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

August 10, 2016

Re: OSTI-2016-01064-F

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

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

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

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

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

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

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

Sincerely,

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

THE UNIVERSITY OF UTAH

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

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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.

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

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 x 10^{-14} s^{-1} and are aiming to achieve say 3 x 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 ^3He 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

$$-D\left(\frac{\partial C_b}{\partial x}\right)_{x=o} = k_1 C_s - k_2 C_b$$

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, β, 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 ($\beta + \gamma$) 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 3×10^{-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} $(°)^{-1}$ 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 3×10^{-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 <u>if</u> we were correct in attributing the excess enthalpy to nuclear fusion, then we are certainly <u>not</u> 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.

<u>Thermal neutrons</u> 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.

<u>γ- rays</u> 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.

<u>Tritium and ^{3}He</u>. 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, ß 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 ßPd-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.