organization Unit : Stanley Pons
c. Street/P.O. Box : Department of Chemistry
d. City Salt Lake City
e. County Salt Lake
f. State Utah
g. ZIP Code 84112
h. Contact Person *(Name & Telephone No.)* Stanley Pons
(801) 581-4760

5. EMPLOYER IDENTIFICATION NUMBER (EIN)
187600525A1

6. PROGRAM *(From CFDA)*
a. NUMBER 8 1 . 0 4 9
MULTIPLE ☐
b. TITLE
Basic Energy Sciences

7. TITLE OF APPLICANT'S PROJECT (Use section IV of this form to provide a summary description of the project.)
The Behavior of Electrochemically Compressed Hydrogen and Deuterium

8. TYPE OF APPLICANT/RECIPIENT
A—State
B—Interstate
C—Substate Organization
D—County
E—City
F—School District
G—Special Purpose District
H—Community Action Agency
I—Higher Educational Institution
J—Indian Tribe
K—Other *(Specify)*:
Enter appropriate letter I

9. AREA OF PROJECT IMPACT *(Names of cities, counties, states, etc.)*
Salt Lake City

10. ESTIMATED NUMBER OF PERSONS BENEFITING
5

11. TYPE OF ASSISTANCE
A—Basic Grant
B—Supplemental Grant
C—Loan
D—Insurance
E—Other
Enter appropriate letter(s) A

12. PROPOSED FUNDING

a. FEDERAL	$ 656,162.00
b. APPLICANT	56,000.00
c. STATE	.00
d. LOCAL	.00
e. OTHER	.00
f. Total	$ 712,162.00

13. CONGRESSIONAL DISTRICTS OF:
a. APPLICANT Utah 2nd
b. PROJECT Utah 2nd

14. TYPE OF APPLICATION
A—New
B—Renewal
C—Revision
D—Continuation
E—Augmentation
Enter appropriate letter A

15. PROJECT START DATE *Year month day* 19 88 10 1

16. PROJECT DURATION 36 *Months*

17. TYPE OF CHANGE *(For 14c or 14e)*
A—Increase Dollars
B—Decrease Dollars
C—Increase Duration
D—Decrease Duration
E—Cancellation
F—Other *(Specify)*:
Enter appropriate letter(s)

18. DATE DUE TO FEDERAL AGENCY ► *Year month day* 19 89 2 27

19. FEDERAL AGENCY TO RECEIVE REQUEST DOE Office of Energy Research
a. ORGANIZATIONAL UNIT (IF APPROPRIATE) Office of Energy Research
b. ADMINISTRATIVE CONTACT (IF KNOWN) Ryszard Gajewski
c. ADDRESS Acquisition and Assistance Management Division
Office of Energy Research ER64 DOE, Washington, DC 20545

20. EXISTING FEDERAL GRANT IDENTIFICATION NUMBER

21. REMARKS ADDED ☐ Yes ☒ No

SECTION I—APPLICANT/RECIPIENT DATA

22. THE APPLICANT CERTIFIES THAT► To the best of my knowledge and belief, data in this preapplication/application are true and correct, the document has been duly authorized by the governing body of the applicant and the applicant will comply with the attached assurances if the assistance is approved.

a. YES, THIS NOTICE OF INTENT/PREAPPLICATION/APPLICATION WAS MADE AVAILABLE TO THE STATE EXECUTIVE ORDER 12372 PROCESS FOR REVIEW ON:
DATE ____________

b. NO, PROGRAM IS NOT COVERED BY E.O. 12372 ☐
OR PROGRAM HAS NOT BEEN SELECTED BY STATE FOR REVIEW ☐

23. CERTIFYING REPRESENTATIVE
a. TYPED NAME AND TITLE
James J. Brophy
Vice President for Research
b. SIGNATURE James J Brophy

SECTION II—CERTIFICATION

24. APPLICATION RECEIVED 19 *Year month day*

25. FEDERAL APPLICATION IDENTIFICATION NUMBER

26. FEDERAL GRANT IDENTIFICATION

27. ACTION TAKEN
☐ a. AWARDED
☐ b. REJECTED
☐ c. RETURNED FOR AMENDMENT
☐ d. RETURNED FOR E.O. 12372 SUBMISSION BY APPLICANT TO STATE
☐ e. DEFERRED
☐ f. WITHDRAWN

28. FUNDING

a. FEDERAL	$ .00
b. APPLICANT	.00
c. STATE	.00
d. LOCAL	.00
e. OTHER	.00
f. TOTAL	$ .00

29. ACTION DATE► 19 *Year month day*

30. STARTING DATE 19 *Year month date*

31. CONTACT FOR ADDITIONAL INFORMATION *(Name and telephone number)*

32. ENDING DATE 19 *Year month date*

33. REMARKS ADDED ☐ Yes ☐ No

SECTION III—FEDERAL AGENCY ACTION

NSN 7540-01-008-8162
PREVIOUS EDITION IS NOT USABLE

424-103

STANDARD FORM 424 PAGE 1 (Rev. 4-84)
Prescribed by OMB Circular A-102

ER F 4620.1
(7-85)

U.S. Department of Energy

Grant Application Budget Period Summary

(See Reverse for Definitions and Instructions)

OMB Approval
No. 1910-1400

Please Print or Type

Organization: University of Utah	Period Covering: From: 10/1/88	FOR DOE USE ONLY Proposal No:
Principal Investigator (P.I.)/Project Director (P.D.): Stanley Pons	To: 3/31/90	Award No.:

A. SENIOR PERSONNEL PI/PD Co PIs, Faculty and Other Senior Associates (List each separately with title, A.6 show number in brackets. Attach separate sheet, if required.)	DOE Funded Person-Mos. Cal.	Acad.	Sumr	Funds Requested By Applicant $	
1. Martin Fleischmann, Co PI, Professor	6			25,000	
2. No Employee Benefits/From England					
3.					
4.					
5.					
6. (1) TOTAL SENIOR PERSONNEL					
B. OTHER PERSONNEL (SHOW NUMBERS IN BRACKETS)					
1. (1) POST DOCTORAL ASSOCIATES	18			30,000	
2. (1) OTHER PROFESSIONALS (TECHNICIAN, PROGRAMMER, ETC.)	18			27,000	
3. (2) GRADUATE STUDENTS	18			31,000	
4. ( ) UNDERGRADUATE STUDENTS					
5. ( ) SECRETARIAL-CLERICAL					
6. ( ) OTHER					
TOTAL SALARIES AND WAGES (A + B)				113,000	
C. FRINGE BENEFITS (IF CHARGED AS DIRECT COSTS) Postdoc 14%; Tech. 30%; Grad. St. 8%				14,780	
TOTAL SALARIES, WAGES AND FRINGE BENEFITS (A + B + C)				127,780	
D. EQUIPMENT (LIST ITEM AND DOLLAR AMOUNT FOR EACH ITEM) Calorimeter for temp. msmt.					28,500
3 Potentiostat-galvanostat-power for cells					17,940
1 Waveform generator for potential programs					5,790
Temp trandlucers, PC for control and recording					11,000
TOTAL EQUIPMENT				63,230	
E. TRAVEL 1. DOMESTIC (INCL. CANADA AND U.S. POSSESSIONS)					
2. FOREIGN Fleischmann to Utah to participate in work				4,000	
F. OTHER DIRECT COSTS					
1. MATERIALS AND SUPPLIES Rods, heavy water, other metals				73,750	
2. PUBLICATION COSTS/PAGE CHARGES				750	
3. CONSULTANT SERVICES					
4. COMPUTER (ADPE) SERVICES					
5. CONTRACTS AND SUBGRANTS					
6. OTHER					
TOTAL OTHER DIRECT COSTS				74,500	
G. TOTAL DIRECT COSTS (A THROUGH F)				269,510	
H. INDIRECT COSTS (SPECIFY RATE AND BASE) 47% Direct costs, except equipment					
TOTAL INDIRECT COSTS				96,952	
I. TOTAL DIRECT AND INDIRECT COSTS (G & H)				366,462	
J. APPLICANT'S COST SHARING (IF ANY)				44,000	
K. TOTAL AMOUNT OF THIS REQUEST (ITEM I LESS ITEM J)				322,462	

For Further Information, Other Than
Technical Contact Richard H. Timpson
Sponsored Projects 801-581-6903

PI/PD TYPED NAME & SIGNATURE Stanley Pons	DATE 2/27/89
INST. REP. TYPED NAME & SIGNATURE James J Brophy JAMES J. BROPHY Vice President For Research	DATE FEB 27 [illegible]

Equipment Justification

Potentiostat-galvanostats are instruments used for accurately controlling the potential and/or current applied between the electrodes in the cell. There will be several of these operating at the same time, and for extended periods of time. We have requested three of these to control three cells simultaneously. Suitable instruments cost $5,980 each.

The calorimeter setup requested will consist of glass evacuated dewar type cells to contain the rods, counter electrodes, and solutions; two constant temperature baths to hold the cells; accurate thermistors and voltmeters to monitor relative changes in the temperature of the cells and bath; and a scintillation counter to monitor the changes in the tritium content of the dewars. The cost of these components is $28,500.

A waveform generator is requested to drive the potentiostats above when applying potential programs to the experiments. This device will be required for experiments dealing with the determination of the heavy water equivalent of each cell. These experiments require precise timing of applied voltage/current levels. The cost for a suitable instrument is $5,790.

A personal computer is requested for recording of the various variables in the experiments: cell current, cell voltage, applied voltage, bath temperature, dewar temperature, and scintillation counts for blanks, controls, and dewars. The device will also be used for calculation and plotting of the cooling curves and thermal equivalents, as well as general calculations. Interfaces for the various transducers (A/D converters; suitable bus configuration), extended memory, large hard disk, and a printer output are required. The components cost $11,000.

Travel Justification

Professor Fleischmann intends to travel from the University of Southampton, Southampton, UK, to London to Salt Lake, and return, three times during the first eighteen months of the project. Travel to London return is under $100, and airfare (return) from Gatwick to Salt Lake either on Delta or British Air-Continental is estimated at $1200. Professor Pons will be responsible for local expenses in Salt Lake City.

Materials and Supplies

The metal rod electrodes are to be purchased in 10 to 20 cm lengths and in diameters from 1 to 30 mm. High purity metals are required. High purity deuterium oxide is used as the solvent and fuel. We estimate that we will require 20 kG during the first year. Platinum wire will be used as the counter electrode in each cell. Each cell requires approximately 5 feet of wire. In addition, there will be Pt supports, framing, and wire necessary for the neutron counting experiment, as well as a 50 x 50 x 1 mm Pd sheet as the working electrode. The costs for primary electrode metals will be $51,000, platinum $7,750, and deuterium oxide $15,000.

ER F 4620.1A
(7-85)

U.S. Department of Energy

GRANT APPLICATION
PROJECT PERIOD SUMMARY

(Must be completed for all new and renewal applications.)

Please Print or Type

OMB Approval
No. 1910-1400

Categories	01 Budget Period	02 Budget Period	03 Budget Period	04 Budget Period	05 Budget Period
A. Senior Personnel Totals	25,000	29,000			
B. Other Personnel Totals	88,000	92,000			
C. Fringe Benefit Totals	14,780	15,660			
Total of A, B & C	127,780	136,660			
D. Equipment	63,230	22,800			
E. Travel 1. Domestic					
2. Foreign	4,000	5,000			
F. Other Direct Costs	74,500	78,000			
G. Total Direct Costs	269,510	242,460			
H. Total Indirect Costs	96,952	103,240			
I. Total Direct & Indirect Costs	366,462	345,700			
J. Applicant's Cost-Sharing (If any)	44,000	12,000			
K. Total Amount of Request (Item I. Less Item J.)	(1)* 322,462	(2) 333,700	(3)	(4)	(5)

*This should equal item K on Budget Period Summary (ER/F/4620.1)

ESTIMATE

TOTAL COST OF PROJECT

$ 656,162

(add K(1) thru (5))

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

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

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

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

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.

J00490

002624

SHARP FO-700
(801) 378-2800

BRIGHAM YOUNG UNIVERSITY, PROVO, UTAH
Communication Systems (801) 378-7311

TO: Ryszard Gajewski,
~~ER-16~~
DOE

( ) FACSIMILE NUMBER
Country Code:
City Code:
Number: 301-353-387

FROM: S E Jones
BYU

Phone (801) 378-2749

COMMENTS:

MESSAGE CENTER

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CONTACTED	Renda			
DATE/TIME	6) 10:35			
OPR. INI	mn.			

Sending Instructions: ( ) RUSH ( ) CONFIDENTIAL ( ) SEND ACKNOWLEDGEMENT
Additional Message:

For office use only - DO NOT write on this portion of the form

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2916 FS

Billing Information:

Number of pages: 3

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Amount: $

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Received by:

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/fluxes 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).

THE UNIVERSITY OF ARIZONA
TUCSON, ARIZONA 85721 USA

COLLEGE OF ARTS AND SCIENCES
FACULTY OF SCIENCE
DEPARTMENT OF PHYSICS
BUILDING #81
(602) 621-6820

Reviewer #2

Dr. R. Gajewski
Department of Energy
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16
Washington, DC 20545

October 4, 1988

Dear Dr. Gajewski:

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.

I trust that this report will be of help. Given the postal delay incurred on reception of your mail in Arizona, I am forwarding an advance copy of this report by FAX.

Yours Sincerely

Jan Rafelski
Professor of Physics

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

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: <u>what</u> 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.

THE UNIVERSITY OF ARIZONA
TUCSON, ARIZONA 85721 USA

COLLEGE OF ARTS AND SCIENCES
FACULTY OF SCIENCE
DEPARTMENT OF PHYSICS
BUILDING #81
(602) 621-6820

Dr. R. Gajewski
Department of Energy
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16
Washington, DC 20545

November 24, 1988

Dear Ryszard:

RE: Proposal of Dr. Pons "The Behavior of Electrochemically compressed Hydrogen and Deuterium"

Here: Reply to my (reviewer #2) comments:

I have considered carefully the rebuttal of Dr. Pons to my review. In my opinion the material submitted does not offer clarification of specific points I requested in my review.

As to my point 1), the rebuttal does not offer any professional background for the estimate of the range of detectable fusion rates, which are restated as given in my review. Dr. Pons does not address in a specific manner (see below) the question how such a nuclear rate can be measured by identifiable nuclear observables. Let me illustrate the gravity of the problem by noting that fusion rate of 10^{-16}/s implies that even in 4 months, that is in 10^7s (not 75, 155 or 101 hours) only a 10^{-9} fraction of all atoms in the Dewar would undergo a reaction and even if all reactions would produce tritium, such a small concentration would probably be below his background level of tritium in the deuterium used. On the other hand it is extremely difficult, if not impossible, to directly observe tritium as fusion product, and one has to look at the accumulated concentration in the set up envisaged by Dr. Pons.

Thus the one method proposed and only vaguely outlined how to diagnose the reactions will not work at the level needed to match the sensitivity of the calorimetric measurement. But in my opinion there are many ways this problem can be solved. Even with the fusion rate of 10^{-20}/s there would be about 10^4 reactions per second, plenty to observe with help of specific detectors the products of direct nuclear reactions. In my opinion nuclear detection methods are much more sensitive than the calorimetric methods, if dealt with appropriately.

In response to my point 2. Dr. Pons refers in his reply in very general terms to gamma rays, thermal neutrons and tritium as the means of understanding of the specific origin of the excess heat, if such is observed.
"gamma rays"
In which energy range, and in particular from which nuclear fusion reactions are these expected. Note that normally gamma rays are considerably less abundant than other nuclear reaction products, except for a few exceptional cases, with well known energy. Will the considerably smaller gamma rate be at all observable? And how?
"thermal neutrons"
It appears that Dr. Pons has not considered the fact that in his experimental arrangement in case nuclear reaction occur, he will not have to deal with "thermal neutrons" but with energetic reaction products which carry the considerable nuclear energy released.
"tritium"
Where does tritium come from, why should it be the product of nuclear fusion reaction that has yet to be discovered, and finally why to look for this extremely rare and elusive product of nuclear reactions (see above).

Aside from faulty and/or incomplete responses to my specific two requests, I do not see in particular a survey which would list those nuclear reactions that are possible and a proposal how to approach their identification in any specific way. There is a very incomplete list on page 8 of the proposal which surprisingly includes secondary reactions induced by neutrons. Indeed, the vague mention of tritium means presumably that Dr. Pons proposes to follow up the possibility of d-d fusion (see page 2 of proposal) as to my knowledge only in this primary fusion reaction there is an appreciable branching ratio to tritium. But ^{3}He produced equally abundantly in this reaction, is a much better isotope to use as tag for this reaction...Tritium is also produced in the above mentioned secondary Li-n reactions, but neutrons have to be produced in the first place in a nuclear reactions, hence it would be wiser to look for them, rather than for a secondary and rather elusive reaction product.

All this means that:
A) the nuclear part of the proposal has not been seriously addressed;
B) there is extremely limited expertise in the field of nuclear reactions.
These observations are further supported by the paragraphs from the rebuttal to the observation of the reviewer #3 pertinent to the dangers of increased background radiation.

Dr. Pons missed the opportunity to respond in an accurate and expert fashion. I conclude with near certainty that nothing will come out of the proposed diagnosis of the specific origin of the excess heat, should the latter be indeed found. However, I consider this as the most worthwhile part of the proposed research program. In my opinion mere calorimetric reconfirmation of the excess heat generation leads us nowhere. I therefore do not recommend the funding of this project.

Yours Sincerely

Jan Rafelski
Professor of Physics

Cornell University

Laboratory of Atomic
and Solid State Physics

Clark Hall
Ithaca, NY 14853-2501

Telex WUI6713054

October 12, 1988

Dr. Ryszard Gajewski
Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16
Department of Energy
Washington, DC 20545

<u>Re:</u> Pons/Fleischmann Proposal

Dear Dr. Gajewski,

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 <u>solid</u> 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: <u>what</u> radiation? Why wasn't this followed up by the University safety people?

I don't think you should proceed with this.

Yours sincerely,

Neil Ashcroft

Neil W. Ashcroft
Professor of Physics

NWA:ksl

Enclosure

Reviewer #3

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

Enclosures

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.

Cornell University

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and Solid State Physics

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Telex WUI6713054

November 23, 1988

Dr. Ryszard Gajewski
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16
Washington, DC 20545

Re: Pons/Fleischmann Proposal

Dear Dr. Gajewski,

Thank you for your letter and the (somewhat revised) proposal by Pons and Fleischmann.

I have not changed my opinion and I will take up the rebuttals one by one.

#1. The authors have forgotten their elementary chemistry. In particular they need to be reminded of the cusp theorem. The idea that deuterium loses its electron to the d-band of palladium is very naive. It's a rigorous theorem that the gradient of the electronic charge density at the deuteron nucleus is proportional to the electron density itself (at the same position). Since this density is not very different in Pd-D from pure solid deuterium, then by a Heitler-London argument, the interactions controlling the collisions between deuterons in Pd-D will likewise not be very different from the solid deuteruim case. Differences can certainly be expected at long range, but this is irrelevant from the standpoint of the present proposal. If the authors do not believe this, they might instead consider doing a little homework: screened point ion potentials appropriate to metallic environments are readily available in the literature (even for hydrogen). If they think the electrons weaken the potential in the region that matters, they should think again.

#2. The muon through its mass presents a favorable length scale for deuteron-deuteruim collisions. The authors in their last proposal were implying that electron-screening would achieve the same purpose. They still hold to this view, as they say in the abstract, and the argument is specious for the reasons given above.

#3. The previous proposal had very little discussion on important experimental details. In spite of the figures given, I remain dubious. Was any attempt made to verify that the sample remained in the same bulk phase? Is electromigration a problem? Is the temperature dependence of C sufficiently small that equation (5) follows accurately from (3)?

General Remark:

It is very important to support speculative research, provided there's some physical basis to the speculation. In my mind, the authors have presented no such argument. I would be willing to consider this proposal further if the authors will produce a microscopic estimate that would demonstrate in this alloy (and under conditions that are quite typical of condensed matter physics) a high likelihood of the close deuteron encounters that are necessary to fusion. I emphasize the word alloy.

Again, I do not think the proposal should be supported.

Yours sincerely,

NW Ashcroft
Neil W. Ashcroft

P.S. You might seek the advice of a metal hydride physicist, for example, Prof. R. Barnes, Ames Laboratory, Iowa State University, Des Moines.

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)

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

October 8, 1988

Reviewer #4

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

Enclosures

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.

Department of Energy
Washington, DC 20545

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

Enclosures

11-21-88

There is no controversy or discrepancy between my original report and the authors' response. I stand by my original recommendation.

Falicov

L. M. Falicov
Prof. of Physics

-ard Gajewski - Department of Energy

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.

Reviewer #5

Theodore R. Beck

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

Enclosures

Reply to reviewer #5

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

(1) and (2) (in part). These points are covered by our reply to question #6 of Reviewer #1. 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.

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.

December 8, 1988

Professor Stanley Pons
Department of Chemistry
University of Utah
Salt Lake City, UT 84112

Dear Professor Pons:

Following yesterday morning's telephone conversation, I am enclosing copies of the reviewers' responses to your and Professor Fleischmann's rebuttals.

My reading of the situation is that the project can be allowed to proceed only if a credible capability is established to diagnose the products of the suspected nuclear reactions.

Please let me know your thoughts on this subject.

Sincerely,

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

Enclosures

DEPARTMENT OF CHEMISTRY
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PROFESSOR MARTIN FLEISCHMANN, F.R.S.

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Dr. R. Gajewski,
Director,
Division of Advanced Energy Projects,
Office of Basic Energy Sciences, ER-16,
Department of Energy,
Washington, DC 20545
U.S.A.

MF/KJW

20 December 1988

Dear Dr. Gajewski,

As you will see I am at present in Southampton and Stan Pons has sent on to me a copy of your letter of 8th December enclosing the further comments from the reviewers # 1 to 5 of our research proposal. I was relieved to see from your letter that this project may be able to go ahead provided we can establish means of credibly diagnosing products of the suspected nuclear reactions. I know that Professor Pons is actively making arrangements to this end and no doubt he is corresponding with you about this question. However, I have been so disturbed by the nature and tenor of some of the comments made by some of the reviewers that I feel compelled to write to you further in part to seek to "set the record straight" but mainly to seek to correct the impression made by these reviewers. As I hope to show you, if we were to follow their advice then this would impose an unwarranted and untenable bias to our work. I shall be sending this letter to you via Stan Pons as he may wish to comment further on some of the points I have made. It may well be also, that you will wish to send some of these comments to the reviewers in which case I may want to tone down some of the remarks!

I now realise that there has been a major lack of communication between us and some of the reviewers (notably # 2 and 3 and to a lesser extent # 1). This has been due in part to the rather outrageous character of our proposal which has made us reluctant to make definite statements: it is really still necessary to make exploratory measurements. It is also largely my fault in that I persuaded Professor Pons that we should submit a short proposal - I should explain to you that we are here restricted to six pages and discouraged from giving a great deal of background material which obscures the main objectives of the research programme. Be that as it may it is evident that these reviewers have not understood the restrictions on the diagnosis of the nuclear reactions posed by the nature of the electrochemical techniques and in consequence have failed to understand the physics of the problem. This has led them to place a quite unwarranted emphasis on energy discriminative neutron detection (and, I suspect, on the search for ^{3}He). We have considered most of these points (and many others) in designing the experiments outlined in our proposal and I think it is therefore desirable that I should outline some of these considerations to you in replying to the reviewers' second set of comments. However, to

/continued...

telescope my replies: may I point out to you that the dimensions of our cells lie between that of the mean free path of the neutrons and the thermalisation distance. The information content in energy discriminative neutron analysis will therefore be lost. It seems to us more sensible therefore to take the opposite approach namely to increase the dimensions of the experiment and to look for the secondary reactions induced by thermalised and, maybe, fast neutrons.

I would like to re-emphasise that the major objective of our proposal is to investigate whether we can create conditions by simple, essentially chemical, means to generate highly compressed D^+ such that there will be <u>significant</u> thermonuclear fusion. I have underlined significant because we would be quite content to demonstrate unequivocally the generation of excess enthalpy with appropriate <u>supporting</u> evidence for the generation of neutrons and γ-rays. To this end we must investigate the effects of the dimensions of the electrodes and cells, the thermal balances, the effects of electrode potential and temperature, solution composition (including isotopic composition), the effects of electrode material and surface poisoning etc. etc. All this information will be required to evaluate our results and we will be fully stretched to meet these objectives. By contrast some of the reviewers want us to concentrate on the nuclear physics aspects and seem to believe that the demonstration of $E = mc^2$ does not provide a sufficient objective on that score! To follow some of these objectives we would need to institute experiments, which require high vacuum techniques, high resolution mass spectroscopy, energy discriminative neutron and γ-ray detection etc. etc. Our proposal is not costed to achieve these objectives and, as I point out in the detailed comments nor is any conceivable experiment design (apart from energy discriminative γ-ray). It would seem more logical for us to make any supporting measurements using other peoples' instrumentation (such as that of Professor Steven Jones at Brigham Young University).

The situation with regard to the reviewers is really worse than I have outlined: in order to make their point the reviewers quote parameters which do not apply to our experiments (e.g. reviewer # 2 talks of fusion rates of 10^{-20} s^{-1} when we believe that we had 3×10^{-14} s^{-1} and are aiming to achieve say 3×10^{-12} s^{-1} in this next phase. Where is the sense in such comments?). Furthermore, they cast aspersions on our competence to make relatively straightforward measurements. Let me assure you that we do know how to make such measurements and, if we don't. whom to ask. A long time ago I supervised a small radiochemical laboratory: the instrumentation may have changed but the principles have not. More recently I have been concerned with single photon counting techniques (as has Professor Pons) and I have developed the use of position sensitive single photon counting X-ray detection methods in surface chemical problems (including the construction of the detectors), work which has been supported by the U.S. Office of Naval Research. I am well aware of the application of closely related techniques in fusion research. Furthermore I was a consultant at the United Kingdom Atomic Energy Authority (UKAEA) Winfrith Heath Laboratories in the days when the Steam Generating Heavy Water (SGHW) programme was in full swing.

/continued...

The team there has now been dispersed. At present I consult at UKAEA, Harwell, on matters not related to the substance of our proposal but, inevitably, one acquires the relevant background. I have told you all this because I take a dim view of the tenor of some of the remarks.

Although I now seem to be erring on the side of length rather than brevity, I feel it is necessary to reply in detail to the reviewers. As some of these replies to specific reviewers cut across the comments made by some of the others, I will number the comments and will indicate the origin of the reviewers' first comments by: # reviewer, lines a-b; our replies by: reply lines c-d; the reviewers' second sets of comments by: # reviewer lines e-f and my further comments by: reply lines g-h. This will allow me to cross refer where necessary.

Yours sincerely,

Martin Fleischmann.

Martin Fleischmann

P.S. Having just completed the attached set of comments, I inevitably find that I have been driven into making rather strong statements about the interpretation of our preliminary sets of experiments. I feel that I should at the end of this letter seek to correct this impression. Professor Pons and I certainly still have many doubts about the feasibility of the project but, as some of the reviewers have said, the pay-off, if successful, would be so enormous that we do of course feel that we have to continue!

Comment 1
Reviewer # 1 lines 4-8
Reply lines 3-21

I believe that the reviewer now understands the logic behind our experiment design and, indeed, we had thought it likely that he is Professor Steven Jones. Right at the start of our project Professor Pons and I discussed its possible relevance to muon catalysed fusion research and, subsequently, we have repeatedly considered contacting Professor Jones. The reason we have not done so before now is that we could not decide whether highly compressed D^+ would offer advantages in this field - but we may be quite mistaken on this score alone. However, it now appears that Professor Jones' interests extend beyond this field and it may well be of benefit to all of us to collaborate on various aspects of these research programmes.

Reviewer # 1 lines 41-46
Reply lines 108-111
Reviews # 1 further comments
lines 23-45

I hope that you will not mind if I make a critical comment on some of the reviews: I wish that Professor Jones had revealed his identity in his first set of comments. It is bad reviewing practice (for proposals as for papers) to introduce new material in replies to a response. This remark applies equally to the second sets of comments of some of the other reviewers.

We are well aware of the formation of inert gas bubbles in metals. Indeed our knowledge of the formation of such bubbles (and, as I have pointed out in my letter, I consult for UKAEA) was part of the background knowledge which led to the Pd/D project.

Reply line 80

As we pointed out in our reply, line 80, we intend to search for He bubbles but the search for ^{3}He cannot be a primary line of attack for reasons which are outlined in comment 3 in response to reviewer # 2.

Comment 2
Reviewer # 1 lines 9-20
Reply lines 22-47
Reviewer # 1 further
comments lines 3-17

Quite frankly, I cannot see how Professor Jones can make this comment. He (and some of the other reviewers) seem to be unaware that D_2O has been extensively used as a moderator in much European fission research. As I have pointed out in my letter, the dimensions of our electrodes + electrolysis cell lie between the mean free path and the thermalisation distance for fast neutrons. The characterisation of neutrons of a particular energy cannot therefore be used as a primary research technique. I am sure that Professor Jones and some of the other reviewers will believe that it would be possible to modify the experiments to give a configuration in which there will be little energy loss of the primary neutrons but if we were to do this we will probably switch off the fusion reaction. To forestall further discussion on this point let me outline a typical experiment which we have considered. In order to approach the typical thin film configuration favoured in nuclear physics, we could use the electrochemical version of the Pd-diffusion tube in which D_2O would be electrolysed on the inside of a thin tube and diffuse to the outer surface and this tube would be used as the source. At least half of any neutrons generated would then reach the detector without loss of energy. However, in any such configuration, the chemical potential of the dissolved D will drop markedly because the system is not now in equilibrium. It was established already before the second world war that the boundary condition on the ingoing interface is of the type

$$-D\left(\frac{\partial C_b}{\partial x}\right)_{x=o} = k_1C_s - k_2C_b$$

where D_D is the diffusion coefficient in the lattice, C_b is the concentration of D in the Pd, C_s the concentration on the surface and k is a rate constant i.e. we have the diffusion analogue of the radiation boundary condition of heat conduction (the flux is taken in the direction x positive). There is therefore a marked discontinuity at the interface and this will cause a marked fall of the chemical potential. A negative result of such an experiment therefore could not be taken to predict a negative result for the experiments outlined in our proposal. I am sure you will understand that we do not wish to make our research strategy dependent on experiments whose interpretation we can judge to be at best ambiguous but we do of course intend to make such measurements to support those which we have outlined in the proposal. Perhaps I should also add that supported films of Pd will not be usable as they will certainly detach due to formation of bubbles at the Pd/support interface.

We do of course know the reactive penetrating power of α, ß, and γ-radiation of a given energy and we are well aware of proper shielding procedures (please see my covering letter - I would add that some of our experiments require shielding at least as demanding as that in nuclear physics and chemistry). The reason we referred to (ß + γ) radiation is simply that the detector we used measured such combined radiation. The reviewers should bear in mind that the preliminary experiments we reported were carried out with zero funding!

Furthermore, Professor Jones will see from our proposal and our reply that: (a) there is no H in the experiments we propose for the major part of the work so that the generation of γ-rays by proton-deuteron fusion cannot be observed. As pointed out in our first reply to reviewer # 5, we intend to

Reply to reviewer #5
lines 5-8

make measurements in systems containing H alone and we will certainly include measurements on mixed H/D systems. (b) that we propose to use energy discriminative γ-ray analysis. If on grounds of safety alone, the starting point for our experiments requires that we should increase the dimensions of the electrodes + electrochemical cells to thermalise the neutrons and maintain the cells in a large water bath to absorb the neutrons. It therefore seems more sensible to us to look for the secondary reactions of slow neutrons (and, maybe, of fast neutrons) by using energy discriminative γ-ray analysis rather than to concentrate on the characterisation of the primary neutron flux. As we have also pointed out, we shall add appropriate electrolytes to the Dewar and/or the water bath to look for such secondary reactions.

Reviewer # 1 further
comments lines 13-17

Why? As I have pointed out (and elsewhere, see comment 3) the characterisation of the energetic neutrons and of the $^3He/^4He$ ratio will be difficult and expensive and could be inconclusive. At the end of such a programme we would still have to carry out the project which we have outlined so why not do it our way? If it turns out that we are correct in our assignment of the excess enthalpy to nuclear fusion than our own programme will give most of the information required for any further work on this topic and, we submit, will give all the information required for an unequivocal assignment of the nuclear reactions involved.

Comment 3
Reviewer # 2 lines 9-21 and
lines 41-42
Reply lines 11-19
Reply to reviewer # 3 lines 14-20
Reviewer # 2 further comments
lines 7-9

I simply do not know how to reply in a sensible way to the further comments made by this reviewer nor, indeed, whether there is any point in doing so. I submit that any scientist reading his first set of comments would conclude that his question about the sensitivity of the calorimetric method we have used (and propose to use) was of primary concern to him. We have answered this but in his further comments he simply rejects this and says that we have quoted his figure given in the first set of comments. This is simply untrue - he stated that he believed that fusion rates as low as $10^{-16}\ s^{-1}$ would be detectable whereas we told him that our calculations showed that $3 \times 10^{-16}\ s^{-1}$ would have been measurable in our apparatus and using particular measurement techniques. Moreover, we gave him values of actual temperature differences measured while typical values of the Heavy Water Equivalent of the Dewar + contents (292.7 g D_2O) and of the cooling rate $(0.3113\ J\ s^{-1}\ (^\circ)^{-1}$ were quoted in the proposal. Surely, it is too straightforward a matter to multiply the latter figure by the temperature difference in order to get the rate of cooling to warrant an extensive discussion of such a trivial point? Subtraction of the rate of Joule heating gives the excess rate of enthalpy production which can be equated to the enthalpy production in nuclear fusion. It is this which gives the observed rate $3 \times 10^{-14}\ s^{-1}$. Temperature differences two orders of magnitude smaller than the ones we observed would certainly be measurable and if the joule heating were reduced proportionately, we would therefore be able to measure rates as low as 3×10^{-16} s^{-1}. Increase of the dimensions of the Pd electrode and reduction of the electrolyte volume would give a further order of magnitude without changes in the technique. Improved construction of the Dewar (better vacuum, silvering of the inside surfaces) and tracking of the Dewar temperature by that of the water bath would certainly give one further order of magnitude. However, this is all really beside the point because _if_ we were correct in attributing the excess enthalpy to nuclear fusion, then we are certainly _not_ dealing with such low fusion rates. Indeed our objective is to raise these rates by perhaps two orders of magnitude in the next phase of the work.

Reviewer # 2 lines 32-46
Reply to reviewer # 1 lines 68-90
Reply to reviewer #3 lines 14-20

One would conclude that the question of other sources of energy was the second matter of concern to reviewer # 2. We have answered this (as far as we can at the present stage) but the reviewer does not state whether he is satisfied by our answer.

Reviewer # 2 lines 43-44
Reply lines 31-34
Reviewer # 2 further comments
lines 9-58

I submit that anyone reading the reviewers first set of comments would conclude that he wanted a short and general statement about the type of radiation to be measured. This we have done but the reviewer now introduces a great number of new objections. As I have already told you, I consider this to be unacceptable reviewing practice but I will nevertheless attempt to answer briefly the points made.

Thermal neutrons My comments 1 and 2 to reviewer # 1 are relevant to this point and to the rest of this section. Suffice it to say that the dimensions of our apparatus lie between those of the mean free path and the thermalisation distance. I must state again that diagnostic measurements on the primary neutrons generated in any nuclear reaction will be difficult or impossible. Redesign of the apparatus to allow such measurements may lead to cessation of any fusion reaction.

γ- rays As we shall therefore be dealing with low energy neutrons, there are many n-γ reactions which could be used to detect these neutrons. As we have pointed out, we can change the electrode material and, within certain restrictions, the electrolyte in the Dewar. We can also add suitable electrolytes to the surrounding water bath (quite apart from the n-γ reaction with H_2O). Diagnostic measurements with γ-rays are not particularly difficult and we have the relevant experience to develop such measurements.

Tritium and ^{3}He. It seems to have escaped the reviewers attention that the main substance of our proposal is to seek to induce D + D fusion! ^{3}He will be difficult to detect because of dominant D_2 and O_2 evolution in the cell (I have done mass spectroscopy in the past so I am well aware of what is involved). We have pointed out to reviewer # 1 that we may look for the bubbles but such a search will interrupt the measurements and may well prove to be inconclusive. By contrast T will inevitably accumulate in the electrolyte in the Dewar but why does the reviewer insist on basing his calculation on a fusion rate of 10^{-16} s^{-1} when we have told him that an interpretation of the excess enthalpy in our preliminary experiments in terms of fusion implies a rate of 3×10^{-14} s^{-1}? Elementary considerations show that the fraction α of T species will build up as

$$\alpha \approx \frac{S\beta}{R}\left(1 - \exp\left(-\frac{Rt}{SN}\right)\right)$$

where S is the separation factor for D in the D_2O/TDO mixture, β is the fusion rate (atoms of T s^{-1}), R is total rate of electrolysis of T and D (atoms s^{-1}), N is the total number of atoms of T and D in the system and t is the time (s). In the previous experiments the characteristic time SN/R would have been > 100 days and the final concentration of T $\sim 4.4 \times 10^{-7}$ molar (certainly measurable but requiring long term experiments). The initial build would have been $\sim 4.5 \times 10^{-9}$ molar day^{-1} (admittedly difficult to measure). The redesign of the experiments should allow a reduction of SN/R certainly to 10 days (following saturation of the electrodes) an increase of the final concentration of T and in consequence of these two factors an increase in the rate of build up of T.

Reviewer # 2 further comments
lines 65-71

Professor Pons and I would have been quite content to produce an unequivocal demonstration that the excess energy is due to D + D fusion and this demonstration will certainly require the resources we have asked for. If we spend an appreciable part of our time and resources on energy discriminative neutron analysis, then we shall find it difficult to delineate the scope of the problem.

Comment 4
Reviewer # 3 lines 1-5
Reply lines 2-9
Reviewer # 3 further comments
lines 5-17

Granted, but the reviewer also has to be reminded about elementary chemistry. It is well known that the electron density at nuclei is determined by the s-electron wave functions as is fully established in chemistry by inter alia the hyperfine splitting of ESR spectra (controlled by the Fermi contact potentials), spin-spin coupling in NMR spectra, chemical applications of Mössbauer spectroscopy. If the contributions by these wave functions were dominant then D_2 would have to form. However, D_2 is not formed but the reviewer refuses to answer how this can be so on the basis of his argument.

Perhaps I should explain that my personal knowledge of the behaviour of D^+ (or H^+) in βPd-D is in part based on work on the electrolytic separation factors of H/D between HDO/D_2O and ßPd-H/D. These can only be explained by assuming that the H or D species in the lattice behave as classical vibrators, a situation radically different to that in solid hydrogen or deuterium i.e. the species behave as though they are in a very shallow potential well. You will appreciate that our knowledge of this behaviour had a considerable bearing on the research programme we submitted!

We made the great mistake of talking out the d-bands of Pd in our reply when we were much more cautious in our proposal - and caution is indicated! The collisions of D^+ in the lattice are really a supreme example of non-adiabatic, non-Born Oppenheimer processes in which, moreover, nuclear forces must be included. Realistic calculations would be difficult (at least for us) and are unlikely to be definitive. In his General Remark the reviewer states that the demonstration of a high likelihood of deuteron encounters is a prerequisite for any funding (has he been concerned with calculations on H-bombs?). However, such a calculation would not have to demonstrate a high likelihood but a sufficiently high but, in absolute terms, low likelihood. This is obviously difficult.

Reviewer # 3 lines 5-9
Reply lines 10-14
Reviewer # 3 further comments
lines 18-21

I do not really wish to point out yet again that we do not wish to draw an analogy with muon fusion.

Reviewer # 3 lines 10-17 Reply lines 13-27
Reviewer # 3 further comments
lines 22-26

It seems to me that the reviewer now accepts in part that we observed extra enthalpy generation. Electromigration is not a problem because the electrodes are charged to equilibrium; the effects of variations of C with T will also be small as the temperature differences are themselves small. The question of the maintenance of the same bulk phase is a very important point. To date nobody has discovered a phase other than ß-Pd-D either from electrochemical or X-ray diffraction measurements in these potential regions. We hope to make some supporting measurements by in-situ X-ray diffraction on the highly charged Pd-D electrodes. I have made similar measurements on ß-NiH in the past.

Comment 5
Reviewer # 4

We have no disagreements evidently.

Comment 6
Reviewer # 5 further comments
lines 1-4

I am glad to see that the reviewer appreciates that it is impossible to give other than preliminary answers at this stage. It is quite impossible at the present time to complete adequate preliminary experiments before applying for funding especially in the case of a radically new project.

lines 5-9

We will attempt to make such calculations using plausible parameters. However, to the best of our knowledge, the required thermodynamic data are not available and they will be very difficult to determine since we are dealing with systems very far from equilibrium.

THE
UNIVERSITY
OF UTAH

Dr. R. Gajewski
Director, DAEP
Office of Basic Energy Sciences, ER-16
Department of Energy
Washington, DC 20545

January 23, 1989

Dear Dr. Gajewski:

As per our recent conversations, and after further considerations, I am enclosing a statement of intent regarding the unambiguous detection of neutrons that may be generated in the experiments. In addition, I am enclosing the set of comments that I spoke to you about, that we wish to become part of the file. There are obviously several important points that we would like to be part of the record. If we have been overcautious in the proposal, it is simply because there are simply too many unknowns and too little data in the literature regarding the possible processes implied by the preliminary experiments; we hope to remedy this problem, and it will take a great deal of work! We look forward to working with you. Best regards.

Sincerely yours,

Stanley Pons
Professor and Chairman

SP/sdp

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

January 23, 1989

Additional comments to Pons, Fleischmann proposal.

Direct measurements of high energy neutrons formed in one of the possible reaction branches will be attempted by the use of a flat wall cell, going back to a plate type working electrode. The plate will be placed as close as possible to a flat thin glass optical window. The electrode will be surrounded by a platinum grid secondary electrode mounted on a thick platinum frame. This arrangement will give the highest exposure to any large, flat surface detector that may be used.

We have consulted with Professor Steven Jones who has described to us existing high energy neutron spectrometers that exist at Brigham Young University. These are based on lithium doped glass and various scintillators. There is ongoing research to improve the energy resolution; it is possible that these are the best spectrometers available. He has in addition offered to assist us in making the proposed measurements. Since the effective aperture of these spectrometers is at least as large as our entire cell, the efficiency of the measurements should be quite high. While there are numerous doped / activated inorganic salt and glass scintillator detectors known, the BYU group clearly has the expertise and facilities needed to make the required measurements in an unambiguous manner.