December 8, 1988

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

Dear Professor Pons:

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

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

Please let me know your thoughts on this subject.

Sincerely,

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

Enclosures

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 flow 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).

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.

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.

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

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.