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

August 10, 2016

Re: OSTI-2016-01064-F

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

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

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

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

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

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

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

Sincerely,

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

October 3, 1988

#5

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

Dear Dr. Beck:

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

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

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

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MESSAGE CENTER

PACKAGES UNLIMITED
8215 LAKE VIEW BOULEVARD N.E.
SEATTLE WASHINGTON 98115
(206) 524-5721
FAX (206) 524-7341

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CONTACTED	Linda			
DATE/TIME	9/840			
OPR. INI	A			

FAX TRANSMITTAL REPORT

DATE: 9-30-88

TO: Ryszard Gajewski

TELEPHONE NO: FAX (301) 353-3870

FROM: T. R. Beck

TELEPHONE NO: (206) 285-7404

To Ryszard 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.

Theodore R. Bott

Department of Energy
Washington, DC 20545
September 9, 1988

RECEIVED
SEP 25 1988

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

Dear ~~Dr. Beck~~ Ted:

I am enclosing for your review a copy of the proposal entitled, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium," submitted to the Department of Energy's Division of Advanced Energy Projects by the University of Utah.

DOE regulations require that reviewers agree to: (1) return the proposal to us with the reviewer's comments; (2) use the information contained in the proposal for evaluation purposes only; and (3) treat such information in confidence. We shall assume that your proceeding with the review constitutes your agreement to comply with these requirements.

The programmatic objectives of the Division are briefly summarized in the enclosed sheet. You may find this information helpful in performing your review. Should you have any questions regarding the review, please feel free to call me at 301/353-5995.

Your willingness to help in the evaluation of the proposal is genuinely appreciated.

Sincerely,

Richard

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

Enclosures:
As stated

Dear Ryszard:

Enclosed is my review and the proposal.

Best regards,

Ted

REVIEWER #5

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Theodore R. Beck

#1

October 4, 1988

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

Dear Steve:

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

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

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

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REVIEWER #1

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

COMMENTS ON THE PROPOSAL

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

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

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

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

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

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

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

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

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

In concluaion, 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

October 5, 1988

#2

Professor Johann Rafelski
Department of Physics
Brigham Young University
Tucson, Arizona 85721

Dear Jan:

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

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

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

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DEPARTMENT OF PHYSICS
UNIVERSITY OF ARIZONA
Tucson, AZ 85721

Phone #: 602-621-6820 FAX #: 602-621-4721

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88 OCT 6 A7:42

TELEFAX TRANSMISSION

Date: 10/4/88 Time: 5pm

To: Dr R. GAJEWSKI
DOE - AEP
Washington DC 20545

Telephone #: 301-353 3870
TELEFAX #: 301-353 5995

Subject:

From: J. RAFELSKI
U-AZ, Physics

Telephone #: 602-6214212
TELEFAX #: 602-621 4721

Transmitted by: Confirming #: (602) 621-6616

Total Number of Pages: 3

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CONTACTED	Linda			
DATE/TIME	5/830			
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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

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

October 12, 1988

#4

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

Dear Professor Falicov:

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

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

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

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DATE: 10/11/88

FROM: Prof. L. W. T...

Physics D...
Univ... ...eley

...r Operator: (415) 642-7166

T... Dr. Barney
Department of Energy
Washington, DC

FAX: (301) 3... 870

No. of pages .. ver sheet

Remarks:

MESSAGE CENTER

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ONTACTED	Linda			
ATE/TIME	11-330			
PR INI	K			

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)

V. M. Yakovenko
Department of Physics
University of California, Berkeley, California
94720

October 8, 1988

REPORT MAILED BACK BY FIRST CLASS U.S. MAIL
ON SATURDAY OCTOBER 8, 1988

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

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

October 8, 1988

REVIEWER #4

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University of Utah
The Behavior of Electrochemically Compressed Hydrogen and Deuterium

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#3

October 12, 1988

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

Dear Professor Ashcroft:

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

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

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

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000933

TO: Dr. D. Barney
DOE
Washington, DC 20545

TELEFAX# 301-353-3370 (conf. #3486)

TELEPHONE# 301-353-5995

FROM: Professor Neil Ashcroft

TELEPHONE# (607) 255-4192

TELEFAX # 607-255-6428

CHARGE # U76-8496

MESSAGE CENTER

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CONTACTED	Linda			
DATE/TIME	12/215			
OPR. INI				

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

Re: 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 solid deuterium. It is a rather obvious test.

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

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

I don't think you should proceed with this.

Yours sincerely,

Neil Ashcroft

Neil W. Ashcroft
Professor of Physics

NWA:ksl

Enclosure

Cornell University

Laboratory of Atomic
and Solid State Physics

Clark Hall
Ithaca, NY 14853-2501

Telex WUI6713054

October 12, 1988

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

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Yours sincerely,

Neil Ashcroft

Neil W. Ashcroft
Professor of Physics

NWA:ksl

Enclosure