November 18, 1988

Professor Johann Rafelski
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Tucson, AZ 85721

Dear Jan:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #2.

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

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

Sincerely,

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

Enclosures

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

Our reply:

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

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