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

December 2, 1988

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

Dear Professor Ashcroft:

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

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

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

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Cornell University

Laboratory of Atomic
and Solid State Physics

Clark Hall
Ithaca, NY 14853-2501

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,

N W 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.

November 29, 1988

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

Dear Jan:

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

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

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

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TUCSON, ARIZONA 85721 USA

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

FAX to 301-353 3870

5995

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

MESSAGE CENTER

	1	2	3	4
CONTACTED	Terry			
DATE/TIME	28/1/5			
PER. INI	200			

28 NOV 1988 11:05

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

THE UNIVERSITY OF ARIZONA
TUCSON, ARIZONA 85721 USA

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

November 24, 1988

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Yours Sincerely

Jan Rafelski
Professor of Physics

REVIEWER #2

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

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

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

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

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

Reply to reviewer #2

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

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

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

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

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

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

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

Question (6) of Reviewer #1:

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

Our reply:

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

(i) generation of D_2 at voids in the lattice (see also comments by reviewer #5). However, if this explanation applies, the excess energy generated during 331 hours of polarization at the highest current density would have required formation of D_2 bubbles at a higher rate than that corresponding to the applied current, i.e., there would have been a loss of dissolved D. Such a loss is inconsistent with the observation of the generation of a constant excess enthalpy during three successive periods of 75, 155, and 101 hours. Moreover, at least 0.5 cm^3 of bubbles at 2000 atmospheres (the tensile strength of Pd) would have been formed which would almost certainly have disintegrated our sample of Pd. The structural integrity of the sample was preserved and, indeed, it is well known that electrochemical equivalents of Pd diffusion tubes can be used indefinitely. The easiest way to discount this possibility of bubble formation is to increase the experiment times. However, we do have it in mind to search for any D_2 or, more likely, He bubbles.

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

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

Paragraph (3) of Reviewer #3:

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

Our reply:

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

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

November 29, 1988

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

Dear Dr. Beck:

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

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

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

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Department of Energy
Washington, DC 20545

November 18, 1988

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

Dear ~~Dr. Beck~~ Ted:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #5.

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

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

Sincerely,

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

Enclosures

11-22-88

Dear Ryszard:

A response to the Pons/Fleischman response is enclosed.

Ted Beck

Response to Pons/Fleischmann Response

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

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

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

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

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

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

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

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

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

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

Reply to reviewer #5

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

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

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

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

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

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

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

Question (3) of Reviewer #1:

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

Our reply:

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

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

Question (4) of Reviewer #1:

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

Our reply:

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

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

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

Question (6) of Reviewer #1:

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

Our reply:

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

(i) generation of D_2 at voids in the lattice (see also comments by reviewer #5). However, if this explanation applies, the excess energy generated during 331 hours of polarization at the highest current density would have required formation of D_2 bubbles at a higher rate than that corresponding to the applied current, i.e., there would have been a loss of dissolved D. Such a loss is inconsistent with the observation of the generation of a constant excess enthalpy during three successive periods of 75, 155, and 101 hours. Moreover, at least 0.5 cm^3 of bubbles at 2000 atmospheres (the tensile strength of Pd) would have been formed which would almost certainly have disintegrated our sample of Pd. The structural integrity of the sample was preserved and, indeed, it is well known that electrochemical equivalents of Pd diffusion tubes can be used indefinitely. The easiest way to discount this possibility of bubble formation is to increase the experiment times. However, we do have it in mind to search for any D_2 or, more likely, He bubbles.

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

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

Question (7) of Reviewer #1:

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

Our reply:

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

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

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

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

December 6, 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 Professor Pons' rebuttal on the proposal entitled, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium."

Your kind assistance in our evaluation process is genuinely appreciated.

Sincerely,

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

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

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

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

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

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

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mate the procedure of obtaining the best equation of state on the basis of a system of equivalent equations and to estimate the degree of reliability of calculation of any thermodynamic function from experimental p, v, T data but also to ascertain the advisability of incorporating one group of data or other into the calculations and thus to optimally plan further experiments.

[6]G. A. Spiridonov, A. D. Kozlov, and V. V. Sychev, in: Thermophysical Properties of Substances and Materials (in Russian), Vol. 10, Izd. Standartov, Moscow (1976), p. 35.

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Translated by Eugene Lepa

Anomalously high isotope ratio $^3He/^4He$ in technical-grade metals and semiconductors

B. A. Mamyrin, L. V. Khabarin, and V. S. Yudenich

A. F. Ioffe Physicotechnical Institute, Academy of Sciences of the USSR, Leningrad

(Presented by Academician V. M. Tuchkevich, February 24, 1978)
(Submitted February 16, 1978)
Dokl. Akad. Nauk SSSR, 241, 1054–1057 (August 1978)

PACS numbers: 82.90. + j, 35.10.Bg

The helium isotope ratio in any object carries information about processes leading to their formation.

Table I lists natural objects containing helium with sharply differing isotope ratios and also indicates the main processes leading to the formation of helium with the corresponding isotope ratios.

It is shown in the present paper that in a number of commercially-pure metals and semiconducting materials helium is found to be present in a $^3He/^4He$ ratio of close to 1, i.e., several orders of magnitude higher than the values given in Table I. This would indicate that the origin of helium isotopes in the materials studied was due to some processes, nuclear or physicochemical, other than those enumerated in Table I. A high $^3He/He$ ratio in aluminum was reported by Alimova et al.[3]

I. It turned out that our investigations, as those in Ref. 9, could be carried out by employing a sensitive mass-spectrometric technique[10] and a low-background gas-liberating apparatus.[11]

The gas was extracted from the samples by heating them in a vacuum unit with a corundum crucible. The helium was then purified by removing the accompanying gases with liquid-nitrogen-cooled carbon traps and a titanium getter heated to T ~ 400°C, after which the helium isotopes were analyzed in a magnetic resonance mass spectrometer. The minimum measureable quantities of ^3He and ^4He were ~10^6 and 10^9 atoms, respectively.

We studied samples of 18 elements from all groups of the Mendeleev periodic system (Ni, Cu, Al, Ti, Au, Mo, Ta, Ag, Zn, Pb, Bi, Pd, Pt, Re, Ir, Ge, Si, and InSb). Three to five samples of each element were studied; more thorough investigations were made of Ni (~200 samples), Cu (~30), Ta (~20), Ti (~20), Au (10), and Al (10). In most cases the samples were foil ribbons measuring (0.05–0.1) × 10 × 125 mm. The impurities in the samples corresponded to the standard commercially-pure metals.

II. The results of the investigations, based on analysis of more than 300 samples, reduce to the following. Many

TABLE I

Object	Processes	$^3He/^4He$	Source
Earth's crust	α-decay of radioactive elements, reactions of type $^6Li(n, \alpha)T$, $T \xrightarrow{\beta^-} {}^3He$	10^{-7}–10^{-6}	(1)
Uranium-thorium minerals	Ditto	10^{-6}–10^{-5}	(1)
Lithium-beryllium minerals	" "	10^{-5}–10^{-4}	(1)
Upper mantle of Earth	Ditto, presence of primary helium captured during formation of Earth	$3 \cdot 10^{-5}$	(4, 5)
Atmosphere	Diffusion of helium isotopes from mantle to crust, dissipation of helium isotopes into outer space, nuclear reactions in upper atmosphere with formation of T, Be	$1.4 \cdot 10^{-6}$	(6, 7)
Lunar soil	Spallation reactions, implantation of solar wind	$2 \cdot 10^{-4}$	(8)

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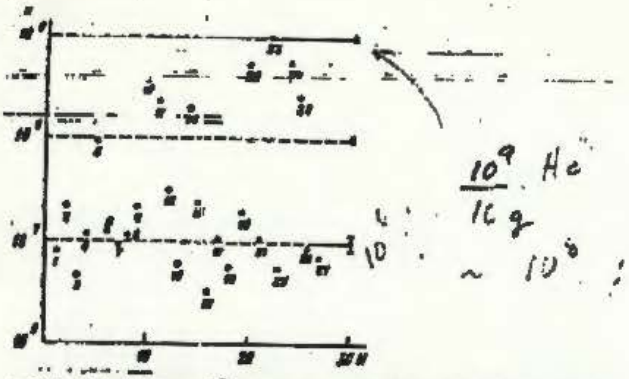

FIG. 1. Distribution of ^{3}He in segment of nickel foil: n is the number of atoms in a sample measuring 0.05×10×125 mm, N is the ordinal number of the sample. The vertical segments characterize the error of measurement.

samples contain a considerable quantity of ^{3}He. These quantities vary markedly both from element to element (10^{10}–10^7 atoms/g) and from sample to sample (from 10^{10} atoms/g to a background value of $\sim 10^6$ atoms/g).

The values of ^{3}He/^{4}He vary from 10^{-3} to 1. These values of the isotope ratio, are, apparently, underestimated since the quantity of ^{4}He, as a rule, should not exceed the threshold of instrument sensitivity (10^9 atoms) and in most cases this was the value taken for calculations.

The ^{3}He distribution was found to be spotty in character. In nickel foil (Fig. 1) samples with a comparatively high ^{3}He content are frequently next to samples with a comparatively low content. It may be said on the basis of the results that the spots in which ^{3}He is contained have a size of less than 1 mm. Next to a relatively large bunching of these spots is the practically complete absence of these spots along the length of the foil up to several tens of centimeters. The results of measurements of the ^{3}He content in other metals also indicate a pronounced nonuniformity in the ^{3}He distribution.

The curve of ^{3}He extraction from nickel foil (Fig. 2) displays two ^{3}He peaks much like those presented by Tishchenko et al.[12] who studied the desorption of helium from a silver film with various helium concentrations. Tishchenko et al[12] showed that when samples with high helium concentrations are heated the helium collects in microbubbles and is desorbed at ~1100°K. At low concentrations the desorption occurs by diffusion of individual atoms at a much lower temperature, 400–700°K. Comparison of our results with those with Ref. 12 shows that the desorbed peaks (Fig. 2) correspond to diffusion of individual atoms and, consequently, there were no bubble formations in our samples.

The certainty of the effect can be confirmed by the following facts.

1. The quantity of ^{3}He considerably exceeded the background level (100 times) in many samples.

2. Repeated (more than 50-fold) blank tests did not once yield quantities of ^{3}He which noticeably exceeded the background level of the instrument.

3. Two gas-liberating apparatuses of different construction were used to extract the gas and the picture of the effect was the same in both cases.

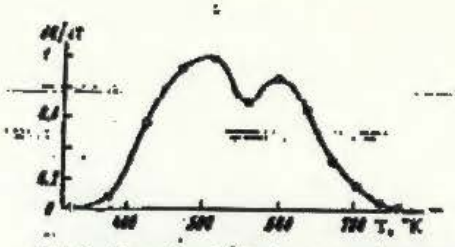

FIG. 2. Extraction of ^{3}He from nickel foil when temperature is raised uniformly at rate of 3.6 deg/min.

4. In some cases with the effect the sample was completely melted but in comparison with heating to 600°C (the ordinary procedure) this did not significantly change either the total quantity of ^{3}He extracted or the ^{3}He/^{4}He isotope ratio.

5. The quantity of ^{3}He in each sample was determined as the average of the heights of five recorded peaks whose magnitude practically did not change.

6. In our instrument[10] the ^{3}He line was completely separated from the HD and H_2 lines.

III. Let us consider the possible processes leading to the occurrence of ^{3}He in metals.

1. Diffusion from the ambient. Any processes of penetration from natural media cannot give the observed helium isotope ratio since there are no helium sources on Earth with such a high ^{3}He/^{4}He ratio. The possibility of the penetrating helium being enriched with the ^{3}He isotope by several orders of magnitude is apparently excluded because of the small difference in the masses of ^{3}He and ^{4}He.

2. It may be assumed that ^{3}He in metals is formed as the result of the β-decay of tritium. Evidence in favor of the T–^{3}He bond is given by: the "point" character of the ^{3}He distribution which is much like the hydrogen distribution in metals[13]; some experimental data indicating the possibility of sharply increased concentrations of tritium in metal. The T/H ratio measured for several aluminum samples[14] was $\sim 10^{-9}$ which exceeds the ratio in possible ambient media by 6 to 8 orders of magnitude.[15] The usual processes of separation of isotopes by diffusion from the ambient medium evidently cannot yield the effects indicated (contamination of the samples with technogenetic T was ruled out).

3. There is a hypothesis[14] that the high tritium concentration in aluminum results from the separation of hydrogen isotopes in electrolysis during the production of the aluminum. However, the aluminum should be enriched in the process but this was not detected in the first experiments. Moreover, with this explanation metals produced without electrolysis should not have high T/H and ^{3}He/^{4}He ratios. However, many samples of metals obtained by the blast-furnace process, zone refining, as well as semiconductors obtained by the Czochralski method contain helium with a high ^{3}He/^{4}He ratio.

4. It can be assumed that ^{3}He in metals is formed as the result of processes which occur right in the metal and these processes lead to the formation of either ^{3}He, T, or T and ^{3}He together. An analog of processes of this type is the nuclear reaction which takes place with the interaction of deuterium mesic atoms with ordinary deuterium atoms at thermal energies $\mu^- d + d \rightarrow {}^3\mathrm{He} + n + \mu^-$.

It should be borne in mind that the absolute concentration of ^{3}He and T in metals are low and the build-up time for ^{3}He and T may be quite long. Accordingly, even processes with a very low yield can, in principle, supply the observed quantities of ^{3}He in metals.

Reliable proof of the causes of the effect discovered requires further experiments.

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Translated by Eugene Lepa