2004 U.S. Department of Energy Cold Fusion Review
Reviewer Comments

Original comments from the reviewers of the 2004 U.S. Department of Energy Cold Fusion Review.
http://www.newenergytimes.com/DOE/DOE.htm

Review # 1

Subject: Experimental evidence for the occurrence of nuclear reactions in condensed matter at low energy

I have reviewed the materials provided, including the summary paper of Hagelstein et al. and the accompanying manuscripts.

The summary paper, in my view, does not provide an adequate overview of experimental results in this area. The references are taken overwhelmingly from conference proceedings (primarily the ICCF series) and other sources than cannot be regarded as peer-reviewed sources. In my view, the references are also culled to present a one-sided view of the current state of experimental results.

The 1993 Fleischmann and Pons article is included, which reported calorimetry experiments yielding excess heat in the several tens of W range. A large number of subsequent experiments have established upper bounds in the 0.1W range. A second paper (Mengoli et al.) is included which reports small power outputs, thus clearly contradicting the 1993 letter, but at a level still in conflict with other results.

This field is 15 years old. It has been characterized by a large number of positive but internally inconsistent results, plus an even larger number of negative results refuting many of the claims. By in large those experiments done by experienced nuclear physics groups have been negative.

As many have said, extraordinary results require extraordinary proof. Such proof is lacking. Existing results are erratic; many past results (excess tritium, charged-particle production, neutron bursts) have been demonstrated to be wrong and retracted. A partial summary of early retractions is given in Morrison's 1990 article.

It is impossible to prove a negative: that cold fusion does not occur at any level. However, repeated retractions; erratic and inconsistent claims of the levels of cold fusion; positive results clearly in contradiction with other, negative ones; and clear evidence of careless or even fraudulent work (such as the MIT analysis of the Pons-Fleishmann gamma ray spectrum) have eroded all of this field's credibility.

In summary:

1) The experimental evidence for "cold fusion" is unconvincing. Much of the work (including several of the papers included in the packet) is of poor quality, with inadequate descriptions of apparatus, a lack of error analysis, and data presented without uncertainties.

2) The evidence does not demonstrate that a new phenomenon is occurring.

3) I do not see a scientific case for continuing these studies under federal sponsorship.

Review #2

Here is my evaluation on the subject of recent scientific reports of low energy nuclear reactions in metal matrices. It is based largely on the material you sent me, including the summary document and appendix material.

In my opinion, there appears to be rather convincing evidence for the production of excess heat and for the production of ^{4}He in metal deuterides. The question is: Could this be the result of a nuclear reaction involving the d+d reaction?

Nuclear physicists have measured the rates of the d(d,□)^{4}He reaction, as well as those of the d(d,n)^{3}He and the d(d,p)^{3}H reactions. It is known that, when extrapolated to near zero energies, the rates of the (d,n) and the (d,p) reactions are about *seven orders of magnitude* larger than that of the (d,□)

reaction. Therefore it follows that if the ^{4}He is being produced by the d(d,□)^{4}He reaction, there would be seven orders of magnitude more neutrons and protons compared to the number of ^{4}He nuclei produced. As stated in the summary document "*Searches for neutrons, tritons, and other energetic emissions in quantitative association with the excess heat effect have uniformly produced null results.*"

On the other hand, there have been reports of low-level neutron (and proton) emission. These are quantitatively entirely too small in numbers to account for the heat production, and occur using current densities in the test cell which are an order-of-magnitude smaller than those needed to produce the excess heat (30 vs. 200-300 mA/cm^2). This indicates that these observations, even if correct, are not related to the observations of excess heat or the observed increase in ^{4}He.

My Conclusion: There is no convincing evidence for the occurrence of nuclear reactions in condensed matter associated with the reports of excess heat production. Independent of this, however, the reports of low level neutron and proton emissions have not been refuted.

It is suggested that the observations of excess heat and ^{4}He are consistent with:

$$D + D \rightarrow {}^4He + 23.8 \text{ MeV (heat)}$$

$$\text{or} \quad d + d \rightarrow {}^4He + 24 \text{ MeV (lattice)}$$

This implies that, somehow, the excited ^{4}He nucleus transmits its energy directly to the crystalline lattice of the solid. This is reminiscent of the Moessbauer effect. However, in that case the recoiling nucleus has an energy of ~2×10^{-3} eV. It is hard to imagine how 23.8 MeV of excitation energy, nearly 9 orders of magnitude more than in the case of the Moessbauer effect, could be coupled to and transferred to the phonons of the lattice!

The observations of low-level neutron and proton emissions is interesting, but appears to be unrelated to the reported observations of excess heat and ^{4}He. Further quantifying these results would seem worthwhile, but not in connection with the generation of excess heat.

The excess heat reported remains unexplained. However, in my opinion, there is no evidence for this being a nuclear physics phenomenon.

Review #3

Comments on the LENR paper

In general, this reviewer found the paper with its supporting appendices to be well-written and easy to read. The authors have, necessarily, limited the scope of the paper to the issues of excess heat and nuclear markers. To cover the entire fifteen years of the "cold fusion" controversy is too much to expect in a document of manageable size.

While the paper might well cause some scientist to revise their thinking about "cold fusion," I doubt if it will do much to sway the thinking of the real skeptics. This is unfortunate in the opinion of this reviewer. Whether or not LENR occur in metal-deuterium systems, the chemistry and physics of these systems are far from being understood. The "stigma" branded upon those who have chosen to study these systems and on the research performed by these individuals has most certainly prevented progress towards characterizing these systems. But, because of the prejudice which has developed around this field, a higher standard of proof, deserved or not, has been put on the authors. In the opinion of this reviewer, they could have done better.

This reviewer has some criticisms about the content of the paper. First, the results presented as evidence for the existence of the various conclusions about the Pd/D system are mostly from the SRI laboratory of one of the authors. While other results are referenced and in some cases mentioned in the text, the case for the existence of LENR would have been strengthened by demonstrating reproducibility using the results of other investigators and laboratories. This is particularly important considering the fact that the observed effects are apparently difficult to achieve, and appear to occur relatively infrequently. Some of the controversy over the effect is undoubtedly due to the fact that the "signal to noise ratio" of positive results to backgrounds are low. Secondly, probably for completeness, results referred to as "excess heat beyond the basic Fleischmann-Pons experiment" and which appear to complicate, or either suggest more than one reaction path or raise doubt about the mechanism yielding the results, are included. It would be much easier to accept LENR as the phenomenon responsible were it not for the variable results introduced by these other metal-deuterium systems. For example, were excess heat and

4He the only observed products, accepting LENR, with the mechanism of D + D to give heat and helium-4, would make sense. But the fact that 3He, T, and protons are reported by some investigators makes the acceptance of LENR much less comfortable. The suggestion that the experimental conditions affect the mechanism is the authors' explanation, but this would suggest that LENR in metal deuterides is an effect which occurs routinely in such systems. If so, an explanation as to why these effects were not seen in the myriad of studies of metal-deuterium systems before would be required.

The authors apparently elected not to discuss the reported cases where explosions have occurred with these systems. While the explosions do not affect the conclusions of the paper, the origin, if related to a LENR effect, could be important in determining whether or not the effect would have practical importance. At present, accepting the concept of excess heat, the reported amounts appear to be too low to compete with present sources of heat.

This reviewer's conclusion is that the Pd/D system is far from being understood and that some challenging and potentially new phenomena are being observed in high loading experiments with the system. As such it should be the subject of further investigation irrespective of whether or not the observed phenomenon is LENR. Ideally, this field of investigation will become acceptable within the physical sciences community and those who wish to perform research on the system will have their work judged without prejudice or dismissal out of hand. As to LENR, the evidence strongly suggests a nuclear origin for the excess heat observed in palladium rods highly loaded with deuterium. However, the inconsistencies in the observed products and the widely different experimental setups, e.g. electrochemical, metal-gas, and beam, producing similar effects, coupled with the apparent low frequency of occurrence for the phenomenon, leaves LENR still debatable.

Have the authors provided convincing evidence that the Pd/D system is worthy of continued investigation? The answer is clearly yes. Have the authors provided evidence that LENR exists? Maybe! Should DOE establish a sizeable program to investigate LENR? No. Should DOE consider individual applications for financial assistance for research on the Pd/D system? Yes. Such applications should be considered on their merit.

Review #4

The articles for which reviews were requested are listed together with an abbreviation that will be used in the discussion.

FP-CALORIMETRY OF THE PD-D20 SYSTEM: FROM SIMPLICITY VIA COMPLICATIONS TO SIMPLICITY
Martin Fleischmann and Stanley Pons
PhysicsLettersA 176 118-129 (1993)

GM-CALORIMETRY CLOSE TO THE BOILING TEMPERATURE OF THE D 2 0/PD ELECTROLYTIC SYSTEM
G. Mengoli, M. Bernardini, C. Manduchi, G. Zannoni
Journal of Electroanalytical Chemistry 444 155-167 (1998)

MK-THE EMERGENCE OF A COHERENT EXPLANATION FOR ANOMALIES OBSERVED IN D/PD AND H/PD SYSTEMS; EVIDENCE FOR 4HE AND 3HE PRODUCTION Michael McKubre, Francis Tanzella, Paolo Tripodi and Peter Hagelstein
International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy.

MM-THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CO-DEPOSITION
M.H. Miles, S. Szpak, P.A. Mosier-Boss and M. Fleischmann
The Ninth International Conference on Cold Fusion. 2002. Beijing, China: Tsinghua University.

PH-UNIFIED PHONON-COUPLED SU(N) MODELS FOR ANOMALIES IN METAL DEUTERIDES
PETER L. HAGELSTEIN
Proceedings of the Tenth International Conference on Cold Fusion, Cambridge, MA (2003) World Scientific

SJ-CHARGED-PARTICLE EMISSIONS FROM METAL DEUTERIDES S. E. JONES, J. E. ELLSWORTH,M. R. SCOTT, F. W. KEENEY, A. C. JOHNSON, D. B. BUEHLER, F. E. CECIL, G. HUBLER, P. L. HAGELSTEIN
Proceedings of the Tenth International Conference on Cold Fusion, Cambridge, MA (2003) World Scientific

In addition I will relate the substance of these articles to the review article posted by DOE:
HMNCH-NEW PHYSICAL EFFECTS IN METAL DEUTERIDES
Peter L. Hagelstein, Michael C. H. McKubre, David J. Nagel, Talbot A. Chubb, and Randall J. Hekman
ftps://www.sc.doe.gov LENR

These articles span a decade, 1993-2003, of research following the initial report in 1989 of cold fusion (CF). The FP article, nearly four years after the original work, was commented on by David Morrison, (see Appendix A) as presenting a complicated analysis and under-instrumented experiments. A rebuttal in a later article by S. Szpak, et al. (including M. Fleischmann) stated that the criticisms raised by Morrison were either irrelevant or inaccurate or both. In reviewing FP I too (and before I had uncovered the Morrison article) was struck by the very complicated analysis, a lack of detail in the description of the apparatus, and a lack of details of the experimental procedure. This article is 12 pages in length - more than a normal "Letter" and is devoted more to the analysis in substantiation of the "excess enthalpy" claim. In the interval from the discovery report the analysis has become more detailed but the apparatus is nearly unchanged (some silvering was added near the top of the cell) and details of the difference between Pd obtained from different manufacturers while recognized was not investigated. Also recognized was the increase in excess enthalpy as the temperature rose to the boiling temperature of the D_2O and it was suggested that pressures above room pressure to permit still higher temperatures would be useful. But so far as is in the paper set of my review and other papers I searched as noted in the Appendix B such overpressure studies have not been carried out. And the FP suggestion for overpressure studies had only to do with suppressing the gas film around the electrodes that increased the potential of the cell constant current. No explicit claim is made in this article that nuclear processes are involved - although a comparison to the rate of energy production with a fast breeder reactor is made. Mention is made that in some cases they observed a decrease in cell potential and an increase of the cell temperature and used this relation to answer the question of how the temperature can increase while the enthalpy input decreases, i.e., with a source of enthalpy in the cell. I did not see any temperature measurement of the effluent gas/vapor although the enthalpy output from the vapor is calculated to be more than 15 times that of enthalpy output to "ambient". A graph displayed a "burst" of excess enthalpy that occurred over a 55 h period with temperatures in the cell more than doubling. Heating pulses and a D_2/Pt system for calibration were reported; the Pt gave no excess enthalpy.
The conclusion I would draw is that greater attention to a more rigorously carried out experiment is needed, not just more complex analysis of the more or less standard cell in order to adequately establish these unusual measurements and conclusions. I agree that some excess energy appears to be generated with the D_2/Pd system.

The GM experiments of 1998 (five years later than FP) clarifies in 13 pages many of the shortcomings in the FP article. (Of course other articles by Fleischmann and Pons may have also addressed these concerns.) The GM article contains both a summary of previous work and substantially more details of the experimental cell, the electrodes, and the experimental method. For example, the use of both strips of Pd metal foil as well as rod/wire material was reported. Foil strips from a Russian source and rod/wire from Johnson Matthey were each obtained through an intermediate supplier. The preparation of each Pd cathode was described as well as the electrical connection and electrolyte isolation technique. The heater wire, thermometers, and anode were described. A method to mix the electrolyte by gas bubbling was carefully calibrated by the tedious method of bubble counting. Operation at a controlled temperature of 95 ^oC was accomplished using a temperature controlled oil bath.
Two different thicknesses of foil, 0.02 cm and 0.05 cm, and a variation of total weight of Pd cathodes between about 0.34 and 1.9 g were used in five experiments plus a sixth with the 0.4 cm diameter rod/wire or 2.2 g weight. And one experiment with H_2O was performed with a foil similar to a foil used for a D_2O experiment. Some excess heat was observed and considered to be enthalpy release from PdH_x formation. Similar enthalpy output would occur for PdD_x but in each case is much less (~0.3 W) than the reported anomalous values of ~0.6-1.36 W that extended over much longer times. Additional variations of experiments were made, some by bubbling H_2 or D_2 instead of N_2 into the system. And the continued production of enthalpy when an open circuit of the electrolyzing current was established provided the most convincing proof of a non chemical process that was generating enthalpy. The suggestion is made that a

$$d + d = {}^4He + 24\ MeV \qquad (1)$$

process was occurring and that the required consumption of deuterons was 4 (10^{16}) deuterons/day for a 1 W output. For a ratio D/Pd = 0.8 or 5 (10^{21}) deuterons this process was stated to be potentially long lasting.

The conclusion I draw from these experiments is that considerable improvement in experimental technique and instrumentation has been developed. Greater attention to the publication of details strengthen the conclusion that excess enthalpy has been developed in the cells of D/Pd. The suggestion of a nuclear process is certainly unsubstantiated from the present experiments; no ^4He was looked for or detected nor were other nuclear reactions considered (see appendix B).

The MK experiments at SRI (Stanford Research International) were designed to determine production of ^4He as a product of the deuteron fusion reaction of Eq. 1. Thus, the cell design is modified so that mass spectrometer spectra can be taken of the effluent gas with a sensitivity to resolve D_2 and ^4He. Stainless steel cells were used with careful exclusion of ^4He from the air (about 5 ppm at STP). D_2 was loaded into Pd on a carbon supported catalyst with pressures to 3 atm and temperatures to 250 ^oC. These experiments were meant to test earlier results by L. Case. And catalytic loading was carried out to replicate with better measurement of helium the experiments of Arata and Zhang. In three "open" cell experiments where excess power was observed they found ^4He in the effluent $D_2 + O_2$ gas. However, unlike the report HMNCH they found some instances where no helium was produced even at measurable excess power levels. The helium increase in sealed cells in gas loaded Pd show a variety of behavior: no increase in ^4He over long periods of time (including the H/Pd control experiments), slow exponential increase of ^4He, and no increase for varying lengths of time followed by rapid increases in some cases to level above 5 ppm (of STP air). A correlation between the excess energy and ^4He production was shown, the slopes give ~ 32 MeV/^4He although the mean value total mass is only 75% of that expected from the Eq. 1. An additional experiment provided a closer fit (104%) to the mass balance. The excess power had a maximum of 9.9 % of the input power and an average of about half that. For H/Pd experiments, up to about half the D/Pd power was observed at higher input powers but the integrated energy produced was - 1 +/-6 MJ compared to a D/Pd energy production of 64 +/-6 MJ over a similar 86 day period.

At the conclusion of these experiments both the Pd-bulk and Pd-carbon supported catalyst cathodes were analyzed for ^4He and ^3He. Large amounts of ^3He were found with the high resolution mass spectrometer and ratios of $^3He/^4He$ of ~ 2 (10^4) above natural abundance were found. Evidence presented in HMNCH show the ^3He radial gradient and support the production of ^3He in the cathode voids from the work of Arata and Zhang. These authors discuss only the above reaction of Eq. 1 but state they will examine other nuclear reactions to explain the ^3He production.

The article by MM includes Fleischmann as a co-author and presents work about a decade after the initial report. The experiments were conducted at the NHE (New Hydrogen Energy) Laboratory in Sopporo, Japan. Copper rod cathodes were used in a co-deposition experiment of the Pd and D_2. A lithium free electrolyte was also used. A Fleischmann-Pons type cell was used and three experiments were carried out at the same time with temperature and voltage recorded and various electrolysis currents set. The results show excess enthalpy that varies in response to the current set - from 0.006 to 0.3 A). The analysis was carried out by M. Fleischmann but a simple analysis was made by M. Miles and the two agreed well. Excess enthalpy during co-deposition of ~0.25 W was stated. A large variation with current was found. Two effects of an applied heater power pulse were shown: the continued increase of excess enthalpy after the pulse was terminated and a return to the expected temperature when there was no excess enthalpy production or constant excess enthalpy production. However, the details of when these two conditions were observed were not presented. No recombination of the D_2 and O_2 gases was found consistent with all earlier results - such recombination would obscure the excess enthalpy calculations. The most important parameters are the radiation heat transfer coefficient, k_R and the water equivalent, C_pM of the cell. An infrared camera revealed hot spots on the cathodes suggesting the inhomogeneity of the D_2 reaction with Pd.

I conclude from this article that co-deposition techniques are an alternative to solid Pd cathodes but the experiments with the Fleischmann-Pons type apparatus continue to lack convincing instrumentation and interpretation noted nearly a decade ago.

The article by SJ presents the results of a different experiment, one designed to determine the occurrence of the $d + d = {}^3T + p$ reaction by measuring the proton that has an expected energy of about 3 MeV. Titanium foils were loaded with D_2 in a separate experiment and then electrically wired in series and placed in front of a plastic scintillator, a glass scintillator, and a 5 in diameter photomultiplier tube. Pulses were digitized at 100 MHz over a 160 ms window and resolution was such that a distinction could

be made between narrow plastic scintillator pulses and the broader glass scintillation pulses (particle passing through both plastic and glass materials). The light intensity integral corresponds to the energy of the known particle, ^{4}He (or a), ^{3}T (or t), p, e^-. Calibration with ^{241}Am (5.45 MeV) was carried out. The loading produced TiD_x where x= 0.5 to 1.4. Two thicknesses of foils were used, 0.025 mm and 0.25 mm. The proton emission from up to 50 mm depth of the foil has sufficient energy to provide a light pulse. Various complications were examined: cosmic ray pulses and those from radon in the surrounding air of the experiment in the light tight box. To start a run the foils were heated by Joule heat from a current passed through the foil set. Two clear peaks from the plastic and glass scintillators were observed that were stated to be completely different from any of the calibration or background determinations. A matrix of possible particle/energy values was made and a 19 mm thick Al degrader was placed between the TiD_x foil and the scintillators to help determine the particle involved. The plastic scintillator peak broadened and the peak shifted to lower energy and these changes were stated to most likely represent a proton flux although the energy loss was somewhat smaller than expected. From the use of SRIM code the 2.6 MeV energy calculated and the 2.4 MeV energy measured through the degrader film is within the experimental error for proton flux. The best results give a proton energy of about 3 Mev from a depth of about 12 mm in the TiD_x foil. They also found evidence for "burst" type emissions; some 27 multi-proton events were observed in 2490 s of observation. Over the course of an hour the counts increased from background (3 counts/hr) to a rate of over 2100 counts/hr. The rate declined after about 1.5 hr and was 30 counts/hr some two weeks later - still above the background level measured earlier.
Two ion-implanted silicon detectors were set up to do coincidence counting of proton and triton emission. Thin TiD_x foils were mounted between the two detectors in a vacuum chamber Faraday cage. A value of x = 1.0 - 1.6 was stated for the different deuteration process and the foils were not heated in the vacuum due to likely detector damage. Coincident events were observed as an equivalent energy at each detector and the cumulative (9.7 days) set without cosmic ray and non-coincident events plotted as detector energy (1) vs. detector energy (2). The 9.5 counts/day compared with 3 counts/day background. The rate variations from 10^{-21} to 10^{-25} fusion/deuteron pair/s were found in a majority of the experiments but the variations are not understood and were stated to be investigated further. A variety of special preparations have been found essential to give these results such as cleaning and gas loading at high temperatures and some variations are considered the result of lattice defects etc. in the foils.
My conclusion from this article is that as an initial experiment substantial care was exercised both in the specimen preparations and loading and with the nuclear detector qualification, background evaluation and control, and data analysis. Independent measurements would be highly desirable coupled with analysis of the foil material to assess the initial quality, the final damage and the isotopic content before and after the experiments.

The theoretical article by PH presents a scenario for a nuclear process in understanding the excess enthalpy observed in the D/Pd experiments. After an introduction describing some of the many attempts at theory by the author and others, he has decided to work on the physical description of the process by including the solid state environment from the start. Not included are: low-level dd-fusion, energetic products not due to dd-fusion, heat and helium production, tritium production, and other anomalies but these were expected to follow from the inclusion of the solid state environment. A series of conjectures is formulated. These relate (1) to enhanced interaction of deuterons with a double site occupancy in a metal deuteride. Following each conjecture theory and connection with observations is presented. Conjecture (2) relates to new physics in the solid state environment, the possibility of nuclear reactions at two sites to be coupled together via a lattice resonating method. Conjecture (3) states that with generalization of vacuum models to include the solid state environment no new basic physics is needed. Photo-induced alpha emission is a model for formation of an excited nucleus from which alpha particles can evaporate but at energies below the maximum energy so that in deuterides a broad energy spectrum should be measured. Conjecture (4) states that "anomalies in metal deuterides are stimulated by strong phonon excitation." Phonons are indigenous to the solid state environment and different theoretical approaches are available. For new site-other-site interactions that the fast alpha particle emission requires, he considers a phonon-coupled fusion process coupled to an inverse process written as: $(d+d)_a + (^4He)_b \Longrightarrow (^4He)_a + (d+d)_b$. His current picture involves this process as well as compact states such as n+ ^{3}He states. Then conjecture (5) states that "null reactions are the dominant processes of the new phonon-coupled site-other-site reactions in metal deuterides". He proceeds to the development of localized states that increase the kinetic energy and centripetal energy terms. A solution of the complicated dynamics has not been found but believes the dynamics can produce rather compact states. This leads to the conjecture (6) that null reactions in metal deuterides can give stable compact states when phonon exchanges of the order 20 units of angular momentum are exchanged. He discusses the theory of Kasagi. A three deuteron interaction is suggested by Kasagi: d+d+d ==> n+p+ ^{4}He and Hagelstein believes this supports his compact states conjecture. Other channels of d+d+d would give d +

^4He or t + ^3He. The conversion of a nuclear quantum of energy to heat in the absence of fast ions or neutrons as in Eq. (1) has been an issue and conjecture (7) states "if sufficient angular momentum exchange occurs so as to stabilize the compact states, then the phonon exchange that occurs in association with null reaction couple energy effectively between nuclear and phononic degrees of freedom". A phonon-coupled SU(3) model was introduced and sheds light on the nuclear energy exchange process to phonon energies. This model would predict excess heat at high angular momentum exchange and other decay modes such as d+d would be dominant at lower angular momentum. A conjecture (8) about the slow tritium production that has been reported in some articles states the production is a consequence of tunneling from the compact state population to a double occupancy (molecular) state. The SU(4) model is employed for this calculation. The model predicts excess heat production with tritium production but this has yet to be observed. The possibility is suggested that an exclusion effect may occur such that a process that starts is reinforced to the exclusion of competing processes. The tremendous acceleration of the tunneling process between deuterons as is implied by the relatively fast reaction rates remains a difficult theoretical problem. Large screening enhancements do not seem reasonable. If a localized nuclear state with an energy resonant with the two-deuteron state this would change the dynamics significantly. Whether this might be aided by the solid state environment is not known. The theory of Rabi oscillations is considered relevant to this issue. And conjecture (9) states tunneling between deuterons can occur in connection with other fast processes that are coherent, such that the associated rate is linear in the tunneling factor e^{-G}. Haglestein states that models where tunneling comes in with e^{-2G} do not lead to reaction rates as observed. An SU(3) model is again presented. Maintaining coherence is required. Conjecture (10) states "this coherent tunneling process can be enhanced by a phase coherence between transitions at different sites, producing a superradiant enhancement". A possible model for "bursts" is suggested. And the observed anomalies imply rates that are inconsistent with rates for incoherent processes in all known cases. So tunneling as a coherent process provides reasonable estimates to experimental data.
The main conclusion of Hagelstein is that new effects, though wildly variant from nuclear physics in textbooks may follow new physical laws that are reasonable, understandable, and amenable to analysis. The systematic understanding of the disparate experimental anomalies is argued to result from an underlying physical picture of the interaction with the solid state environment. The need for high loading and the role of materials properties such as vacancies can be understood. The most significant test would be the fast alpha particle detection.
My conclusions are that this work, published at the same meeting as the SJ article is somewhat self serving since the TiD_x alpha particle measurements were known at this time and neither paper references the other. However, the approach to suggest that nuclear physics may undergo modifications in the solid state is certainly interesting. Although the set of conjectures are far from a coherent theory they do present insight into the experimental results. Curiously the theories, neither Hagelstein's nor Kozima's (see Appendix A) were discussed in the HMNCH.

SUMMARY

This set of articles make a significant case for phenomena in the deuterium/palladium system that is (I) markedly different from that of the hydrogen/palladium system, (ii) supportive of the claim that excess energy is generated in the deuterium/palladium system, and (iii) without a coherent theoretical explanation. In the 15 years since the discovery the articles under review (see above) show some progress toward investigating the phenomena with better controlled and instrumented apparatus. Variations in cathode design and materials, constant temperature bath near the D_2O boiling temperature, monitoring of the effluent gas for ^4He, searching for nuclear reaction processes by direct measurements, and investigating of isotopic changes in the cell materials have all strengthened the case for the initial claim of excess energy. There seem to this reviewer some gaps that need filling in the experimental set - in addition to the theoretical effort clearly needed. The significant increase in excess energy near the boiling temperature of D_2O compared to some 50 ^oC lower is hard to reconcile with a variation in nuclear process rate or any phonon assisted process, a la P. Hagelstein. If this temperature variation is important then experiments to go beyond the boiling temperature of D_2O would seem a logical step as mentioned (for different reasons) but apparently not accomplished by M. Fleischmann. Some repetition of the direct nuclear process measurements would seem to be in order, combined with isotopic assessment of the cathode material before and after an experiment that produced excess energy. There are enough failures of the experiments to produce excess energy that efforts to understand the differences should be made by better characterization and documentation of the experimental components. My gut feeling is that unless a substantial effort in made in a laboratory equipped and funded to carry out the metallurgy, chemical and material analysis, nuclear instrumentation and experimental design and analysis, etc. there will only be more conference reports without much advancement in the understanding of the phenomena.

The Italian (GM) and SRI (MK) efforts were a step in this direction but still lacked a concerted and encompassing attack. The understanding what effect if any, the solid state environment has on nuclear processes would certainly rank as a significant achievement especially in view of the very small effect that pressure is known to have on the nuclear decay rates.

Appendix A Articles Scanned in Addition to those Reviewed (the first author is noted)

L. Case, "Catalytic Fusion of Deuterium into Helium-4", *Proc. ICCF7*, Vancouver, Canada, April 19-24, 1998
Y. Arata and Y. Zhang, *Proc. Japan Acad.*, 70(B), p106, (1994); 71(B), p98, (1995); 71(B), p304, (1995); 75(B), p281, (1999); *J. High Temp. Soc.*, 21, p130, (1995)

J. Kasagi, T. Ohtsuki, K. Ishu and M. Hiraga, *Phys. Soc. Japan* 64, 777 (1995)

G. Preparata *Trans. Fusion Technology* 26 397 (1994) States ratio D/Pd > 0.7 and that D^+ is very mobile (diffusion 10^{-3} cm^2s^{-1}). A Fleischman-Pons cell with a thermal relaxation time $t = C_pM^*/4k_R$ where k_R is the radiative transfer coefficient, and a value 3600 s results.

David Morrison *Physics Letters A* 185 498 (1994) Critique of FP *Physics Letters A* 176 118 (1993).

Y. Ohta *Int. J. Hydrogen Energy* 29 1553 (2004) Discusses $D^+ + D^+$ fusion rate by implosion of a cavitation bubble. [This is a different approach to d+d fusion similar to ORNL work reported earlier.] The theory of Gamov-Teller is invoked to calculate the rate of reaction. For a bubble with consideration for D^+ density increase that reduces the temperature they find $T_{max} = 1.5\ (10^8)$ K , $n_D = 3.2\ (10^{17})$ and a minimum radius of collapse ~ 5 (10^{-3}) cm. Some 2 (10^{11}) fusions modified by an unexplained parameter a^{-1} are stated. The article ends with a discussion of world energy source etc.

H. Kozima *J. Electroanalytical Chemistry* 425 173 (1997) Theory is presented using the ^{6}Li atom in the electrolyte of many experiments to provide a neutron that begins an interesting series of reactions leading to d+d fusion.

H. Kozima *J.Hydrogen Energy* 25 505 (2000) Use of his theory to understand Tritium production in others experiments.

H. Kozima *J.Hydrogen Energy* 25 509 (2000) This article gives branching ratios:
d+d = t(1.01 MeV)+p(3.02 MeV) (1);
d+d= 3/2 He(0.82 MeV) +n(2.45 MeV) (1);
d+d= 4/3 He(76 keV) + g(23.8 MeV) (10^{-6}).

D. Afonichev *J. Hydrogen Energy* 28 1005 (2003) Experiments with Ti saturated with D_2 are presented with neutron emission found in one batch but never in another batch. Tritium concentration at the cathode surface is 3-7 times above background.

B. Constantinescu *J. Hydrogen Energy* 26 507 (2001) Irradiate Ni with D^+ ions to mock up a fusion reactor condition (1273 K and high vacuum) and study the damage of Ni. He bubbles.

J. Xiao *J.Hydrogen Energy* 24 741 (1999) This article examines the potential for cold fusion reactions in hydrogen storage media, $LaNi_5 + 6H = LaNi_5H_6$ and FeTi + H = FeTiH or FeTi + H = $FeTiH_2$. He shows acoustic emission.

D. Gozzi *J. Electroanalytical Chemistry* 435 113 (1997) X-ray emission, heat excess and ^{4}He measured from a D/Pd system. X-ray film is used and is placed outside the Pyrex vessel. Spots are found but are not seen with a blank cell.

W-S. Wong *J. Electroanalytical Chemistry* 434 31 (1997) A maximum loading ration of D/Pd is 0.85 at 295 K for either D or H when pressures of 2000 atm for D_2 or 570 atm for H_2 are used. He states that electrolytic loading results in a much lower ratio.

F. Wills *J. Electroanalytical Chemistry* 426 177 (1997) Reports that H + O recombination and related enthalpy production occurs in electrolytic cells but is important only at low current densities. This mechanism can not explain the FP type excess enthalpy in the D/Pd system.

G. Mengoli *J. Electroanalytical Chemistry* 395 249 (1995) A report using a Pd -95% and Rh-5% alloy to observe neutron emission with either electrolytic or gas loading of the cathode. An NE213 liquid scintillation neutron spectrometer was used. A statistical excess of 0.2 - 0.7 ns^{-1} with electrolytic loading and 1-15 ns^{-1} with gas loading.

D. Gozzi *J. Electroanalytical Chemistry* 380 91 (1995) Reports measurements of ^4He and t but no neutrons were measured. The t excess was less than expected from the excess enthalpy measurements.

Appendix B Some nuclear reactions of interest in this Review (Handbook of Chemistry and Physics 83 2002-2003, ed. D. Lide, CRC Press, NY, NY)

From the mass differences and a partition of energy by particle mass the nuclear reactions can be determined. Thus,

n mass = 1.6749271 (10^{-24}) g	p mass = 1.6726216 (10^{-24}) g
^1H mass = 1.6726216 (10^{-24}) g	^2D mass = 2.014107780 au
^3T mass = 3.0160492675 au	^3He mass = 3.0160293097 au
^4He mass - 4.0026032497 au	1 au = 1.66054 (10^{-24}) g

d + d = ^4He + 0.0256123103 au = ^4He + 23.89 MeV

d +d = ^3He + n + energy 3.28 MeV or = ^3He (0.82 MeV) + n (2.46 MeV)

d + d = ^3T + p + energy 4.56 MeV or ^3T (1.52 MeV) + p(3.04 MeV)

Review #5

This is a review of the recent material submitted by workers in the area of "cold fusion" (CF) for consideration for funding by the DOE. I have carried out research on Pd-H and Pd alloy-H systems for many years but I am not an expert on the nuclear or theoretical aspects of the CF research. I have carefully read the document entitled "*New Physical Effects in Metal Deuterides*" by Hagelstein, et al. Their review describes the latest research on the *Excess Heat* and *Nuclear Emissions* aspects of CF.

With respect to the section on *Excess Heat* I was disappointed that the review described some more sophisticated versions of the original Fleischman-Pons experiment but basically it seems to be "more of the same" of this type of research. At the end of the review they state that the ``scientific questions posed by these experiments are, in the opinion of the authors, both worthy and capable of resolution by a dedicated program of research". There are no specific plans offered for this ``program of research" that might elucidate the validity of CF but the implication is that if money were given for CF research we would get ``more of the same". The absence of specific research plans is a serious drawback to their proposal.

It seems to me that the authors should have had a section which carefully addressed the many cogent arguments offered in the literature tending to discredit the existence of CF. For example, K. Shanahan (Thermochim. Acta, 387 (12002) 95) has argued that the excess heats (E. Storms, ICCF8, (2001) p. 55-61) can be explained on the basis of fluctuations in the calibration. He has elsewhere pointed out that the catalytic recombination of D2 and O2 can also be a factor for the excess heat since the latter is evolved at the anode and the former will be evolved at the cathode once a steady state is reached.

In "*New Physical Effects in Metal Deuterides*" by Hagelstein, et al it is pointed out on page 3 that ``in no case was a calorimetric imbalance observed (19 examples) where an electrode failed to achieve a bulk average D/Pd loading of 0.90. However, all electrodes achieving a loading of 0.95 or greater (15 examples) exhibited an heat excess more than 3 times the measurement uncertainty". If this is the case, then I fail to understand why gaseous loading of Pd with D using up to 3.1 GPa of D2 to achieve D/Pd ratios greater than 0.95, does not lead to CF. Baranowski and coworkers (J. Less-Common Mets., 158 (1990) 347), who have had extensive experience with high pressure loading of metals with H2 and D2, failed to observe any evidence of *excess heat* in a system which is inherently simpler than the electrochemical ones. They also tried dynamic loading to these high D/Pd ratios without any evidence of either nuclear events or *excess heat*. The proponents of CF have failed to convince me why loading to D/Pd>0.95 in the gas phase does not lead to CF, in fact, they did not address this contradiction in "*New*

Physical Effects in Metal Deuterides" by Hagelstein, et al. It should be kept in mind that the bulk phase Pd doesn't care where the D comes from once it is within it!

In "*New Physical Effects in Metal Deuterides"* by Hagelstein, et al there are 130 references and only 2 of them are not directly from favorable CF literature. This illustrates the rather narrow focus of these researchers. My feeling is that there should be no funds set aside for support of CF research but, if the DOE receives a proposal in this area which suggests some definitive research which settle some of the issues, it should consider it for support as it would any other proposal.

Review #6

To begin a review of "Cold Fusion" it is useful to remind oneself of the quote by Dr. Gordon Baym from his article in Phys. Rev. Lett 63,191(1989).

"We are searching for new experimental phenomena in an area in which theory must be supported by consistent, systematic data. Any search for 'anomalous phenomena' is, in its early stages an experimentally, not theoretically driven field. It is necessary to stay as close as possible to conventional physics for as long as one can hold out, and only when driven up the wall should theorists invoke new physics."

Clearly the data described in the position paper is not consistent and systematic. Furthermore the scientists quoted do not spend enough effort searching for conventional causes of the phenomena claimed or for systematic errors in the measurements. Little has changed in Cold Fusion from the publication of John R. Huizenga's book "Cold Fusion: The Scientific Fiasco of the Century", U. of Rochester Press, Rochester, New York (1992). Cold fusion is inconsistent with a huge body of knowledge about nuclear processes developed over the past 70 years. Three miracles are required for "Cold Fusion" as described to occur. These are:

1. The Fusion Rate miracle. The inter-atomic distance of deuterium adsorbed onto palladium is larger than deuterium gas, 0.28-0.17 nm. The estimated tunneling rate for that distance is $3x10^{-64}s^{-1}$.

2. The Branching Ratio miracle. When deuterium atoms fuse a compound nucleus with an excitation energy of 23.85 MeV is formed. This a well studied reaction because it is commonly used as a source of 3 MeV neutrons. The excited nucleus is known to decay with a 50 percent probability by neutron emission 50 percent probability by proton emission. No significant production of neutrons have been observed in "Cold Fusion" studies.

3. The Concealed Nuclear Products miracle. Neutrons, tritium, or gamma rays are not observed in quantities consistent with fusion, see table 1.

The new claims are as follows ^{4}He production, charged particle detection, and a theory that would allow energy from the ^{4}He compound nucleus to be transferred to a nearby palladium nucleus. The ^{4}He measurements are not reproducible in other similar cold fusion experiments. The positive ^{4}He data are suspect because the details of the analysis are not give. The type of mass spectrometer and sample preparation are crucial to understanding these results. The better documented experiment gives a negative result. The charged particle detection measurements were not sophisticated in terms of particle identification. It is therefore not convincing as evidence of reaction 1(b). The theory paper is not believable and easily tested. If Pd isotopes were being excited by the process described ^{106}Ru would be an observable (and only ^{106}Ru).

	Reaction	*Energy Release (MeV)*	*Reaction sec $^{-1}$ per Watt output*	*Branching Ratio*
1(a)	D + D→ ^{3}He + n	3.27	1.91×10^{12}	~0.5
1(b)	D + D→T + p	4.03	1.55×10^{12}	~0.5
1(c)	D + D→ ^{4}He+ γ	23.85	2.61×10^{11}	10^{-7}
2	p + D→ He + γ	5.49	1.14×10^{12}	
3	p + T→ ^{4}He + γ	19.81	3.15×10^{11}	

4	$D + T \rightarrow {}^{4}He + n$	17.59	3.55×10^{11}	

Table 1. Known Fusion Reactions of Hydrogen Isotopes

Claims of excess heat are reviewed in more detail.

Excess power is measured rather than heat. This quantity is prone to anomalies due to several likely causes. The storage of hydrogen gas in the Pd electrode being release and combined with oxygen from the anode could cause excess heating. The power level is determined assuming a steady state heat flow. Changes in heat conductivity due to the dynamics of gas flow can result in temperature excursions.

The amount of excess heat claimed is small compared to the energy put into the system. For example take the experiment of Fleischmann and Pons, Phys. Lett. A, 118-129(1993). A palladium electrode was operated at 4 Volts with a current of approximately 0.4 Amperes for six days. The total energy input was therefore 830 kJ and the integrated excess heat reported was 26 kJ, 3 percent of the input.

The estimate of power generated by the palladium electrode is based on an equation that describes the balance heat flow in by electrolysis and heat flow out by either radiative transfer of advection of gas. While there is a great deal of discussion concerning the determination of radiative heat transfer coefficient there is essentially none on the advective component. The gas is evidently assumed to be in thermal equilibrium with the solution. Errors in the treatment of advective heat loss will be transferred to the radiative heat loss. The method of calibration does not address this type of error because the gas bubbles are formed at the palladium electrode not at the heater element. The radiative heat transfer coefficient is stated as being lower than the black body radiation rate. An estimate of the black body radiation rate based on the figure 2 in the paper is 28 percent lower. These details are important. A question I would raise is does excessive bubbling improve the heat transfer from the palladium to the solution by stirring? Does normal bubbling remove more heat by advection?

Remembering as Huizenga pointed out in his book that the ICCF has a long history of excluding negative results, other power production experiments are presented as confirming evidence. The first of these is the SRI experiments. The first is an improved version of the Fleischmann and Pons electrolysis experiment. The results of this experiment is similar to Fleischmann and Pons. The excess power is correlated with deuterium flux. The objections I've raised to the methodology of measuring power applies equally to this experiment. The observed correlation with deuterium flux is consistent with heat transfer errors due to bubbling. The SRI experiment was improved to the extent that an attempt was made to mitigate uncontrolled recombination. The uniformity and distribution of oxygen is still questionable.

Correlations of “excess power” with deuterium loading is also claimed. The threshold for “excess power” is a D/Pd atom ratio of 0.89. This is an enormous amount of stored deuterium. The volume of stored D2 is 1000 cm 3 per cm 3 of palladium. If even a fraction of this stored were released the heat transfer balance would be disturbed significantly. If the stored deuterium were to recombine with oxygen the energy release would be 12 kJ per cm 3 of palladium.

Correlations with temperature and current density are also claimed these correlations may be the result of the flawed method of measure power generation. Again the most likely culprit being bubble generation an inherently nonlinear and turbulent process.

Helium production

Another class of experiments are referenced for the production of “excess heat” which do not involve electrolysis. The first of these is the Case experiments. Platinum group metals are loaded onto carbon substrates, 0.5 - 1.0 %. The excess heat is only observed with this low loading of platinum metals. This implies that carbon is involved in the effect. Six of 16 cells show excess heat. Four or five show helium excess as well. The most conventional explanation is that the carbon has adsorbed gases from the air, oxygen and helium. Oxygen combines with the deuterium to produce heat and helium is released on heating. The authors attempted to discredit this explanation by asserting that the container was helium leak tight. Presumably this was based on the ability to hold hydrogen. I don't see how the apparatus could be guaranteed leak-tight without a helium leak check. The authors suggest that a 5 day hydrogen presoak should displace helium adsorbed on the carbon substrate. That is only true if carbon is more selective for hydrogen than helium. They suggest that the rate of sequestration is low. That in fact does not help their argument because the carbon is not likely to be produced just prior to use. A few sample of catalyst were test and found to indeed have helium adsorbed but the quantities were considered low by the authors. The amount It is difficult to prove no contamination. The authors must look for ancillary evidence of air contamination argon for example, would be adsorbed more strongly than helium on carbon and would make a good air tracer. The fact that the effect is destroyed when the catalyst is heated above 300 C suggests that adsorbed gases are important. I have assumed that the mass spectrometer used could completely resolve ^4He + from D_2^+ . This can also be an issue. The helium build up shown in fig. 13 could be the result of saturating a titanium getter. Nonetheless these studies are neither consistent nor reproducible as the next experiment shows.

The Arata and Zhang experiment was another variant on the Case experiment. In this experiment a hollow palladium cathode is loaded with deuterium by electrolysis. Their experiment claims to have found excess power, tritium, and 3 He. The 3 He is undoubtedly from the decay of the small amount of tritium present in heavy water. The tritium observed was 2 to 5 $x10^{15}$ atoms more than 4 orders of magnitude smaller than should be expected from reaction 1(b) in table 1. Most significant is the complete absence of 4 He in this experiment. This is in contrast to the Case experiment. The description of this experiment indicated that the a titanium getter pump was used to remove deuterium gas and that the mass spectrometer was capable of resolving ^4He + from D_2^+ . The more careful experiment sees no effect with regard to ^4He production.

Note the article by McKubre et al. Misstates Arata and Zhang as having observed 4 He production.

Charged Particle Production

Jones et al. report a number of experiments attempting to measure charged particles from metal deuterides. The equipment used is somewhat primitive. The first apparatus described is a photomultiplier with a glass and a plastic scintillator sandwiched in front. Ionization in the plastic and glass is presumably differentiated by pulse shape discrimination. Coincident signals from both the glass and the plastic are rejected. The argument for identifying the particles as reaction products from reaction 1(b) in table 1 becomes convoluted. One must assume that pulse shape discrimination is very efficient because two parameter plots for beta and alph emitters are not shown. Then one must accept the arguments about range and trajectory to eliminate the ambiguities in the particle identification. Namely a 10 MeV alpha looks like a 3 MeV proton and looks like a 150 keV electron. Jones makes some argument about rejecting the 10 MeV alpha from 212 Po by looking for a coincidence with the beta in ^{212}Bi. His detector geometry is only 2 Pi the rejection rate is 50 percent at best.

Jones then reports an improved experiment where the TiD foil is placed between two surface barrier detectors. These experiments also suffer from the lack of a definitive particle identification. The data shown in figures 12 and 13 do not show the expected 3 MeV alpha in coincidence with a 1 MeV triton. In 9.7 days of running I see 2 events where a 3 MeV particle is in coincidence with 0.5 MeV particle and 2 events where a 3 MeV particle is in coincidence with 0.1 MeV particle. I do not find this convincing. Jones concludes that he has observed 10^{-21} to 10^{-25} fusions per deuteron pair per second. That is hardly sufficient to provide a significant source of energy. If one believes that is correct it is still a miracle 40 orders of magnitude larger than conventional physics allows.

Hagelstein Theory

This theory was apparently developed to explain Huizenga's miracle number 3, concealed nuclear products. The mathematics presented in the paper is sound. But, the devil is in the conjectures. The most implausible being conjecture II. While it is possible for nuclei to be coupled a two different sites as demonstrated by Terhune and Baldwin in the 1960's. That coupling occurs through the Mössbauer effect. There is no crystal lattice that could produce a recoilless transition of a 23.8 MeV gamma ray. Converting all of that energy in to phonons (heat) must have negligible probability of then reassembling into the narrow width of another nuclear level. I was somewhat puzzled by Hagelstein's discussion of phonons being mediated by the strong force. The reaction he conjectures is amusing because one of the reaction products from equation 8 is ^{106}Ru produced by alpha emission from ^{110}Pd (11 % natural abundance). The anomalous heat claims suggest 10 18 to 10 19 fusions if this reaction were at all probable the ^{106}Ru activity would be at the Curie level, quite hazardous.

Conclusion

I find nothing in the articles that I've read that convinces me that the new anomalies reported are not experimental artifacts. Exposing or disproving experimental artifacts is far more difficult than generating them. Better experiments could be done, however. For example, a time projection chamber trace showing a proton and triton originating from the same point in a TiD foil with the correct energy would be convincing. Certainly the weight of the evidence present thus far is not strong enough to overcome the three miracle requirement.

Review #7

I. General Comments.

I have spent the past few weeks reading the papers sent to me by the Office of Science, DOE, including many of those that are referenced in the overview paper by Hagelstein et al. (cited in this review as reference DoE31). I find it fascinating that, as noted by these latter authors in their Introduction, "thousands of papers" on this topic have been written since the initial cold fusion claims of 1989.

Compared with the early work on cold fusion with which I am familiar (e.g., I was a participant in the 1989 cold fusion evaluation workshop at Erice), I find the large number of different experimental methods that have been applied to the cold fusion problem to be very impressive. However, one aspect of cold fusion studies has not changed, namely that the field crosses the boundaries of several rather different scientific areas, such as chemistry, electrochemistry, thermodynamics, solid state physics, hydrogen storage in solids, and nuclear physics. It is difficult to find scientists who are knowledgeable in all of these areas – either cold-fusion practitioners or peer reviewers. As I note in a few examples in section II below, I have the impression that in some instances, cold-fusion experimenters are not as expert as they should be in the methods that they have chosen to use.

Also, I think that some scientists have, to say the least, not adequately explained their choices of various initial conditions in their cold fusion experiments, in particular to other scientists, such as myself, who are not working on cold fusion,. Researchers have gone from (i) the classic Fleischmann-Pons electrolytic cells containing aqueous D_2O solutions and metal electrodes (Pd or Ti); to (ii) arrays that establish "non-equilibrium" conditions by Joule heating in deuterated metal electrodes; to (iii) treatment with LiD powder followed by deuterated water and sulfuric acid, $D_2O + D_2SO_4$; to (iv) exotic sandwiches of coated metals through which D_2 gas permeates, without any application of electrical energy, to (v) glow discharges in the presence of deuterated metals, to (vi) laser irradiation of deuterated metals, to (vii) etc., etc. It is not made clear, at least to me, how such changes in initial conditions are expected to affect the outcome of the experiments. The initial conditions change drastically but the claimed final results seem to be the same: cold fusion reactions in the presence of deuterium but not of normal hydrogen.

I find it a bit disheartening that, despite the efforts of the dedicated researchers in this field during these past fifteen years, they have been unable (a) to completely solve the nagging problem of the non-reproducibility of the experimental results, or (b) to elucidate and/or nail down all the important parameters involved in the proposed cold-fusion phenomena (plural nuclear mechanisms have been proposed) or (c) even to convince the broader scientific community that cold fusion is real.

As a nuclear scientist, I must make note of what I think is an interesting question of logic and philosophy in the history of this field. From the earliest report of Pons and Fleischmann, the statement was made that the observed excess heat could not be explained by *known chemical effects* (or by extension, of *known solid-state effects)*. Therefore it was concluded that by default, any excess energy release had to be the result of *unknown nuclear processes* (all of the italics and underlining in this paragraph are by me). This single-minded conclusion has been pushed ever since, even though, as noted in DoE31 in footnote b on page 2, *"The excess heat effect itself is consistent neither with a conventional D + D fusion reaction mechanism, nor with any other nuclear reaction mechanism that appears in textbooks or in the mainstream nuclear physics literature."* So far as I can tell from my reading, there have been few attempts to search for evidence of *unknown non-nuclear processes, either chemical or physical*, to explain the results of cold fusion experiments. I will return to this point briefly in Section III.

II. Detailed Comments and Questions About Searches for Nuclear-Reaction Products in Cold Fusion.

In this section, I will focus on several topics that I am knowledgeable about, related to searches for evidence of nuclear processes occurring in cold fusion experiments. I must note that, because of constraints on my time and because of the large number of relevant papers, I have selected papers that seemed important and/or sparked my interest. My review necessarily does not cover all of the work referenced by DOE.

I also will not discuss the electrochemistry, calorimetry, and thermodynamics issues involved in cold fusion experiments, since these are outside my areas of expertise.

In the first case, section A below, I will present the results of my analyses that I believe are contradictory to the claims of a paper that has been cited as providing particularly strong experimental evidence for nuclear fusion, namely the paper on charged-particle detection of Jones et al., which was one of the additional papers distributed by DOE (cited in DoE31 as Ref. 94).

A. "The Jones Experiment" Concerning Charged-Particle Emission.

This paper by Jones et al. was presented at the Tenth International Conference on Cold Fusion. The paper shows data obtained with nuclear particle counters, and claims direct evidence for the fusion reaction, d + d $\rightarrow$ 3.02-MeV protons + 1.01-MeV tritons, from the observation (a) of protons in a solid scintillator array, and (b) of protons and tritons in silicon particle-detectors.

In particular, Fig. 5 of the paper shows that heated TiD_x foils produce two relatively narrow peaks, which are claimed to be protons detected in (a sandwich of) plastic and glass scintillators. The energy of the peak in the plastic is claimed to be 2.4 MeV if the peak is produced by protons. Fig. 6 of the paper purports to explain these peaks as being caused by 3.0 protons that are produced at a depth of 12 μm within the TiD_x foil. Those that exit perpendicular to the foils surface have 2.6 MeV (from Range-Energy Tables) and can pass through the plastic into the glass, while those emitted at 45^o to the surface have 2.4 MeV and stop in the plastic. A 19-μm Al degrader foil is subsequently placed between the TiD_x and the scintillators to lower the energies of the peaks, measure the energy loss, and thus determine the identity of the charged particles. Note that the energy response of the plastic scintillator is calibrated with a source of ^{241}Am.

I have reanalyzed these results and dispute the authors' analysis. Using Range-Energy Tables (with values interpolated between Ca and V to give values for Ti – not for TiD_x), I was able to verify the energy values of 2.6 and 2.4 MeV listed in Fig. 6 of the paper. However, it makes no sense to me to assume, as was done in the paper, that all of the protons are produced at the same 12-μm depth inside the TiD_x; actually, I think that the 12-μm depth was selected to fit the observed spectral data, not for any intrinsic physical reason. An alternative assumption, which has been stated in numerous other cold fusion papers, is that the active sites where the fusion occurs are near the surfaces of the TiD_x foils. A different assumption, also reasonable, is that the deuterium loading of the foils produces active sites that are uniformly distributed throughout the TiD_x foils. Note that some of the foils used in these experiments are only 25 μm thick, whereas the range of 3.0 MeV protons in Ti is 60 μm (my calculation). So in actuality, these two alternative assumptions will not have very different outcomes. It doesn't matter very much if the protons are produced only near the front and back surfaces of the foils or uniformly throughout the foils. The main point of the argument is that the protons produced even at the back surface of the foil can escape from the foil's front edge and enter the plastic scintillator.

In addition, the angular distribution of the protons from the fusion reaction should be isotropic because presumably the deuterium atoms are at rest in the crystal lattice. The proton and triton are emitted 180^o apart, but there is no preferred angle in space for either particle. Thus, the angular spread of the protons that enter the scintillators will be broad, limited mainly by the distance, i.e., the solid angle, between the active site in the Ti and the scintillator. (Note that the authors also used 250-μm thick TiD_x foils, which are much thicker than the range of the protons. It would have been interesting to see the energy spectra from these foils).

My point in this analysis is that the energy distribution of the detected protons should be broad, not narrow as was observed in the experiment. My approximate calculation for protons produced uniformly throughout a 25-μm Ti foil, with a maximum emission angle of 60^o, gives an energy spread to the purported proton peak of ~0.9-3.0 MeV (An accurate calculation should integrate over all positions of the active sites and over all emission angles that would intercept the plastic scintillator). The resulting energy distribution should be similar in shape to the spectrum of a thick alpha-particle source, with a sharp edge at 3.0 MeV and a relatively flat distribution all the way down to ~0.9 MeV, which according to Fig. 2 of the paper, is at the threshold of the scintillator. In other words, if the particles observed are protons, the TiD_x spectrum should have a sharp edge at an abscissa value of about channel 25 and be flat all the way down to ~ channel 1, markedly different from the sharp peak actually shown in Fig. 5 of the paper. This conclusion causes me to question the assignment of the observed peak to protons from fusion.

I have some other comments and questions about the experiment and data analysis presented in the paper of Jones et al.:

(1) Range-Energy relations are accurately known for protons. Thus, inserting the 19-μm Al degrader foil, as the authors did, should nail down the energy loss and unequivocally identify the particle as being a proton, as opposed to other possible light nuclei from fusion. This did not happen in the paper. Instead, the authors found that the result was "most consistent with protons, although the energy loss is somewhat smaller than might be expected" (0.3 MeV vs. 0.5 MeV). They then offered an ad hoc

argument about light reflections from the Al foil affecting the response of the scintillator, as a way to explain away the difference of their measurement and the expected energy loss. Thus, I find that the energy-loss argument they presented actually sheds additional doubt on their claimed identification of protons.

(2) As noted, the authors calibrated the plastic scintillator with ^{241}Am. This nuclide emits a 60-keV γ ray in addition to a 5.45-MeV α particle. It is well known that scintillators are sensitive to γ rays as well as to charged particles, with low-energy γ rays producing a noticeable photopeak. Did the authors take account of this γ ray? For example, did they do an experiment where they inserted a thin absorber between the ^{241}Am and the scintillator, to stop the α particles and check the scintillators' response to the γ rays? Have they demonstrated that their calibration curve in Fig. 2 is not affected by the ^{241}Am γ ray?

(3) The authors mention the observation of bursts of events. Have they (or others) investigated if these bursts include photons, such as γ rays, x rays, or visible light? Would their scintillators record such photons?

(4) The authors state on page 10 that their observed fusion yield increases with time, then decreases. They call this behavior remarkable – to them it seems to be a signature of the fusion process, but I do not understand their reasoning. They make the additional statement that "A prosaic (non-fusion) explanation for these data must explain this remarkable time dependence." I would comment that a fusion explanation for these data must *also* explain this remarkable time dependence. They do not offer any such explanation in the paper. To me, these bursts remain another mysterious aspect of cold fusion.

(5) The experiment using Si detectors to look for proton-triton coincidences is interesting in principle. But the data that are presented look ragged. In the oscilloscope traces shown in Fig. 11, it is not obvious to me why the triton time distribution from the upper detector should be broader than the time distribution for the proton, which had to traverse the TiD_x foil before entering the lower detector. Also, the 3D coincident energy spectra in Fig. 12 don't look quite right. One should be able to extract the energy distributions of the two particles by projecting these data onto the x and y axes. The distribution of counts in Fig. 12 does not look as if they would give defined peaks centered at 0.9 and 1.7 MeV.

(6) So far as I could recall, papers by Jones and his colleagues are the only ones that stress the fact that they do their cold fusion experiments in an underground lab, to try to reduce the background from cosmic rays. In another of their papers, they state that their lab has ~ 100 m rock overburden plus passive shielding that was added inside the lab. It would be interesting to know: What is the composition of the cosmic ray flux in the lab? Is the flux of pions reduced to zero? What is the flux and energies of the muons? Do they have any ideas of the types and yields of spallation nuclear reactions induced by these fluxes? Can muon-catalyzed deuterium fusion contribute any events to their data?

In summary of this part of my review, I believe that my analyses and comments about the data by Jones et al. raise serious doubts about the so-called definitive identification of protons from cold fusion.

B. Concerning the work of Jones et al. on Neutron Emission from Metal Deuterides.

This paper by Jones et al. also was presented at the Tenth International Conference on Cold Fusion. It is cited in DoE31 as Ref. 90.

The experiment was designed to search for the fusion reaction,
d + d $\rightarrow$ 2.45-MeV neutrons + 0.82-MeV ^{3}He. Plastic scintillator was used to detect the fast neutrons while ^{3}He-filled proportional counters were used to detect slow neutrons that were thermalized in the plastic. Evidence for neutron production was claimed., with "sufficiently high repeatability", even though two of the nine experiments gave rates that were consistent with the background rate.

I have not had sufficient time to consider many aspects of this experiment. However, I do have a few comments:

(1) Neutron backgrounds from energetic cosmic-ray muons can be important in such an experiment. Are all such effects taken into account by the veto shield and by the control experiments done with normal hydrogen instead of deuterium?

(2) Capture of a thermal neutron in the ^{3}He counter produces an α particle with a well-known energy spectrum. This spectrum can be used as a diagnostic for the presence of neutrons. I am surprised that the authors are not taking advantage of this additional identification tool.

C. On the Production of ^{4}He.

There have been extensive experiments done to search for ^{4}He production in deuterated metals. In fact, cold-fusion advocates claim that it is the reaction d + d $\rightarrow$ ^{4}He + 23.8 MeV of energy, which is correlated with the observation of excess energy from their experiments, as opposed to what a nuclear physicist would expect from d-d fusion, $\rightarrow$ p + t or n + ^{3}He. In fact, a good part of the discussion in Ref. DoE31 is devoted to the observation of ^{4}He.

It sounds as if the "^{4}He seekers" have been careful in their experiments and have devoted much effort to reducing backgrounds and to eliminating the ingress of ppm levels of atmospheric ^{4}He in order to measure the ppb levels that they claim are consistent with the energy releases from their cold-fusion chambers. However, it should be noted that results from different experiments are not always in accord. On pages 22-23 of DoE31, it is noted that experiments at SRI observed production of excess heat and of ^{3}He and ^{3}H, but not of ^{4}He. DOE31 notes, with some understatement, that "…the apparent absence of ^{4}He…is of concern." So even the evidence for ^{4}He production from cold fusion is not uniformly robust.

Measuring gas concentrations, especially at low concentrations, is not my experimental forté. Yet I must comment on a nuclear physics aspect of a corollary of the claim for ^{4}He production in d-d fusion, namely that the 23.8 MeV released in the reaction does not produce any nucleons or photons but is directly absorbed by the lattice as heat. Such a Mœssbauer-type of resonance absorption at such high energies would be very surprising if not extraordinary, especially when one takes into account the fact that the metal foils used in some experiments are quite thin, e.g., the 25-μm thick foils used by Jones et al.(sections A and B, above).

Cold-fusion enthusiasts have been discussing such a mechanism for years, and some models have even been developed. But have any microscopic calculations been done that follow the reaction step by step and show exactly how the energy is absorbed by the lattice? I note for comparison that microscopic calculations have been very successful in explaining results of neutron irradiations of solids, showing how recoil nuclei are produced and dislodged from their customary positions in the crystal lattice, and following their progress as they move through the lattice. One might ask, for example, if in the cold-fusion reaction, the transfer of energy to atoms in the lattice does produce some recoils.

The main problem with this direct-heat scenario is symptomatic in many ways of the entire history of cold fusion. One begins by proposing a very unusual new mechanism, namely d + d fusion at room temperature, that some chemists and solid-state scientists can accept but most nuclear specialists cannot. However, as one travels down this nuclear-energy path, one finds results that are not in accord with the body of knowledge in the nuclear field. So one is forced to invoke other "new" mechanisms to explain the data. Proponents would call this pathway the route to discovery of new science. Critics would call it a slippery slope.

D. On the Production of Other Radioactive Species in Cold Fusion.

(1) Kevin Wolf of Texas A&M University was a bonafide, respected researcher in nuclear chemistry/physics, who participated in some cold-fusion experiments. Unfortunately, he died several years ago at a relatively young age. He was a colleague and friend of mine.

Cold-fusion advocates often refer to Kevin's work as definitive in demonstrating that radioactive nuclei are by-products of cold fusion. I found a discussion of these experiments in a 1995 EPRI report, entitled "Radiation data reported by Wolf at Texas A&M as transmitted by T. Passell." The report, which is a collection of slides and figures from Kevin Wolf, is a systematic presentation of the observation of γ-ray spectra that are identified as coming from radioactive nuclei in close proximity to Pd (the metal in the cold-fusion cells) in the Periodic Table, Ru, Rh, Pd, and Ag. Passell notes that these observations were a one-time affair. He states that Kevin "was never able to replicate these results, and he never published or reported them at a conference."

In his slides, Kevin indicated that what he saw in the γ-ray spectra seemed associated with known types of deuteron-induced reactions on Pd, such as (d, γ), (d,n), (d,p), and (d,α). He noted that he had seen "two fast neutron episodes" in this particular cell and that is the reason that he did the γ-ray spectroscopy.

In hindsight, it seems difficult to decide if these products arose from cold fusion or from contamination or from some external radiation source. However, it certainly is inaccurate, in view of the non-reproducibility of these particular results from Kevin Wolf, to characterize them as being strong evidence of cold-fusion induced nuclear reactions.

(2) I began this Section with a discussion of Wolf's work, to contrast it with later rather unusual claims for discoveries of radioactive nuclei from cold-fusion experiments. Wolf's analysis was very much in line with what other nuclear scientists would have done, had they obtained the same results. If his results had been reproducible, he would have had argue about the mechanism for introducing energy into his system, e.g., cold fusion, but the products would have been understood in terms of well-known nuclear processes, such as (d,n) and (d,p), indicated above in D1.

By way of contrast, I now briefly discuss two other papers:

(a) The paper by Iwamura et al. presented at ICCF10 (Ref. 47 in DOE31) does an exhaustive job of using a variety of modern analytical chemistry methods to identify elements produced on the surface of coated Pd cold-fusion foils. There are two very unusual aspects of this work: (i) The energy source is gas pressure, permeation of D_2 gas through the foils into vacuum. (ii) The claim is made that if Cs is coated on the metal surface, it is converted into Pr; if Sr is coated on the metal surface, it is converted into Mo. The analytical results, from a variety of techniques, such as mass spectroscopy and electron spectroscopy, are very nice. It seems difficult at first glance to dispute the results. However, the Japanese workers conclude, not that the elements in question are constituents from the interior of the Pd that migrated to the surface, but that they are the products of sequential nuclear reactions, in which changes of atomic number and atomic mass of 4 and 8 are preferred.

From a nuclear physics perspective, such conclusions are not to be believed. The energetics of merging two deuterons in a fusion reaction are tough enough. Merging four deuterons with a heavy nucleus such as Pd is not to be believed, especially when no evidence is presented for any nuclear products such as Y, Zr, and Nb that are between Sr and Mo. Yet people in the cold-fusion community are citing this paper as further evidence for exotic new nuclear phenomena.

(b) Bernadini et al., in a paper at ICCF8 (Ref. 76 in DOE31) present results from γ-ray spectroscopy (the spectra are not shown in the paper and the numbers of counts under each peak are small, ~ 20 or less), and claim that Sc radioisotopes were produced from Ti foils. In some ways, this claim is similar to that of Wolf, above. It remains to be seen if it will be verified by others.

These researchers note an interesting fact, namely that they did not see any radioactivity when they ran a cell in H_2O, while they did see activity in D_2O. If true, this result would indicate that the deuterons in the cell can acquire energy but the light hydrogen nuclei cannot. This statement is different from saying that d + d can undergo a nuclear fusion reaction, but p + p cannot (because formally written, $p + p \rightarrow {}^2He$, which does not exist; a competing reaction, $p + p \rightarrow {}^2H$ + positron + neutrino, is very slow). If energy is deposited in the system by the electrolysis, then one might expect that nuclear reactions on Ti could be initiated either by d or p, since the Coulomb barrier is the same for both isotopes of hydrogen.

II. Non-nuclear Searches.

I note here two examples that I came across of results from cold-fusion experiments that may be indicative of processes that are not nuclear in origin.

(1) Lipson et al. in a paper presented at ICCF10, noted the emission of intense low-energy x rays from Ti at ~1.4 keV when they established a deuterium-gas glow discharge with a Ti cathode at low voltage, 0.8-2.5 kV. They were able to image the x-ray emission using a camera obscura. Although these authors claim to have observed 3-MeV protons in solid-state plastic track detectors (an established nuclear detection technique, especially for heavy ions), I feel compelled to note that such low x-ray energies are really not characteristic of nuclear phenomena but of transitions in the extra-nuclear electron shells.

(2) Arata and Zhang (Ref. 65 in DOE31) showed electron micrographs of Pd-black, highly deuterated and non-deuterated, and commented that it was “startling that no crystal damage seems to exist” even though “the highly deuterated sample was heated to a very high temperature and generated huge excess energy.” However, it was noted that the micrographs showed different particle shapes for the deuterated and non-deuterated samples. This paper raises the following question: Have systematic structural studies been done of the structures of metal electrodes before and after the proposed cold-fusion reaction had been completed, with light hydrogen and with deuterium, to look for bulk damage of the metal, and to determine any changes in the locations of the metal atoms (e.g., by x-ray diffraction) and of the deuterium atoms (e.g., by neutron scattering) in the lattice?

IV. My Evaluation.

I find in summary that, even after all of the work that has been done, the case is spotty for the existence of the cold fusion phenomenon. I am not convinced by the evidence that I have seen, especially after the past few weeks of intensive reading. I note here that in addition to many of the references provided by DOE and in reference DoE31, I have looked online at the papers presented at recent International Conferences on Cold Fusion and have read selected ones that dealt with particular nuclear phenomena.

Some papers that I read seem to be the result of very careful work, while others are not. What is especially troubling to me is that in several papers that I noted, the authors seemed so intent on using their results to prove the existence of nuclear reactions in their “low-energy” experiments that they (a) overlooked certain aspects of their data that were pertinent to and possibly inconsistent with the conclusions that they were drawing, (b) did not do as complete a job as they could have done in designing their experiments, and/or (c) arrived at conclusions that not only are *“(not) consistent… with* *any other nuclear reaction mechanism that appears in textbooks or in the mainstream nuclear physics literature*" (page 2 of reference DoE31), but also contradict what is known from decades of studies of nuclear reaction mechanisms. I have tried to allude to each of these concerns in my comments above.

The purpose of this DOE review in which I am participating is to decide if a national program of cold fusion research should be funded by the Office of Science. The proponents of this research clearly believe that they have made their case. As I said above, I do not concur. I note that reference DoE31 itself contains several instances where it points out conflicting results from different cold fusion experiments; e.g., on page 24, "this discrepancy .. is large, and this difference has not been resolved." And DoE31 also describes the conclusions drawn from several experiments in terms that are not at all definitive, e.g., "apparently" or "it seems that" or "we conclude tentatively that... ".

Playing the Devil's Advocate, I might also ask the proponents of this research the following questions: (1) "If a cold fusion program were to be funded, what would you propose to do that is new, that you have not already done during the past fifteen years?" (2) "What do you hope to learn that is significant, scientifically (and for society)?" This latter question is especially important, I think, because we cannot ignore the fact that from its beginning, cold fusion has not simply been advertised as a possible new scientific phenomenon that requires elucidation, but as a potential, limitless supply of energy.

One must guard against the hype (and the political pressures) that have accompanied the cold fusion debate. Even if cold fusion were to turn out to be real, it must be realized that a phenomenon that so far has been observed sporadically and often in a non-reproducible, i.e., uncontrolled, manner, will not easily be transferred into the marketplace.

Playing the Devil's Advocate again but this time from the other side of the argument, I note that the DOE Panel that will evaluate the external reviews, such as mine, has a difficult task. As I have indicated a few times in my comments, unless one decides from the available evidence to rule out completely any possibility of cold fusion's being real, there are interesting questions that do arise when one considers at least some of the papers that have been published about cold fusion. It may well be that future, carefully planned experiments will prove that cold fusion can occur or, more likely in my estimation, cannot occur. In no case do I see the need for a national program. However, it might be that a few carefully selected experiments, described in rigorously written and rigorously refereed proposals, could be undertaken – on an experiment by experiment basis. Such a decision would be up to the DOE Panel and to DOE management.

Review #8

Hagelstein et al. have focused rightly on providing a summary of the strongest experiments in the study of highly deuterided Pd (many of which originated with or were repeated by the McKubre team at SRI)--I will elaborate below on why this focus is the right one.

This paper especially highlights what we know now that we didn't know in the six months post-23 March 1989, when the DOE-ERAB made its first assessment of the state of the field.

The parametric understanding of what high deuteron loading levels (x) in PdDx are necessary to initiate heat effects and achieve correlatable (if not necessarily overwhelmingly definitive) levels of 4-He simply were not known in 1989 (or even into the early 1990s). The importance of triggers (current/heat jumps) and interfacial flux of deuterium were also poorly understood and were irrelevant in any event until high D loading levels were achieved.

These extreme experimental measures point to the importance of nonequilibria and critical-state phenomena -- two areas that are still poorly understood in most physicochemical systems.

These deuterium loading levels ($x > 0.9$ at ambient temperature/pressure) are well past the $x \sim 0.67$ characteristic of the beta-Pd-deuteride phase and are not trivially achieved in the lab. Ample evidence points to the criticality of the quality of the Pd in achieving such high D loadings (even pre-cold fusion).

Most experiments in the area of anomalous effects in highly deuterided Pd were performed by researchers who simply had no materials science understanding of their starting Pd or the PdDx they created.

A past prominent member of the ERAB, John Huizenga, performed his own measure of a meta-analysis (which is commonly done in biomedicine to discern trends of truth in a sea of less-than-clear clinical studies) by looking at *all* cold fusion experiments. In that most of these experiments were "negative," he felt the field could be dismissed. But in light of how much materials science was (1) not done; (2) not known (e.g., the segregation of Pt-group elements (dissolved in Pd metal at tens of parts-per-millions) to the surface of highly hydrided and deuterided palladium was unreported in the literature until Rolison and O'Grady, *Anal. Chem*. 1991, *63*, 1697); and (3) is still not known, not all experiments are created equal. It is unscientific to give all experiments equal weight.

If the bottom line is that experiments in which $x > 0.95$ in PdD_x (at room temperature) give anomalous effects reliably (even if achieving that high x is very difficult and very dependent on the materials science of the Pd), while heat balance is attained for $x < 0.9$ in PdD_x (or when using PdH_x at all x), we've got the start of science.

...but with all the above said... these experiments are frustrating and difficult, and require expertise that cross-cuts physics, materials science, electrochemistry, as well as analytical chemistry of breathtaking difficulty. The two most difficult things any scientist can be asked to do are trace analysis/mass balance and calorimetry. Most scientists simply aren't good enough to do extremely demanding experiments in every aspect of the research -- and highly deuterided palladium seems unwilling to cut us a break at any stage.

Review #9

I have evaluated the experimental evidence for LENR in metal matrices as presented in the summary document, "New Physical Effects in Metal Deuterides" and various additional references provided by the DOE. My comments are in response to the two questions posed in the letter that I received from Patricia Dehmer and Dennis Kovar.

(Determine whether the evidence is sufficiently conclusive to demonstrate that such nuclear reactions occur.

1. Excess Heat

Evidence for excess heat in LENR experiments is compelling and well established. As is stated in the summary, "…excess heat has been observed with a variety of calorimeters based on varying operating principles and by different groups in different labs, all largely with similar results." It is this effect that initially captured the interest of the scientific community in 1989 because it was reported to be far in excess of what would be produced by any known chemical reactions. Quantitative analysis of the excess heat effect in many LENR experiments has yielded values of hundreds of ev/atom of palladium in electrochemical loading experiments. SRI reported 450 eV/atom of Pd in a closed cell flow calorimeter. This calorimeter employs redundant temperature sensors that operate on different principles thus minimizing the possibility of systematic errors. The SRI group has done an impressive job of quantifying the sources of uncertainty in their measurements and propagating their errors throughout their calculations.

Since 1989 much has been learned about the necessary conditions required to produce excess heat in LENR experiments. The original electrochemical cell and method employed by Pons and Fleischmann was designed to permit many experiments to be conducted as well as large variations in experimental parameters such as current density, which are not possible in closed calorimeters. A systematic study of the many variables in these experiments was enabled by this approach and led to an understanding of some of the requirements for producing excess heat. The downside of using an open calorimeter is the complexity of the data analysis. Pons and Fleischman used heat calibration pulses to calculate the heat transfer coefficient of their electrolysis cells with high accuracy (better than 1%). Control experiments using light water or platinum electrodes in heavy water exhibited no excess enthalpy generation. It should be noted that one of the best control experiments is a palladium/heavy water experiment in which no excess enthalpy is generated. The original Pons and Fleishmann time series thermal data have been examined independently by Wilfred Hanson using multiple statistical methods and found to be correct.

It is now clear that loading level and current density thresholds are required in order to observe excess heat in these experiments. The values are consistent regardless of the approach used and the laboratory where the experiment was conducted. Early failures to reproduce the heat effect were, in part, due to not meeting these requirements. It has also been found that thermal and current density transients, which are thought to effect the chemical environment such as deuterium flux, can trigger heat "events". SRI has published an expression for the correlation between excess power and current density, loading, and deuterium flux. These discoveries have led to a better understanding of the phenomena and more reproducibility.

2. Helium

The high levels of excess heat suggested that a nuclear process might be occurring in LENR experiments. Various attempts have been made to detect the expected nuclear "ash" or radiation with mixed results. One of the more compelling examples is a quantitative correlation between excess heat and helium. The first such experiments were conducted by Miles in glass vessels that were shipped out of state for analysis. The quantity of helium found in these studies was below ambient laboratory air and there was a concern about helium diffusion through the vessels. That being said, Miles employed several blanks and controls and demonstrated a statistically significant correlation between excess heat and helium. This lead to Miles and others to perform experiments in metal containers. The correlation was confirmed and consistent with a D + D reaction resulting in helium and 23.8 MeV in the form of heat. No existing theory can account for this reaction.

3. Nuclear Emissions

There have been many reports of nuclear emissions from LENR experiments. The measurements involved are highly complex and subject to interferences and artifacts. Some of these experiments appear compelling and are worthy of thorough review by qualified experts who understand the intricacies of these types of measurements. As I don't fall in this category I will defer to my colleagues who are.

(Determine whether there is a scientific case for continued efforts in theses studies and, if so, to identify the most promising areas to be pursued.

Electrolytic experiments are extremely difficult to conduct properly and are not geometrically compatible with many detectors for radiation or nuclear particles. This explains the shift to non-electrochemical approaches, which should continue. Emphasis should be placed on developing theories that explain existing data and guide future experimental work. New experiments that test the underlying principles of the theory should be performed.

The body of work that has resulted from LENR investigations is formidable and worthy of attention of the broader scientific community. It is unfortunate that a few vocal individuals have manage to stigmatize this field and those working in it. The implications of this work, if correct, could be profound. Other nations have pursued LENR and continue to do so. Further work that would add to the understanding of LENR is warranted and should be funded by US funding agencies.

Review #10

Preamble

It has been about 15 years since the first ERAB report appeared and it is appropriate to examine the work in the area of cold fusion (or LENR) that has been carried out in the interim. A fair appraisal can be clouded by the cultural problems in this field. There are "true believers," whose judgment may be clouded by a confirmed belief in the reality of chemically driven nuclear events and the perceived need to defend the field. There are also the "confirmed disbelievers," who are so bothered by the fact that the results do not follow the established nuclear paradigm that they won't even examine the results, a position that is equally dangerous. Many papers (~3,000) have appeared during this time period, making a complete evaluation very difficult, especially in the limited time available. However, a reasonable picture can be obtained from the review by Hagelstein, McKubre, Nagel, Chubb, and Hekman (HMNCH) and some of the references therein, indicated in what follows as (ref. x) and the presentations at our meeting. Nevertheless, there are real difficulties in assessing the work. As Antoine Lavoisier wrote in 1784, when faced with an analogous task, "The art of concluding from experience and observations consists of evaluating probabilities, in estimating if they are high or numerous enough to constitute proof. This type of calculation is more complicated and more difficult than one might think."

My comments focus on the calorimetry and electrochemical aspects, which are closest to my area of expertise.

Comments on calorimetry

The original Pons-Fleischmann (PF) experiment introduced the basic idea of an electrolysis cell with a Pd cathode, a Pt anode, and a D_2O-LiOD electrolyte, which was said to produce excess power during electrolysis. This excess power was measured by temperature rises (the measured variable), which can be converted into units of power by straightforward, although often complex, treatments. The PF cell was an open one, so corrections had to be made for products, D_2 and O_2, that escaped the cell and any liquid carried out as spray, as well as D_2O additions to make up for losses. The heat balance under these conditions is complicated and open to systematic errors, e.g. those that arise from any unrecognized D_2 - O_2 recombination in the cell.

The recommendation of the 1989 ERAB report for calorimetry and excess heat was:
"The Panel recommends that the cold fusion research efforts in the area of heat production focus primarily on confirming or disproving reports of excess heat. Emphasis should be placed on calorimetry with closed systems and total gas recombination, use of alternative calorimetric methods, use of reasonably well characterized materials, exchange of materials between groups, and careful estimation of systematic and random errors. Cooperative experiments are encouraged to resolve some of the claims and counterclaims in calorimetry."

In the years since the report numerous other calorimetric experiments have been undertaken, with either the original PF Pd/D_2O materials or with others, in a number of different calorimeter arrangements. Many of these experiments, like those at SRI, have involved closed cells, with a great deal of care taken in the design and calibration of the calorimeters. These closed systems, in which the electrolysis products recombine in the cell, especially when used with a flow calorimeter, are easier to understand and less prone to systematic error. The SRI experiments have also been undertaken to assess the effect of variables (e.g. level of Pd loading by deuterium, current density) on the observation of excess heat effects. The review paper (HMNCH) contained selected experiments with few experimental details about the exact cell parameters, calibration procedures, and complete time histories of cells, both those that showed excess heat (i.e. excess power) bursts and those that did not. Additional details were provided after the committee meeting, however, although there was not the time to do a detailed analysis of these experiments.

Most of the reported excess heat affects are reported as excess power (in W or W/cm^3) at a given time. However instantaneous excess power is not the real issue, even in closed cells, since it does not account for possible accumulation of materials that later recombine. For example, certainly at high current density, Li metal can plate out on the cathode and later react with oxygen. Even if such reactions don't occur, it is unlikely that the electrochemical cell is at a true steady state during the whole time of operation, so the excess energy (J), which involves integration of the power with time from the start of the electrolysis, is more meaningful, but not often given. It is also misleading to report results in terms of volume or moles of the Pd cathode, e.g. W/cm^3 or MJ/cm^3 or W/mol or MJ/atom Pd, since there is no evidence that the effect scales with electrode volume or weight.

The careful calorimetric experiments at SRI show occasional "heat bursts" (really measured temperature rises) that occur with high D loading (above 0.95 D/Pd) and high current densities (above 265 mA/cm^2) and seem to be initiated by changes in the current mgnitude. However, even under these conditions, the effect is not always seen. For example, a second batch of Pd, presumably prepared in the same way by the manufacturer (Englehard), did not show such effects. The presentations suggest that this is caused by subtle, significant metallurgical changes in the Pd, but, in any event, a reproducibility problem in these measurements remains. The needed parameters like current density and loading also appear to depend upon things like electrode nature, size and geometry, since other papers report excess heat effects at 64 mA/cm^2 (ref. 75) and even 12 mA/cm^2 in the experiments described by the ENEA group during the review.

An important point made from the calorimetric experiments is that the excess energies observed cannot be accounted for by chemical processes, e.g. by build up of some chemical reactant during the charging phase (where small endothermal changes might not be detected but are significant over time) followed by the triggering of a discharge process where a heat burst is observed. If this is true, then nuclear processes must be invoked. The levels of excess energy obtained in the SRI experiments were modest, ranging from 0.01 to 1.12 MJ (representing 0.2 to 3.9% of the input energy).[1] Some of the results, e.g., cell P1a that produced 0.07 MJ (2.1% of the input energy), might be accommodated by a chemical reaction. For example, if one assumes that Li metal plates on the cathode and later reacts with oxygen from the anode, the reaction enthalpy would be roughly 300 kJ/mol Li, so this amount of energy would require about 1.6 g of Li. However for levels of excess energy in the 1 to 10 MJ region (or more), as reported in a number of studies, it is difficult to find a chemical explanation. Either one must explain these results by flaws in the measurement or seek a nonchemical (i.e. nuclear) explanation. The reports of heat evolution after the current has been turned off clearly indicate that some storage process is occurring, but do not necessarily negate a chemical storage process (and are indeed difficult to understand in terms of a nuclear process if high loading and pressure is needed).

One must, however, consider the difficulty of these calorimetric experiments and of operating electrochemical cells like those used in the measurements for periods of time of weeks to months. The system is complicated, with gases evolved as bubbles from both electrodes, probable film formation (e.g. deposition of Li) on the cathode and changes in the cathode resistance with time, with possible changes in the heat distribution in the cell over time. The observed temperature changes corresponding to the "excess heat" effects are frequently rather small (~ 0.4 to 0.5^{o}/W), so, for example, the excess heat burst shown in Fig. 1 in HMNCH represents a temperature change of about 0.3^{o} and even the larger effect shown in Fig. 3 is below 3^{o}. Small problems with the calorimetry (e.g. nonuniformities in temperature in the calorimeter) could lead to such effects, although none were apparent from the materials supplied and certainly several groups have been doing these measurements for a long time and have worked hard to minimize such effects. Nevertheless, it is disconcerting that the magnitude of the effects observed have not shown an apparent increase in magnitude in the 15-year period since the first reports. The committee was given, after the committee meeting, some new, unpublished work that claimed very large power outputs and temperature changes (a burst of up to 34 W and temperature change of 60^{o}). However there were insufficient details and information about this experiment and any controls to assess this work, even superficially. The rather low temperatures attained in most cases also suggest that the electrochemical approach has doubtful applicability for practical energy (heat) generation.

There is also the problem of reports of excess heat from systems very different than the Pd/D_2O system, apparently from groups that have had experience with calorimetry. For example, Storms, who wrote a good discussion of calorimeters and their possible errors (ref. 3), reported excess heat effects with the Pt/D_2O system (ref. 82). Note that in other reports systems with Pt cathodes are considered controls or blanks in calorimetric measurements. In this work, excess power bursts with a Pt cathode, analogous to those reported with the Pd/D_2O system, were seen. Storms also reports excess power from Ag/D_2O.[2] Similarly, there have been reports of excess energy by several groups in a light water system: Ni/H_2O, Na_2CO_3.[3] While these results do not negate the careful work on the Pd/D_2O system, they are disconcerting. Either they are correct, which now brings to question the nuclear mechanism proposed that is largely based on deuterium in a Pd lattice, or they are incorrect, which then exposes serious problems with calorimetric measurements of the type reported.

In a general summary of the calorimetric results, the observation of sudden and prolonged temperature excursions (bursts of excess heat), has been made a sufficient number of times that, even if not totally reproducible, still have not been explained in terms of conventional chemistry or electrochemistry (a conclusion also made in the 1989 ERAB report). However the systems are sufficiently complicated, the measurement sufficiently difficult, and the effects sufficiently small, that it is difficult to conclude from these effects alone that nuclear processes are involved. Even with all of the careful work that has been done on electrochemical cells and calorimetry, the system is still not under experimental control, in the sense that one knows exactly the materials needed and the operating conditions to get the same results, even semiquantitatively, every time.

Comments on ^{4}He production in electrolytic cells

The 1989 ERAB reported recommended: "A shortcoming of most experiments reporting excess heat is that they are not accompanied in the same cell by simultaneous monitoring for the production of fusion products. If the excess heat is to be attributed to fusion, such a claim should be supported by measurements of fusion products at commensurate levels." At the time of the report, these fusion products were assumed to follow the usual branching ratio of the D-D reaction, so that the implication was that production of neutrons or tritium would be investigated.

A number of studies have focused on finding nuclear products. While reports of neutron and tritium have appeared, none have claimed a correlation with the excess energy produced. The main emphasis of HMNCH is on detecting ^{4}He and correlating these amounts with the heat produced in electrochemical cells. The work described seems to have been done carefully and with attention to prevention of contamination. A difficulty with tracking ^{4}He is the fact that it appears in the electrochemical experiments in very small amounts, often below the ~ 500 ppb amounts in normal air. In fact, laboratory air, because of the possible use of He nearby for things like superconducting magnets for NMR, as the inert atmosphere in glove boxes and as a carrier gas in chromatography, often contains higher amounts of He. The presence of hydrogen is also known to promote the desorption of He from glass. Another problem with the proposal of ^{4}He as the major product, as is recognized in the review, is that the proposed D-D branching ratio must be assumed to be very different from that in previous studies of deuterium fusion and the absence of gamma rays, which would accompany this route, must be explained.

Response to the Charges

Is the evidence sufficiently conclusive to demonstrate that such nuclear events occur?
At this stage, I think the evidence suggests the possibility of such events, by cannot be considered conclusive beyond a reasonable doubt, for reasons alluded to above.

Is there a scientific case for continued efforts in these studies? Identify promising areas.
I don't think there is a case for focused funding in this area. After 15 years and at least $60M spent on this area, it is doubtful whether there is much to be learned from more of the same type of research. However there remain interesting unanswered questions about these systems and DOE should be willing to entertain novel proposals in this general area. For example calorimetry with anodes that can oxidize D_2 (fuel cell anodes), if they can be made to operate in closed cells at the needed current densities, especially with a cell resistance minimized by close spacing of anode and cathode, would be interesting. These experiments they would eliminate O_2 evolution and possible attendant reactions and also probably decrease side reactions like Li deposition.

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[1] McKubre, M. C. H., Proc. ICCF10, 2003, slide 13.
[2] Storms, E.,"Ways to Initiate a Nuclear Reaction in Solid Environments," American Physical Society Meeting, March 15, 2001, Seattle, WA.
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Review #11

Evaluate the experimental evidence presented for the occurrences of nuclear reactions in condensed matter at low energies (less than a few electron volts).

I would like to preface my remarks by saying my area of technical expertise is in the area of Material Science and I will focus my comments primarily to the material science aspects of LENRs.

For the electrolysis experiments with palladium cathodes and heavy water, the correlation of excess heat with helium measurements is compelling particularly given the control experiments with light water. Calorimetric results for palladium electrodes do not consistently show excess heat, but the care in which the measurements are done for experiments that do show excess heat are convincing evidence of low energy nuclear reactions. The striking differences between experiments conducted with light and heavy water also point to a nuclear phenomenon.

There seems to be a growing understanding of what makes a 'good' or 'bad' cathode. The electrochemical process produces high Pd/D ratios (greater than 1) and this implies high fugacity for D and effective pressure of up to 15 Kbars. When cracks form in the cathode, deuterium can leak out of the cathode and the deuterium loading is insufficient to promote the low energy nuclear reactions that are observed. The Palladium-Hydrogen phase diagram indicates two solid solutions with a significant increase in lattice parameter as hydrogen content increases. This lattice expansion results in significant compressive stresses in the surface and when the yield strength of the Pd is exceeded, dislocations are nucleated to relieve stress. As hydrogen (or deuterium) continues to diffuse there will be a build up of subsurface compressive stresses again from the lattice parameter change and these stresses will now put the surface in tension. If the surface layer is not strong enough to elastically deform, cracks will form to relieve the tensile stresses. This is a thermal diffusive fatigue mechanism. It is observed that Palladium cathodes that work best seem to be less pure than those that are of a higher purity. Boron and aluminum impurities that are beneficial are also expected to strengthen the palladium alloy since they occupy interstitial positions in the lattice. The surface contamination of the Pd cathodes is another area which is claimed to effect reproducibility. There seems to be less consensus as to which contaminates are 'bad' and which are 'good'.

The high energy particle emissions from deuterium loaded foils presented by Professor Jones provide evidence of low energy nuclear reactions from metal foils in the form of high energy particles. Temporal correlation of particles emissions if confirmed by more sensitive measurements would be strong evidence of unexpected solid state mediated nuclear reactions.

The lack of testable theories for low energy solid state nuclear reactions is a major impediment to acceptance of experimental claims. In the palladium electrolysis experiments the means by which helium-4 is formed from D-D fusion and how the ~24 MeV of energy is transferred to the lattice rater than by emission of a gamma needs a more testable hypothesis than has been developed at this point. The focus on octahedral site occupancy for deuterium seems to be misplaced. As the Pd/D ratio exceeds 1, all the octahedral sites are occupied and one would expect deuterium to occupy tetrahedral sites as well as double occupied octahedral sites. Segregation of deuterium at dislocation cores should also be expected and may provide a way to focus energy during dislocation motion. The combination of expertise in dislocation mechanics and physics needed to model this problem is an example of how multidisciplinary the problem may be.

Palladium cathodes containing boron and/or aluminum produce more 'excess heat' than chemically pure cathodes. The explanation for this is the role of interstitials as strengtheners. These interstitials also compete with deuterium to occupy octahedral sites. There seems to have been very little research looking systematically at palladium solid solutions. Interstitial dopants could be used to systematically either increase or decrease the lattice parameter changes as deuterium is charged into the cathode.

Determine whether the evidence is sufficiently conclusive to determine that nuclear reactions occur.

There is strong evidence of nuclear reactions in palladium, and suggestions of reactions in the titanium foil experiments. The body of evidence does not rise to the level of being conclusive at this time. What is required for the evidence to be conclusive is either a testable theoretical model or an engineering demonstration of self powered system that continues to produce heat without an external power supply such that the device would appear to be a perpetual motion machine if not for the nuclear reaction.

Determine whether there is a scientific case for continued efforts in these studies and, if so, to identify the most promising areas to be pursued.

I believe the scientific case has been made for continued studies. For the palladium system, systematic studies of alloying and dislocation effects combined with theoretical modeling may be useful in understanding the parameters that control the observed excess heat effects. More sensitive instrumentation for particle detection and energy determination should be applied to the experiments described by Professor Jones. The confirmation of a solid state catalyzed nuclear reaction would open a new field of basic research. The Mossbauer effect is perhaps the closest example of such an effect, which suggests theoretical models which include atomic isomers.. Experimentally, experiments which include a Pd isotope effect might be useful in looking for an isomer mechanism.

Review # 12

To examine and evaluate the experimental and theoretical evidence for the occurrences of nuclear reactions in condensed matter at low energies; To determine whether the evidence is sufficiently conclusive to demonstrate that such nuclear reactions occur

There exists a large variation in the quality of the work in this field. It is also very easy to find faulty or incomplete measurements in many of the papers published in the ICCF Proceedings. However, I believe that we should concentrate on the small number of careful works for the purpose of assessing an unknown field. In other words, we should look at the best available experiments in order to get more information on whether there is some new physics involved.

There are two kinds of experiments that address the occurrence of nuclear reactions:

a) Study of low-energy nuclear reaction in the presence of electronic screening in a solid-state environment (e.g., charged-particle emission measurements by Jones). Unfortunately, current measurements are not done professionally, and more work is needed. Nevertheless, this is a reasonable scientific problem.

b) Experiments involving excess power/heat. More careful experiments have been done in recent years (e.g. SRI work). There seem to be increasing evidence for the production of excess heat, even though the reason is totally unknown. Reproducibility has been improved, but it still has not reached a satisfactory level. Yes, it is likely that an unknown process (in materials physics or in nuclear physics) is responsible. However, the link to nuclear reaction is still not strong enough at the present time.

In order for (b) to have anything to do with low-energy nuclear reactions, an enhancement of the reaction rate in the solid state has to be verified. Yet the evidence is not conclusive yet. In addition, current understanding of nuclear processes is not sufficient to explain many of the findings in (b).

Compared with the experimental efforts, the theoretical work is even more unconvincing. To make a case for nuclear reactions to happen in condensed matter at low energies as suggested or speculated by experiment, theory has to be formulated to explain (1) the enhanced nuclear reaction rate in the condensed matter environment, (2) the completely different Branching Ratio for the d-d reaction from the gas phase, and (3) the mechanism for the dissipation of the 24 MeV energy through the lattice. None of these has been demonstrated, nor any promising directions have been shown. Because of these deficiencies, one is having a difficult time in understanding the experimental implications. My comments on each of the three areas are given below.

(1) It was mentioned at several places in the documents or presentations that high deuterium loading might result in double occupation of the octahedral site in Pd, and thus bring the deuterium atoms closer together and enhance their interaction. However, it has been shown by first-principles electronic calculations [P. K. Lam and R. Yu, Phys. Rev. Lett. 63, 1895 (1989)] that the lowest energy configuration for two deuterium atoms at one octahedral site is an arrangement along the (111) orientation with a D-D distance of 1.3 Angstroms, which is still significantly larger than that in the molecule (0.74 Angstroms). On the other hand, so far the quantum nature of deuterium in the metal has not been taken into account. Previous electronic calculations did show that the potential well was not harmonic, and the zero-point motion was quite significant [C. Elsasser *et al.*, J. Phys.: Condensed Matter 4, 5207 (1992)]. Most importantly, the two deuterium atoms have to be described by correlated wave functions with a mutual interaction $V(r_1, r_2) \neq V(r_1 - r_2)$ inside the crystal, which is completely different from the situation in usual scattering experiments on the gas phase. These critical issues for a decent theory have been completely ignored so far and would require an interdisciplinary effort in the future.

(2) The most puzzling part for nuclear theory is the lack of neutrons commensurate with the heat production and the complete reversal of the ratio for the reaction channels. This is still the crucial and seemingly insurmountable physics problem that needs to be resolved.

(3) The lack of gamma rays being detected from the sample forced researchers to invent a coupling between the nuclear interaction and lattice vibrations. Being able to write down the equations does not imply physical justifications. An effective interaction normally involves some type of fundamental interactions that lead to the coupling. For example, the effective electron-electron interaction mediated by phonons, through electron-phonon coupling, leads to superconductivity. Under the carpet, the electron-phonon coupling arises from the electromagnetic interaction, one of the four known fundamental interactions in physics. To create a coupling between nuclear interaction and phonons at such a low energy region (namely, the electromagnetic interaction) is beyond one's imagination at the moment.

A series of conjectures is formulated in Hagelstein's paper, but a lot of them appear to be too *ad hoc*. In particular, the phonon mediated site-other-site reaction is, at most, a "conjecture". The exchange of a large angular momentum with phonons is unprecedented. This paper has a lot of holes and is not likely to go through any peer review process of reputable journals. Better theory could be done, however, by considering the points mentioned in (1).

In summary, in my opinion, there is no theory for low-energy nuclear reaction yet. Therefore, the burden of proof lies on experiment. Although there is still a long way to go, the experimental efforts are moving in the right direction to provide a converging conclusion, one way or the other. The current evidence is not sufficiently conclusive to demonstrate that nuclear reactions occur in metal deuterides yet.

To determine whether there is a scientific case for continued efforts in these studies and, if so, to identify the most promising areas to be pursued

I would not recommend a large-scale program on Cold Fusion, but a few carefully selected projects on the relevant science are worth considering. The proposals should go through the normal reviewing process. Some areas are listed below:

Progress has been made in characterizing the Pd electrode over the past 15 years, but more needs to be done to better understand the sample properties. In other words, materials problems need to be addressed, as well as the physics and chemistry of metal deuterides.

It would be crucial to have independent verifications of the "charged-particle emission" from metal deuterides. In other words, more careful measurements are needed to sort out the proposed "screening" effect.

Good theoretical studies on the behavior and interactions of deuterium in metals are also needed. Very few exist at the moment.

Addition comments

The quality of work is so inconsistent in this field, including the work of some key players, which makes it difficult to clear the black cloud and to increase the credibility of the field. Repeated retractions and conflicting experimental results in the past certainly did not help. Hopefully as time on, a few careful studies will provide a definitive conclusion. Unfortunately, that has not happened yet, although some progress has been made.

I found the nuclear reaction aspect intriguing, but not fully convincing. However, our scientific training taught us to be open-minded. Before the answer is available, we should concentrate on the science problems that can be defined. Some of those have been identified in this review.

Review #13

The charges to the review panel were to:

1) Evaluate the experimental evidence presented for the occurrences of nuclear reactions in condensed matter at low energies (less than a few electron volts)

2) Determine whether the evidence is sufficiently conclusive to demonstrate that such nuclear reactions occur.

3) Determine whether there is a scientific case for continued efforts in these studies and, if so, to identify the most promising areas to be pursued.

I have considered the documents and the mail reviews provided to the panel before its meeting on August 23-24, the presentations that were made to the panel on August 23, the discussions among panelists on August 23 and 24 and the documents and responses to questions that were provided subsequent to the meeting. (Jim Horwitz is to be commended for the extremely effective way in which he handled all of the documentation and the queries to cold fusion proponents and their responses.)

I came to the panel meeting with a high degree of skepticism about the "cold fusion" claims and the radical changes in thinking about nuclear physics that they demand. I still retain some of that skepticism after considering the evidence. However, particularly because of what seem to me to be very careful experiments carried out by McKubre and his associates at SRI, I conclude that the answers to charges 1) and 2) above are yes – there is sufficient evidence to demonstrate that very low energy nuclear reactions can occur in condensed matter at rates that are totally unexpected.

It is disappointing that McKubre has not been able to do an integral of the total power in and out from the beginning of an experiment to show that there is a net out. However, the difference of out-power minus in-power integrated over a period of hours at least in a few clearly presented cases seems to greatly exceed the energy that could be stored as chemical energy in the cell. There also seems to be reasonably convincing evidence for He production.

The irreproducibility of the evidence for excess heat generation and He production in different batches of Pd expected to be the same, or in different experimental runs on the same material and the non-predictability of the conditions under which or precise timing at which they will be observed is very disconcerting for a scientific claim. The proponents' assertion that there is reproducibility if 50% (or maybe even less) of experimental attempts indicate at least some excess heat, never mind how much or when it occurs is frustrating to the objective scientist and has some of the characteristics of "pathological science". The lack of understanding of what is happening in the material that makes the results so unpredictable – even after 15 years of effort – is very unsatisfying. McKubre et al have succeeded in parameterizing "necessary" (but not "sufficient") conditions for improved predictability of "success" (for some batches of material) in their high current density electrochemical cells. However, some cells don't work, and cells which do work are quite noisy in their power production, and they "stop working" for no as yet controllable reason. Then there are the gas loading experiments that require no electrochemistry but do require thermal gradients to get "positive" results. The common thread shared by these two very different kinds of experiments is elusive.

In spite of the lack of reproducibility and predictability, positive observations have been made a number of times and by several different groups under what seem to be credible experimental conditions. I conclude there must be something of nuclear origin going on. It defies both the expectation for the d,d fusion rate and its branching ratio and that is a lot of defiance!

In response to charge 3), yes, I think it is important to get to the bottom of the science that is going on, not with some massive attack on it, but in considered support of well conceived proposals submitted to address the scientific issues. In the current state of the field, finding nothing in a given experiment teaches us nothing whether it is in a search for charged particles, neutrons, gamma rays, He, or T. The only normalizing measurement seems to be heat generation. Although electrochemical cells are in my opinion the most convincing evidence that something strange is going on, and although they have been developed to demonstrate heat generation with great care they exclude the important material experimental variable of temperature. I do not believe they are the way to get to the bottom of the science.

Because of the "noisiness" and the unpredictability of heat generation in electrochemical cells it seems to me the central scientific issue must not be in the coupling of d's to Pd in a normal lattice but someway to the defect structure of the solid which is doubtless extremely dynamic under conditions of high d loading (electrochemically or from the gas phase). I think it's time to look at the properties of the material under conditions of high d loading while measuring heat generation and doing this combination as a function of temperature. This sounds like a tall order, but maybe with x-ray scattering the dynamic features of the material can be examined while (and if) heat is being generated. Without the measurement of heat generation I don't think any experiment is going to be convincing. How do you know anything - of low energy nuclear reaction interest such as cold fusion – is going on? It may be feasible to look for charged particles in combination with heat generation, an additional test of the conclusion based on existing data that the nuclear branching ratio is completely different than it is known to be in d,d fusion at all higher energies. But once again, the heat generation has to be measured at the same time. If and/or when the reproducibility of heat generation is 100%, such simultaneous measurements will not be required, but that condition doesn't seem likely to happen soon.

Review #14

Members of the panel were asked to

- Evaluate the experimental evidence presented for the occurrences of nuclear reactions in condensed matter at low energies (less than a few electron volts)
- Determine whether the evidence is sufficiently conclusive to demonstrate that such nuclear reactions occur.
- Determine whether there is a scientific case for continued efforts in these studies and, if so, to identify the most promising areas to be pursued.

RESOURCES FOR THIS REVIEW

This assessment of cold fusion is based on the summary prepared for the review [1] and several of the references it cites; papers provided to the panel members, refs. [2-7]; the nine anonymous referee reports on those papers; the 1989 ERAB report, ref. [8]; material in the presentations to the panel on August 23, 2003; and number of documents submitted after the panel review, refs [9-11].

SUMMARY

EXCESS ENERGY

The production of excess energy through low energy nuclear processes is the central issue in cold fusion. I am not persuaded that such energy has been produced. Although there have been many experiments reporting excess energy since the 1989 ERAB report, all of those that I have read about suffer in varying degrees from two serious shortcomings that were pointed out in the ERAB report.

1) Calorimetry
Excess energy is never more than a small fraction of the energy delivered to the system, typically a few percent, so that a small error in calorimetry can yield a large error in an estimate of excess energy. Unfortunately, experimental results are almost always presented as data on excess power, often starting hundreds of hours after the experiment starts, but not on excess energy. No direct inference about energy can be made from such data—what is required is a complete inventory of energy flow into and out of the apparatus from the moment the cell is turned on until the moment the experiment is terminated. Normal experimental practice requires that the accuracy of such an inventory be determined experimentally by dummy runs, and the experimental scatter is consistent with the known sources of uncertainty.

2) Reproducibility
The lack of reproducibility continues to be a serious problem. None of the important phenomena can be duplicated reliably. This has made it impossible to obtain a quantitative understanding of what is taking place.

NUCLEAR EFFECTS

A second class of experiments seeks to find evidence of low energy nuclear reactions, though not necessarily at the rate required to produce significant excess energy. Although I am not an expert on nuclear measurements, it appears to me that the evidence for "new physics," i.e. nuclear processes that would be fundamentally inconsistent with well-known nuclear physics, is weak. These experiments, too, are not reproducible, which makes the case for "new physics" weaker yet.

RECOMMENDATIONS

I find that the overall situation has not fundamentally changed since1989 when the ERAB report was written: the experiments are poorly executed, the phenomena are not reproducible, and the claims of "new physics" are not plausible. Consequently, my recommendations are similar to the major recommendations of that report:

1) DOE should not establish a special program for energy production by low energy nuclear reactions.

2) DOE should consider supporting proposals for research in this area that are of high scientific quality. However, because this research has been underway with little progress for fifteen years, any such proposals would have the burden of clearly establishing what would be done differently.

<u>DISCUSSION</u>

<u>EVIDENCE FOR EXCESS HEAT</u>

The evidence for excess heat is described in Sections 2-4 and Appendix A of the summary [1].

Fleischmann-Pons experiments

Reference (1) of the Summary paper, (1993), describes details of the calibration of the Fleischmann-Pons calorimeter. This is an open calorimeter in which heat loss occurs through radiation and gas flow. The major source of energy transfer out of the calorimeter appears to be radiation, and the radiation transfer coefficient is measured to an estimated error of 1.4%. However, there is no information about the accuracy of heat flow measurements over periods comparable to the length of an experiment.

The paper presents data on specific enthalpy generation, i.e., excess power, but does not discuss excess heat, i.e. the over all heat budget. The total excess heat is about 2% of the energy delivered to the cell. A 2% effect with a 1.4% calibration uncertainty is hardly evidence for excess heat.

The situation is worse than these figures indicate, for the type of calorimeter—open cell—is subject to numerous errors, as described in the ERAB report. The authors appear to be aware of these problems for their conclusion includes some words of warning and calls for pressurized systems. Consequently, I am surprised that 9 years later, Fleischman reported results using the same type of open calorimeter in reference (4) of the summary paper. Once again, data are shown only for excess power.

I regard this work as seriously flawed.

<u>Closed Cell Experiments-</u>

The Amoco design

Reference [11] describes a closed-cell experiment carried out in 1990 by scientists at Amoco. In a calibration run, 2.5 watts was required to maintain a 10 degree C temperature elevation. Assuming that this amount of power was delivered during the two months of the experiment, the total energy to the cell was over 10 megajoules. The excess energy was about 40 kilojoules, less than half a percent of the energy to the cell. Figure R1shows end-do-end data on net energy flow, but a calibration error less than the width of the plotted points would change the interpretation by giving the baseline a slope. Lacking data on the accuracy of the calorimeter there is no way to the evaluate results.

The SRI design

A much improved flow calorimeter has been developed at SRI and is illustrated in [1], Figure 10. This is a closed calorimeter, an inherently more reliable design than the open-cell design of Fleischmann and Pons. Calorimetry with this design is summarized in [1], Appendix A. The accuracy of the calorimetry is described in reference (27) of [1]. This is an EPRI report. Calorimetry is discussed in its Section 3. The following excerpt is pertinent:

3A.4.3 *Power*
Measurement of total integrated excess energy is important in deciding whether one is observing an energy production rather than an energy storage process. However, the existence question, i.e., whether there indeed exists a phenomenon to observe and explain, is more easily and accurately answered by analyzing the instantaneous calorimetric power balance. Therefore, the experiment discussed here was designed to make accurate and stable measurements of the input and output power so that the difference (denoted here as "excess power") can be explained.

This willing substitution of power for energy seems endemic to the field. However, for the following reasons, I do not regard reports of excess energy as credible unless the accuracy of the calorimeter for measuring energy, as contrasted to power, is understood and well confirmed by actual experiments:

1) Because excess heat is typically only small fractions of the total energy to a cell during a run, small errors in power measurements can become large errors in excess heat measurements.
2) Operating conditions can vary during an experiment, particularly if energy is released rapidly, which makes it difficult to determine the long-term energy balance from power calibrations.
3) Consequently, the only way to reliably determine the accuracy of an experiment is to calibrate the electrolytic cell for total energy-in vs. total energy-out over the entire duration of a run, with a heat profile similar to an experimental run, and to check this calibration a number of times. If the variations in the measurements of energy-in and energy-out for these calibration runs are consistent with the calculated uncertainty based on an analysis of the known sources of error, then one finally has a credible estimate of the accuracy of the method. I have not seen the results of such a procedure.

<u>Total energy release: a puzzle</u>

Reference [9] points out that the SRI group has observed a total energy release that is claimed to be greater than 2000 eV/atom. It is argued that such a release is too big to be chemical and so it must be nuclear. However, such a conclusion seems unwarranted. The well known products of deuteron fusion are not observed in the expected manner, and no credible explanation of the enhanced fusion rates has been proposed. The chemical explanation appears to fail by a factor of 1000 but explanations resting on known fusion processes fail by factors that are enormously larger. There is an obvious a gap in understanding the origin of the large heat releases. This puzzle deserves to be resolved. However, the existence of this puzzle is hardly convincing evidence that the energy source is nuclear.

One can summarize the essential question as follows: Is the electrolytic cell a battery or is it a nuclear energy source? Answering this question would require accurate end-to-end energy measurements, using apparatus whose accuracy has been verified as described in 3), above. Until the reality of excess energy is definitively established, all discussions of its source are hypothetical.

DIRECT EVIDENCE FOR LOW ENERGY NUCLEAR REACTIONS

Section 3 of the summary [1] describes searches for the production of 4He accompanying the release of excess energy. Work by the SRI group is described in [4], and was presented to the panel by Michael McKubre. The evidence is tantalizing ([4] Figure 3). However, ^{4}He is found in only 1/3 of the runs that appear to produce excess heat. If ^{4}He were generated by a nuclear reaction that gave rise to the excess heat, then it should be present all the time. Consequently, one could reasonably regard this as evidence that the excess energy (assuming, for the moment, that it exists) is *not* due to a reaction that generates 4He.

Other evidence for low energy nuclear reactions, but at rates below the levels one would expect for producing excess heat, were presented by Jones. Not being a nuclear physicist, I am unable to analyze these results. However, Reviewer #7 apparently is an expert and has provided a detailed critique, the thrust of which is that none of the evidence for low energy nuclear reactions is compelling.

CONCLUSION

I am struck by the similarities between the situation today and in 1989 when the ERAB report was written: the experiments have obvious defects, the phenomena are not reproducible, and the explanations proposed are scientifically implausible. The rapid heat release from electrolytic cells that is sometimes observed has yet to be understood, but there is no compelling evidence that it is nuclear in origin. Fifteen years have gone by and according to one estimate more than $60 million has been spent on cold fusion research around the world. There is a little to show for this.

My conclusion is similar to the conclusion of the ERAB report: I see no case for a special program to pursue cold fusion but DOE should be prepared to support credible proposals for work in this area. However, such a proposal would carry the heavy burden of demonstrating that it is capable of breaking out of the rut in which the proponents of cold fusion have been spinning their wheels for many years.

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Review #15

Review of “Cold Fusion” Conducted August 23 and 24 at the behest of the Department of Energy-

The August 2004 Popular Mechanics gives an overview of the present DOE review and the cover states that one can build an H bomb in the basement. Also, the article claims that this is a cheap way to make tritium. Clearly this article sets the tone for this field of research, one of paranoia with the added impetus that someone will get there first. It is this aspect of the field that the DOE must somehow deal with.

As one of the reviewers stated, one can never disprove something and this is my feeling about “cold fusion”. The workers are true believers and so there is no experiment that can make them quit. Likewise there is no series of experiments that can convincingly confirm their work or they would have done them by now. Of course their answer is that they have done them, but reproducibility and predictability still eludes them.

The presentations and written material presented such a confused picture that it is almost impossible to tie things together. There are conflicting claims amongst the advocates, inconsistencies amongst seemingly similar experiments and a general feeling amongst the proponents that the “system” is keeping them from publishing their results and getting DOE funding.

The presentations for the most part were not well thought out and didn’t focus on where they felt the field needed funding. The statement was made that they had spent more than $30M so far on experiments by the SRI group and yet they have no catalogue of what worked, what didn't, and when asked where you are going next could not give a plan.

It would have been easy for them to prepare a spreadsheet as given below:

Date Purpose of Run Foil # D Loading Time for Loading Stimulus Result

No such summary was presented which strikes me as a way I try to follow my own data taking. Such a summary needs to be prepared by all workers in this field.

So, my first conclusion is that the reason they have trouble getting papers published is that they present their results in a manner that simply can't be judged by referees. When papers are properly prepared by others, they do get published as shown by some of the references they presented on screening effects, i.e. Eur. Phys. J. A19(2004)283.

To try to understand the evidence for nuclear effects we can turn to the published literature. The first question to ask is what is the magnitude of the cross section at "zero" energy. We can extrapolate the results of R. E. Brown and N. Jarmie PRC41, 1391 (1990) and get about .01mb for the ^{2}H(d,n) total cross section. The work of the Rolfs group NP A465 (1987)150 agrees with this extrapolation and their work goes to lower bombarding energy. Within 10%, the ratio of ^{2}H(d,n)/^{2}H(d,p) is equal down to 7keV in the center of mass. The ratio of gamma yield to proton yield for the ^{2}H+d system has been measured in PRC 31 (1985) 2036 by F. E. Cecil who is often a co-author on the "cold fusion" papers. They make the measurement down to 10 keV in the center of mass and if we extrapolate their result to zero energy we would get roughly a gamma ray yield that is 10^{-5} that of the proton yield so that the cross section for ^{2}H(d,α) is about 10^{-7} mb. We also know that the Coulomb barrier is roughly 312 keV and if we use the normal assumption that the attractive nuclear potential "pulls it down" by 0.7 we would say the Coulomb barrier is 200 keV. These are the known nuclear physics facts that we are to suspend in the case of "cold fusion".

The work of Jones is to me the most complete in trying to answer the charge to the committee to establish whether nuclear effects are produced in "cold fusion". The reason I make this statement is that their present work does not rely on electrolytic cells and the confusion around whether excess heat is being produced. They need no stimulus to get nuclear events but the processing of foils is still an art as they state in the conclusion of their paper in our packet. Jones also does not believe that the ^{2}H(d, α) reaction can suddenly become a factor of 10^{+6} larger and so has focused his efforts since 1986 on the two reactions ^{2}H(d,p) and ^{2}H(d,n). The problem is that his experiments see effects have yielded results that are just above background when properly normalized. Some runs yield no effects. Jones would benefit enormously from collaborating with nuclear physicists engaged in very low level counting experiments such as those for double beta decay because they have worked extremely hard to catalog all sources of backgrounds.

Even in Jones' case, I find it hard to follow his thoughts. For example, he cited in his talk to us the fact that his original Nature paper (338 (1989) 737) concluded that the surface of the Palladium rod had become coated with Fe and new measurements of the electron screening of Fe yielded amongst the highest correction factors known, 400eV, so that he could indeed have been be seeing d+d fusion. However, the current coincidence work uses only deuterated Ti foils and the measured screening factor for Ti is less than 30eV so that if I apply the reaction rate obtained from the Nature paper to the present Ti foil work he should not see any events. Also, in my reading of the Jones paper given to us, from the tenth ICCF, page 12, figures 12 and 13 make no sense since they show only events arising within their boxes of expected triton and proton energies, and no experiment can be this clean. He has now provided us the actual picture with all events one sees more events not in the boxes than in the boxes as I would expect, simply because I would expect events of all energies since the triton reactions can occur anywhere in the foils and so the triton events should go down to "zero" energy due to the thickness of the foil. The extensive massaging of the data does not do the authors credibility any good within the community of physicists. As I said above we have much more experience doing very low counting rate experiments in the nuclear physics community than we did 15 years ago and so data that looks unnatural will not be published.

The ICCF Jones paper also used a photomultiplier and plastic to look for protons. It has some inconsistencies in the presentation that need to be understood. The most obvious one is to be found in

Figure 7, where they put an Al foil between the Ti stack and the plastic and argue that the fact that the glass events have shifted down in energy shows that the glass events are protons. But the peak in the plastic should have shifted down also. Also, the plastic events should go all the way down to the cutoff because there is no reason why protons can't be generated throughout the foil. Also, in Fig 9 we have a time evolution of the process, but don't know how long after the foil is loaded one sees excess events. If they would present results from the time the system is mounted until the end it would be easier to follow the results.

I have tried to use the extrapolated cross sections given above and the number of deuterons in the foil to calculate the event rate expected. This calculation is made very difficult because one must make an assumption about the d loading and also the number of particles per second that would have to have a nuclear interaction to give the observed counting rate in Figure 9. To obtain the presented yield, 1.3×10^8 particles per second must interact to give 2000 fusions per hour. Whether this is reasonable or not would require an event generator.

The statement was made that Jones was unable to get his work published in the Physical Review because of the bias against cold fusion. After seeing the presentation and reading the current papers I would say that the confused nature of the presentation is the reason. If they worked with someone like Eric Norman, now at Livermore, who has done many low counting rate experiments on producing a manuscript, it would get published in the Physical Review.

So have we seen nuclear events in the Jones work? Got me! The rates he claims to see are low and at best twice the hydrogen loaded foils (Figure 20 of the new figures sent to us). The way to wrap this up is to allow Jones to prepare several foils and put them in front of several neutron detectors and to count. This would not cost a lot of money as for example the Triangle Labs have tremendous expertise in neutron detectors and Art Champagne knows about small cross section experiments. They also have a great deal of expertise in cosmic ray shielding of detectors because their capture cross section program at low energy has extremely low counting rates. The cost would be for travel money for Jones to go to TUNL and the shipping of foils after he prepares them at BYU. Since I don't know the time between when he prepares foils and when he sees effects, I am assuming that Fed Ex can get them from Utah to TUNL in two days, which should not impair the experiment. Other than something like this, Jones will continue to work along and there will never be an outside confirmation of his results.

The work of Lipson et al is impossible to assess. The experimental runs had no pattern. They used many different foils, solutions and a glow discharge to load deuterium into other sorts of foils. There seemed to be no worry given to the different natural radioactivity background that should be present in their differing setups. Rather they assumed that they did sufficient background runs to account for this, but it was hard to tell if the background runs were done with the same foils and setup as those of the foreground runs. The work appeared to be a random shooting of darts with no real plan of attack. They claim to see protons, and very energetic alpha particles. The rates are very low and in fact they see a factor of ten less alpha particles than protons so even if one were to say that they were indeed seeing nuclear processes they would confirm our basic ideas of nuclear physics, rather than other groups' claims that the production of alpha particles is suddenly at least a factor of 10^5 larger than traditional nuclear physics would say it should be. The data are so poorly presented with different x axes scales etc that it is almost impossible to compare background and foreground runs. They did not worry about pulse pile up in the Si detector runs which can be a significant effect in low counting rate experiments. I was surprised that in their track detectors there were no fission product events since one almost always gets some just from normal background. I would guess that the liquid in the cells prevented the products from getting to the detectors, but it is still surprising.

The review of Hagelstein et al is well written but severely short in not referring to the myriad of papers of papers that showed no effect when doing the Pons-Fleischmann (P-F) cell experiments. In fact it seems that all of the workers in this field accept the P-F results as true and yet the review work shows that no effect is observed until the loading of the foils is greater than 0.95. They also told us that it is extremely difficult to get the foil loading up to 0.95. P-F did no special work to load their foils and in fact based on the SRI work it would be hard to believe that their loading was above 0.9, too low to have any effects.

The question of excess heat it tied up with the production of nuclear products and so one first must be convinced that excess heat is produced. The irreproducibility of the excess heat is extremely hard to understand. There clearly are materials handling and preparation issues that influence the process. For example, it is not even possible to guarantee that if a procedure is followed that the hydrogen loading in a Pd rod will be greater than 0.95. So my first suggestion would be for a standard procedure to be developed so that high loading is guaranteed. The characterization of materials is greatly advanced since the original P-F experiments and maybe a program in the materials science of Pd rods and foils should be encouraged.

The one thing that several presenters agreed on is that if there is an excess of heat produced it is nuclear in origin. The main way this excess heat and nuclear effects are inferred is by comparing the D foil loaded results with that from H loaded foils. I question this, as the SRI presentation showed that in fact the change in the resistance of the Pd deuterium loaded foils is different from the hydrogen loaded ones so that one is not comparing the same system. This difference is due to slight changes in the lattice dimensions because of the larger size of the D_2 molecule as compared to H_2. Perhaps one would get excess heat from hydrogen loaded foils if more current were put through the rods or some other stimulus change were made. My point is that comparing results from hydrogen loaded rods with deuterium loaded rods does not mean that you are comparing exactly the same systems.

If excess heat originates from nuclear fusion than I agree with several of the presenters in that the rate of fusion must be 10^{11} per second. If one has a burst as is claimed then the rate would have to reach 10^{13} or so. Assuming the excess heat production of 60 hours as claimed in the Hagelstein review means that one will have about 10^{14} or 10^{15} tritons in the cell. The same would be said for He. It would be very easy to detect this level of tritium and the fact that they don't means that one has to hypothesize that this highly loaded rod into which as much deuterium has been placed as possible will absorb the produced tritium. Again, we are looking at a low level effect that we can never disprove. Perhaps the hardest effect to accept is that of having the $d+d \rightarrow \alpha$ with no other output than heat. We are asked to accept the fact that 23 MeV of kinetic energy of the alpha particle can be transferred to a lattice with no effect other than heat. There is a large body of literature from the reactor field dealing with the impact that recoiling nuclei have on fuel rods and the damage they produce. There is no reason to suspect that only in Pd or Ti will there be a different coupling mode so that instead of the kinetic energy of the α particle producing recoil into the metal lattice they "gently" transfer energy to heat through coupling to a huge number of phonons. The nuclear energy scale is so much larger than that of the phonons, about 10^7 or more, that recoil damage to nuclear reactor fuel rods leads to their embrittlement and if left in too long, crumbling.

So what advice can I give to the Department of Energy on "Cold Fusion". The question of electron screening of atoms is one of great interest in the nuclear astrophysics community. Because we use atomic targets in our earth bound experiments, whereas stellar reactions take place in plasmas, our earth bound experiments do not exactly reproduce those in stars. The exact energy of resonant states in nuclei is needed because shifts of them by just a few hundred eV can greatly change reaction rates. There have been both experimental and theoretical studies done of electron screening but the field needs much more work to have the predictive power needed for astrophysics and so would be worthy of funding, especially on the theory side.

The properties of Paladium rods in electrolytic cells and the rods uptake of hydrogen and deuterium need to be studied with modern materials characterization techniques. This work might be of future interest in the "hydrogen" economy.

What is sorely needed is a true review of the evidence for heat production that looks at all papers published to date and attempts to quantify the reason for their successes and failures. I was struck by how poorly the experimental programs were planned and how little thought seemed to be given to what one hoped to accomplish in a given experiment. This lack of over sight is because the advocates do not have to go through the normal proposal review process and program advisory committees that the rest of us seeking Federal funding and accelerator resources must. The field suffers because of this lack of review. The claim that not much money is being spent was refuted by the presenters and so those funding this research should put in proper review and oversight procedures.

Have nuclear effects been observed? There is no way to tell without the proponents showing all their results, those that worked and those that didn't. All works presented so far and that were in the Conference Proceedings showed very low counting rates, that were often multiplied by factors to make the excess look larger than actually measured. Anyone who has done low level counting knows that background rates change, there can be tritium in the D_2 gas, and natural radioactivity in the detectors as well as the solutions used in the runs. The background counting rates in the detectors themselves are quite variable even though the source of the Silicon will often be the same. This is why one goes to such extremes in neutrino and double beta decay experiments to eliminate all sources of background.

The only experiment that is easy to check is the Ti foil one of Jones, where he had no stimulus but just loaded Ti foils with either H_2 or D_2. As I outline above in my review of the Jones work, it would cost at most $30,000 to have a collaboration formed between an established neutron scattering group such as the one at TUNL or Livermore to take two of Jones' foils one that will give nuclear events and one that will not and see if one gets excess neutrons from the one expected to yield them. These runs should be done as double blind experiments. Cosmic ray shielding would be very important here, but certainly both groups are experts in this. To prove or disprove the other works will be almost impossible because the reproducibility of excess heat is so low. Also, it is almost impossible to follow what stimulus was used, what type of foils etc. Without the review discussed in the previous paragraph, one will be just shooting in the dark.

Review #16

Overview

I have evaluated most of the experimental evidence provided to us in advance of the panel meeting, given during the presentations to the panel, and contained in supplementary publications that have relevance to this subject. My opinion is that none of the experimental evidence directly presented to us is *conclusive* that nuclear reactions are occurring in these environments, but some of the evidence is certainly suggestive that they are. The most compelling evidence I have seen for anomalous effects in screened nuclear reactions comes from the low-energy *d+d* beam-target experiments [1,2] done in Germany, which were not explicitly part of our review. The experiments reported by Jones appear to have the best chance of being put on that same footing by using improved particle-detection techniques. The experiments reported by Lipson and by McKubre/ Violante contain intriguing results, but they are very hard to interpret according to our current understanding of nuclear physics as evidence that nuclear reactions, rather than some experimental artifact, are taking place. More detailed thoughts about these different types of experiments are given in the section on Recommendations.

The theory presented by Hagelstein to explain the production of trapped alpha-particles and heat by *d+d* reactions in a lattice is reminiscent of an earlier suggestion by Schwinger that *p+d* reactions might dissipate energy through phonon excitation, rather than radiate photons. The resonating group model approach he proposes is indeed a powerful tool for investigating few-body reactions, but the interactions and wave functions used in the calculations he has done so far are only schematic. Including the coupling to millions of phonons in high angular-momentum states within a four-nucleon scattering calculation using realistic interactions and wave functions is far beyond the capabilities of today's computers. Devising a theory to account for Kasagi's observation of *d-d-d* fusion seems premature, given that even *d-d* fusion is not well established at these energies. The well-known triple-a□□□□ reaction that bridges the ^{8}Be stability gap in astrophysics is possible because the □-□ resonance is so close to being a bound state that it can last long enough to capture another □-particle into an excited state of ^{12}C. There are no such narrow *d-d* resonances or excited states of ^{6}Li known, so the existence of such a process would again require an unconventional explanation.

The following comments deal with my main area of expertise, low-energy nuclear reactions that could be screened by the environment in which they take place. The reaction-rate formulas and calculations given below are as yet unpublished, and must be considered as proprietary by anyone using this review. They are included to show that the high levels of screening observed for some deuterated metals in the low-energy *d+d* beam-target experiments [1,2] are able to produce observable rates of emitted nucleons, even at room temperature.

Rates for Screened D(*d,p*) Reactions

After returning from the review panel meeting, I calculated screened reaction rates for the D(*d,p*)T reaction for various temperatures *kT* and values of electron screening potential U_e. The thermal rate, expressed as protons/s/deuteron pair, is given by the relation

$$\dot{N}_p = \frac{\mu}{\pi^2\hbar^3}\int_0^\infty (E+U_e)\sigma_{d,p}(E+U_e)\exp(-E/kT)dE$$

$$= \frac{\langle \sigma v \rangle_{\text{scr}}(T)}{\lambda_T^3},$$

in which $\sigma_{d,p}$ is the reaction cross section and $\lambda_T = \sqrt{2\pi\hbar^2/\mu kT}$ is the thermal wavelength. The initial deuterons are assumed to be asymptotically free, with energies distributed according the Boltzmann factor. The cross section is determined by fitting experimental data at energies well above the region of interest, and using extrapolations provided by R-matrix theory to obtain values in the energy domain of integration. For $U_e \neq 0$, these rates start out increasing linearly with kT, and then rise exponentially. In the non-linear region, the enhancement of the screened rates relative to the unscreened one is described to a very good approximation by the Salpeter screening factor, $f_S = \exp(U_e/kT)$, but it applies only above the linear region.

Room-temperature fusion (kT ~ 25 meV) occurs in the linear regime of the screened reaction rates. In that case, one can use the approximation $E << kT << U_e$ in the integral above to obtain

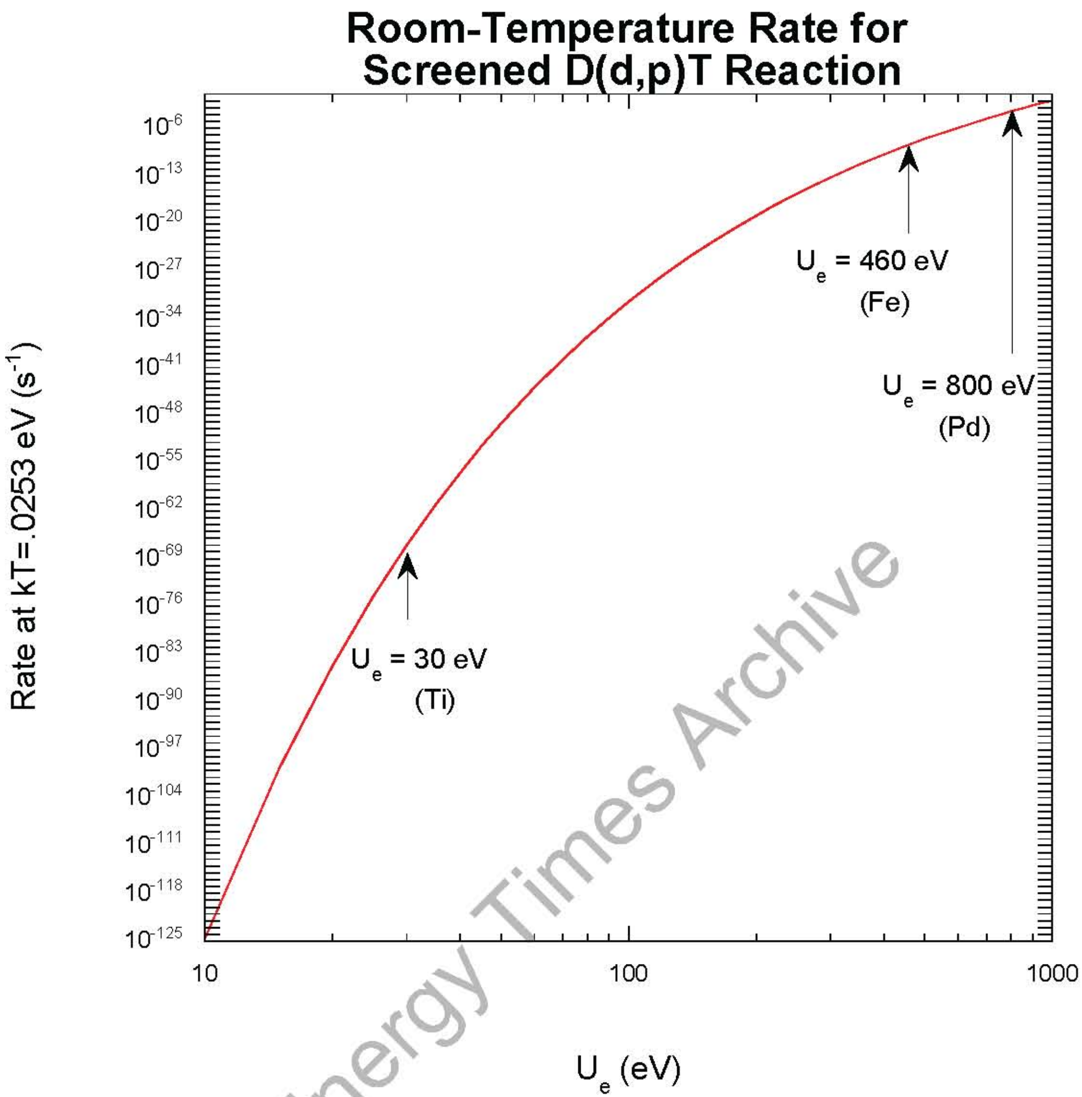
$$\dot{N}_p(U_e) \approx \left.\frac{\mu kT}{\pi^2\hbar^3}\right|_{25.3\ \mathrm{meV}} U_e \sigma_{d,p}(U_e).$$

The figure below shows the screened fusion rate for the D(*d,p*) reaction at room temperature, calculated in the linear approximation as a function of the screening potential constant U_e. Some of the experimental values of U_e as determined for metal deuterides in the experiments of Raiola *et al.* [1] are indicated by the arrows on the plot. Of those shown, probably only the rates for Fe ($U_e = 460$ eV) and for Pd ($U_e =$ 800 eV), about $4 \times 10^{-9}\,\mathrm{s}^{-1}$ and $3 \times 10^{-4}\,\mathrm{s}^{-1}$, respectively, would be observable in room-temperature fusion. The most optimistic rate (10^{-20} s^{-1}) estimated by Jones *et al.* [3] for neutrons emanating from their iron-coated titanium-pellet-cathode electrochemical cell would correspond on this figure to a value for U_e of about 185 eV. Thus, while these screened rates are not nearly large enough to account for the amount of excess heat claimed to have been produced in some of the (Pons-Fleischmann-type) electrochemical cells, they could easily explain the relatively low-level rates observed by Jones and co-workers.

Some general comments on this and other screened rate determinations are in order. The experimental values of U_e determined by Raiola et al. [1] and by Czerski et al. [2] in deuterated metals, while at least an order of magnitude larger than would be expected from theory, appear to have been extracted in a reasonable way. They fit the enhancement of the low-energy cross section with the energy-shifted functional form,

$$\sigma_{\mathrm{scr}}(E) = \frac{S(E+U_e)}{E+U_e} \exp\left(-\sqrt{E_G/(E+U_e)}\right)$$

commonly used in astrophysics, which does not involve Salpeter's screening factor. A cautionary observation is that estimates of low-temperature rates could be spuriously high if the Salpeter screening factor were used at temperatures much below the value of U_e. No specific examples come to mind, but having made that mistake myself initially, I see it as a potential pitfall in this temperature regime.



As already noted, the rates calculated in the figure above correspond to the deuterons being initially in asymptotically free (plane-wave) states of relative motion, which is appropriate for the beam-target configuration, or for a gas of initially non-interacting deuterons at some temperature. It may well not be the appropriate initial state for deuterons packed in a metal lattice, however. The true nature of this initial state is the "wild card" in theoretical attempts to understand what is going on in all but the beam-target experiments. A different choice conceivably could affect even the branching ratios of the outgoing particles. Careful measurements of the rates for all outgoing particles in "Jones-type" cells would indicate if this is the case (preliminary indications are that it is not).

A rate calculation by Hora *et al.* [4] purports to account for the rate of Jones *et al.* [3] with a screening constant of 470 eV, close to that later obtained by Raiola *et al.* [1] for iron. Their approach is entirely classical, with low-energy deuterons being stopped by the Coulomb barrier, and "fusing" at large distances (3 pm!). The rate is obtained from the classical vibrational frequency of deuterons with a given density separated by this distance. It is hard for me to understand how a meaningful rate can result from a description that ignores the essential mechanisms of the fusion process, including quantum-mechanical barrier penetration and nuclear reactions at short distances, so I think the correspondence must be coincidental.

<u>Recommendations</u>

The field of highly-screened, low-energy nuclear reactions is not well understood. In my opinion, there are enough interesting effects suggested by the experiments we have reviewed that further experimental and theoretical investigation is justified (with primary emphasis, at least in the beginning, on experiment). I will order these efforts according to what I perceive as their scientific benefit-to-risk quotient:

1. Low-energy deuteron beams on deuterated metal targets (Raiola/Czerski-type experiments). These experiments have the best chance of characterizing highly screened *d+d* reactions in a “known” environment. Careful measurements of all expected outgoing particles ($n,p,t,^3$He,□,□) would check that the branching ratios are not substantially changed by the screening, or by some other unknown mechanism at low energies. Then the only puzzle to verify experimentally and explain theoretically would be the high values of U_e that have been seen in the experiments done so far. This, in itself, is a very interesting problem. The few existing low-energy accelerators for measuring astrophysical cross sections should be able to perform these experiments, maybe even in ultra-low background environments (caves or tunnels) at very low beam energies.
2. *In-situ d+d* reactions in deuterated metals (Jones-type experiments). The same types of particle detection as listed above for measuring particle emission from metal deuterides in foils or electrodes would establish branching ratios for these low-level *d+d* (presumably) fusion events. Previous work by Jones and collaborators has established rates and branching ratios that are not out of the realm of the highly screened beam-target experiments. However, the particle-detection techniques of these experiments need to be improved considerably in order to make them convincing (*cf.* the comments in write-in review #7). This could be done by having Jones take his most promising apparatus to a laboratory that specializes in nuclear particle detection. These experiments carry somewhat higher risk scientifically than those in the first category because the initial state of the two deuterons is unknown, and probably not easily inferred due to the complications of understanding their behavior in a metal lattice. However, the benefits in terms of probing the nature of these reactions in highly screened environments is comparable to those of the beam-target experiments, with a much simpler experimental apparatus.
3. Particle emission in electrochemically loaded or D_2-glow discharge cells (Lipson-type measurements). These experiments see only weak indications of the charged particles (specifically protons) that one would expect to see from the *d+d* reaction. However, the proton rate seen in a Au/Pd/PdO:D_x electrolytic cell was given as $(4 \pm 1) \times 10^{-3}\ \mathrm{s}^{-1}$, with a spectrum that peaked at about 2.5 MeV, suggesting that *d+d* fusion had occurred in the cell. The presence of peaks in the spectra corresponding to higher-energy particles (especially □’s) is much harder to understand. These were also seen in glow-discharge tubes, especially with Ti and Pd cathodes. Glow-discharge tubes were cited specifically by McKubre as the most promising “new direction” to take the electrochemical loading of deuterium into metals. Perhaps the characterization of the more conventional products of *d+d* fusion could be done in these experiments, using improved particle detection.
4. Electochemical “excess heat” cells (McKubre/Violante-type experiments). The claims of these experiments to detect almost no particle emission, and yet generate large amounts of excess heat, correlated in some cases with the production of alpha particles, are the hardest to understand in terms of known nuclear physics. In some ways, that makes them the most interesting experiments to pursue, but on the other hand, it also makes them the least likely to be accepted by the wider physics community. Any experiment of this type must therefore be done with exceptional care and attention to detail, and documented meticulously. McKubre’s group has made considerable progress along those lines, but there is much more to do in order to establish the excess heat production, and especially the correlation to alpha-particle production, convincingly enough to be published in the mainstream physics or chemistry literature. To do this will require a careful inventory of all energy in and energy out over the lifetime of the experiment, as well as a sensitive measurement of alpha-particle production in the cell that is uncontaminated by external or initial sources of ^{4}He. One way to accomplish this, of course, is to scale up those effects so that they are well above any possible background.

I do not believe it is necessary for the DoE to establish a separate program to fund experiments that probe highly screened low-energy nuclear reactions. Experimental proposals should be evaluated individually on their own merits for the likelihood of establishing these unexpected physical effects convincingly. Independent reproducibility of other laboratory results would be a valid, and important, criterion for consideration. The unambiguous establishment of these effects experimentally is the first priority. Theoretical understanding will follow, since theorists love a challenge to their fondly-held precepts, as long as it is based firmly on incontestable experimental evidence.

References

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Review # 17

The following is an evaluation of the presentations made by members of the LENR (“cold fusion”) community during the review in Rockville, MD on August 23-24, 2004, including an assessment of the material provided in conjunction with the meeting. I will frame this report as a response to the specific charges formulated by the agencies.

Charge 1: *Evaluate the experimental evidence presented for the occurrences of nuclear reactions in condensed matter at low energies (less than a few electron volts).*

The quality of the presented evidence is very uneven. Most "nuclear" measurements (particle emission) are not convincing in comparison with the state of the art in low energy nuclear physics. The technology of measuring of nuclear reactions at very low energies has been developed extensively during the past two decades, mainly because of the increased interest in the cross sections of nuclear processes occurring in stars and supernovae. The state of the art includes high intensity charged particle and neutron beams, highly efficient detector systems with large acceptance, and sophisticated techniques for suppression of backgrounds including, e.g. veto counters.

Although I am not an experimentalist, it is my impression that the experimental groups who presented results, are not always applying or reporting the nuclear measurement techniques employed by them in a manner that is commensurate with the professional state of the art. For example, detector resolutions are not regularly provided, detector acceptances and responses to signal and background effects have not been analyzed in sufficient detail, background effects have not been exhaustively explored and ruled out, and full detector system efficiencies have not been modeled using state-of-the-art Monte-Carlo techniques. Generally, the detector configurations have limited capabilities of suppressing backgrounds, which often are of similar or larger intensity as the measured signal.

The calorimetry has made significant advances, and now looks much more convincing than that of the original University of Utah experiments. The concerns described in the ERAB report have been largely addressed. However, unambiguous evidence for the production of excess energy during a calorimetric run was not presented at the meeting. The presenters showed evidence for long periods of surplus power output by (some of) the electrolytic cells, but it remained unclear whether such a surplus survives when the fully time integrated energy balance is considered. In one case presented at the meeting, the net energy fed into the cell during the early part of the run and the net energy put out by the cell during the following period appeared to roughly cancel each other. Although the researchers argued that the energy initially fed into the cell would be recovered when the electrolytic run is terminated, data demonstrating for this property were not available for this specific run.

The ^{4}He yield measurements are not convincing, because no painstaking analysis of possible contaminations and systematic error effects is/was presented and the measured abundances are not much in excess of natural ^{4}He abundance in air. I find it surprising that the researchers did not perform, or show results from, control experiments, in which helium “production” in the cell was measured in a room environment with elevated ^{4}He concentration.

Finally, I make several comments on the presented theoretical speculations. First and formost, it must be emphasized that this field is not theory driven. Conventional nuclear and atomic theory predicts that no d+d fusion reactions can occur at room temperature at a measurable rate, even in the presence of a metal catalyst. If the experimental results of significant energy release in electrolytic cells were correct and the energy release were due to nuclear fusion, the theory would need to explain not one, but two "miracles": (1) why the fusion rate is enhanced by tens of orders of magnitude, and (2) why the branching ratios for the final states are modified by orders of magnitude. In vacuum, the ^{3}H+p and ^{3}He+n channels are 10 million times more likely than the ^{4}He+γ channel. No evidence is found for the dominance of either of these final states (the radiation hazard from neutrons and gamma rays would be quite severe at the level of the fusion reactions required to explain the observed intermittent power output).

The presented theoretical arguments for a qualitative change of nuclear the reaction mechanisms by solid state environment are not credible. The situation is *not* comparable to that of the Mössbauer effect, because:

- The lattice vibrations would need to couple to the *internal* nuclear degrees of freedom, not to the motion of the whole nucleus as in the Mössbauer effect. For the required energy transfer (> 20 MeV) this coupling would be dominated by the giant dipole resonance of the Pd nucleus, which would cause a large reduction in the coupling to the lattice vibrations.
- The energy transfer for the d+d$\rightarrow^4$He reaction of more tan 20 MeV lies far outside the energy scale of the phonon spectrum. As Hagelstein emphasizes, this would imply that the nuclear system has to transfer its energy to a very highly occupied phonon mode (billions of phonons coherently excited). Although such a mechanism might conceivably work energetically, it would be associated with a matrix element of astronomical weakness. For an interaction of multipolarity L, the matrix element would be suppressed by a factor $(kR)^{2L}$ for each phonon, where k is the phonon wave number and R denotes the nuclear radius. Since $k \approx 1/a$, where a is the lattice constant, any interaction of this type would be unimaginably weak even if the interaction were of dipole character, and the coupling would be even weaker for higher multipoles.

I am convinced that simple order-of-magnitude estimates of this kind could quickly rule out any of the exotic mechanisms proposed by Hagelstein. The problem is not with the formalism he proposes to apply – R-matrix theory, for example, is a standard and proven staple of nuclear reaction theory – but I cannot see, and he does not demonstrate, how any of the proposals could result in required large reaction rates. And no matter how the energy is transferred from the excited compound nucleus to the lattice, the two deuterium nuclei have to fuse first requiring them to overcome the repulsive Coulomb barrier, which impedes fusion at room temperature beyond observability.

Charge 2: *Determine whether the evidence is sufficiently conclusive to demonstrate that such nuclear reactions occur.*

The answer, in brief, to this charge is NO.

The most intriguing results discussed at the meeting were not those reported by members of the LENR community, but the recently published measurements of d+d fusion cross sections in beam-foil experiments by two European groups (Ruprecht/Berlin and Rolfs/Bochum), who reported an enhanced fusion rate, compared with theoretical predictions, when deuteron-implanted metal foils were bombarded with low energy (few keV) deuterons. Both groups have extensive experience with the measurement of low energy fusion cross sections. If the published enhancements are interpreted as effects of additional screening of the Coulomb repulsion between two deuterium nuclei and extrapolated to room temperature energies, they could be compatible with the d+d fusion rates reported by Jones et al., which are tens of orders of magnitude larger than expected. At this time, however, the mechanism responsible for the enhanced fusion rates in beam induced experiments is not clearly identified. For example, the enhanced rates could result from unexpected modifications of the energy loss of deuterons in ion-implanted foils, which could *simulate* an enhanced cross section.

None of the other measurements are even remotely compelling at the level of what one would require for discovery of a novel nuclear effect, such as neutrinoless double-beta decay. This comment applies to both, the particle emission measurements and the ^{4}He production claims. The discovery of a major novel effect, especially if it flies in the eye of expectation, experience, and established theory, requires a careful analysis of all imaginable sources of background and other systematic errors, as well as carefully designed and executed control experiments.

Let me comment on the often stated argument that the curious effects are only observed in the presence of deuterium (^{2}H), but not for normal hydrogen (^{1}H). The motility of light hydrogen atoms or ions in metals is about twice as large as that of deuterium atoms or ions. This does not have a major impact on equilibrium properties, but it can strongly affect transport phenomena and other nonequilibrium physics. Since the observed episodes of energy "production" always occur when changes are made to the cells, e.g. when the electrolytic current is ramped up, the threshold for the occurrence of similar effects with normal hydrogen could be significantly different, presumably higher. For this reason, I cannot regard the light hydrogen control experiments as compelling evidence.

Charge 3: *Determine whether there is a scientific case for continued efforts in these studies and, if so, to identify the most promising areas to be pursued.*

My response to this question is a weak YES. It is weak, because the proponents of the "cold fusion" effects do not seem to be interested in making their observations go away or in finding conventional explanations for them. This is never a good basis for critical experimental investigations. Having made this broad statement, there are some issues, which could be studied immediately:

- The LENR investigators should present evidence for or against excess heat being produced when the energy flow is integrated over the entire history of a run. If net energy is really produced, one deals either with nuclear reactions or a violation of the first law of thermodynamics. Since both explanations would be revolutionary, very careful checks of possible errors in the total energy balance are necessary. It is superfluous to discuss these in detail, until evidence for net energy output is presented.

- Experiments could be conducted to establish the production of ^4He (in the absence of other nuclear fragments) in a closed, isolated deuterium/metal system beyond a reasonable doubt. The main issues here are to rule out contaminations already present before the electrolytic run and airborne helium contamination of the cell during or after the run. If helium production can be established, this would prove that (a) nuclear reactions occur and (b) the solid state environment can totally change the nuclear branching ratios. This observation alone would probably be worth a Nobel prize, suggesting that extreme experimental care is necessary and the experiment would need to be repeated in an environment, which is equipped to, both, detect traces of helium and avoid accidental contamination of the electrodes.

- For the Jones et al. (BYU) claims of low level fusion rates with normal branching ratios, the most promising avenue of exploration may be a continuation of the low energy deuterium beam experiments with deuterated metal foils. The number of experimental groups and laboratories equipped to do such measurements is small, and they would need to be convinced that the enormous effort of doing a careful measurement is worth their time. If the published experiments are correct, it would make sense to explore possible sources of the observed increase in the d+d fusion rates, because any mechanism responsible for such an increase would likely have important implications for other astrophysical fusion processes.

- I see no need to devote more time to do calculations of exotic nuclear reaction mechanisms until the experimental evidence for such reactions has firmed up.

Panel Review #18

Introduction: This review responds to the charge given in the letter from the Offices of Nuclear Physics and Basic Energy Sciences at the DOE, dated July 26, 2004. The charge contains three basic questions:

1. Evaluate the experimental evidence presented for the occurrence of nuclear reactions in condensed matter at very low kinetic energies (< a few eV) (LENR).
2. Determine whether the evidence is sufficiently conclusive to demonstrate that such reactions (in fact) occur.
3. Determine whether there is a scientific case for continued efforts in these studies and if so, identify the most promising areas to be pursued.

I am basing my answers to these questions on an evaluation of recent (past 8 years) papers and conference reports that were submitted by proponents of LENR, the material presented by proponents at the August 23-24 review meeting and additional material sent to the reviewers after the review. The proponents, who are largely the same group of people that first raised the existence of "cold fusion" in

1989, presented what they considered all positive experimental results that bear on LENR. No experiments with consistently negative result were presented. Summarizing the result of my evaluation given below, I answer these questions as follows:

1. Although experiments have become more sophisticated there is no new convincing or even tantalizing evidence for LENR. The discussions and conclusions presented in the Report on Cold Fusion Research prepared in 1989 by the Energy Research Advisory Board to the DOE still apply.
2. The experimental limits on various reaction channels of the d+d fusion reaction that were done around 1989 are still valid. New experiments performed since then were not done with the care that is needed to produce convincing evidence. They are often in conflict with other, so-called positive, experimental results.
3. The research in cold fusion is still done by a small group of relatively isolated electrochemists and physicists. In their work, they strangely neglect very basic and model independent data obtained over decades of nuclear physics research. Most nuclear physicists consider the issue of cold fusion closed with the set of measurements done around 1990. Although surprises can happen (such as the Mossbauer Effect) they are usually quickly understood in terms of phenomena that make sense once discovered. This is not the case in "cold fusion". The alterations in the decay ratios of the d+d fusion reaction that would be required to explain the electrochemical data in terms of LENR cannot be understood in any sensible model. Unfortunately the experimental work is often not written up with the care and detail that would pass peer review of a scientific journal. The data from the various experiments are not correlated with each other nor translated into reaction rates that can be compared. It would be difficult to motivate a group of first-class scientists to redo the few crucial experiments since it will be a thankless job. However, in order to move this area of study, I recommend that small proper proposals that arise spontaneously form the field and withstand per review, could be entertained as part of the low energy nuclear physics program

General remarks: In 1989 two basic sets of data were presented as evidence for LENR**:** An electrochemical measurement of the energy balance observed with palladium electrodes heavily doped with deuterium. These measurements indicated a large excess energy which supposedly exceeded any value that could be generated by chemical reactions. Thus the invocation of LENR, specifically the d-d reaction, in materials. However, the proposed rate of fusion reactions would have to be so large that lethal numbers of neutrons would have to be emitted from the $D+D \rightarrow He^3+n$ reaction.
The second set of experiments claimed to observe the neutrons stemming from this reaction, but at a rate that would indicate a much smaller energy release.

Thus from the very beginning the discussion of cold fusion raised really three scientific issues, which are not necessarily related.

A. Do electrochemical or similar reactions in metals that are heavily doped with deuterium show the production of such a large excess of energy that would suggest that LENR occur at a high rate in such material at eV energies?
B. Is there evidence that the Coulomb barrier between d-d is noticeably lowered in deuterium doped metals leading to low level LENR?
C. Are the rates and ratios of fusion products from the D+D reaction altered in the solid-state medium?

Question B may have a positive answer even when question A has a negative answer, but the opposite is not true. Question C requires a positive answer if Question A is answered positively, owing to the fact that the electrochemical experiments do not (fortunately) show a neutron rate compatible($\sim 10^{12}$ neutrons/sec) with the known nuclear physics for the decay of He^4 formed in the d+d reaction even at lowest energies. The branching rations ($\sim$50% He^3+n, 50% T+p, 10^{-7} $He^4+\gamma$) result from simple facts, namely, that the first two reactions involve the strong interaction while the third is electromagnetic.
In the "cold fusion" community people working on experimental verification of question A do not seem to interact quantitatively with the groups trying to answer question B. Thus the electrochemists are bolstering their results by claiming to detect He^4 with a branching ratio of 1 (i.e. no neutrons), while the other groups report observing the normally expected number of neutrons and protons, but at a much lower reaction rate.

Answer to Question A: The presentations convinced me that electrochemical flow calorimeters have been much perfected since 1989 by the work done at SRI and in Italy (Rome). They have learned that to see an excess power effect requires very large deuterium loading (>90%) to a d/Pd atomic ratio ~1, require an activating current through the foils or rods to produce excess energy and become "active" only after a substantial period of time that is not understood. The excess power observed amounts to between 3% and 30% of activating power, with the average about 6%. This is much less than the original Fleischman observation who reported 4 Watts out for every 1 W input. Although much systematic work has been done on the materials properties that produce a successful cell, the reproducibility is still, at best, only 50%. In the SRI work the successful cells are reported to show He^4 that would be consistent with a fusion reaction of the required reaction rate if He^4 is the only reaction product. Unfortunately, not every successful cell shows this helium. The Italian group has spent a considerable effort investigating the material properties that would allow heavy d loading and would lead to an excess power effect. It did produce cells that appear to show excess energy bursts with "at least ten times the integrated energy greater than the sum of all possible chemical reactions in the cell". However cells that appear to show excess power did not appear to produce excess energy when the energy balance was integrated over the whole experiment. An interesting paper was added after the review which did seem to show large excess when integrated over a long time before and after the activating current was turned on.

I was not convinced that the calorimetric measurements are quantitative enough to believe the total energy balance. Experts on the panel will address this issue in more detail.

The observation in electrochemical reactions of He^4 as reaction product of a LENR fusion reaction with a branching ratio of 1 remains very doubtful. The production of measurable amounts of He^4 as product of the d+d fusion reaction requires three new effects: first that the fusion barrier is hugely altered by a solid state effect in the metal, secondly, that the decay branching ratios of the d+d reaction is altered by ~7 orders of magnitude in the metal, and thirdly that the γ ray energy (224 MeV!) of the $He^4 + \gamma$ channel be taken up by the lattice since these γ-rays have not been observed. The evidence for the first will be discussed below. As to the second miracle, all experiments, including the most recent ones done in Germany down to keV energies indicate that the branching ratios are the same as measured at higher energies, since they observe the protons and neutrons from the major decay channels. Even the Russian experiment presented at the review and the measurements of the Jones group report seeing the appropriate protons and neutrons from the dominant decay channels. The Italian group presented data on the interaction of laser beams with thin hydrogen/deuterium loaded Ni films inserted into an electorate bath with activation energy provided by a laser. A Cu contamination was added into the films with the intent of observing a change in the Cu65 to Cu 63 isotope ratio (presumably due to neutron capture in the film). Even when loaded with hydrogen (instead of deuterium) these films appeared to show a strong (ten fold!) shift in the isotope ratio from Cu63 toward Cu65. This would require neutron fluxes found in the core of nuclear reactors, at variance with the claims of the electrochemical measurements that see no neutrons. The neutrons would also leave behind a large amount of He^3, which is not observed.
Thus the possibility that the observed He^4 is an instrumental effect, perhaps due to diffusion of environmental helium into the system, must be decisively ruled out before this "signal" of a LENR can be accepted.

Answers to Question B: A number of data on different reactions were shown that reported observation of low rates of neutrons, protons and alpha particles at very low energies. If true these would indicate the occurrence of LENR and a lowering of the Coulomb barrier for deuterium loaded in metals. I discuss these experiments in some detail:

<u>1. Detection of neutrons in metal deuterides by Young et al.</u> : This experiment written up in an undated, but recent manuscript reports observation of excess "high-energy" presumably 2.45-MeV neutrons from partially deuterided titanium fouls activated by Coulombic heating (I guess this is with some current) . The experimental set up is quite elaborate The sample cell containing the deuterided Ti foils is surrounded first by an unsegmented plastic scientillator and then with two circles of He^3 neutron counters in embedded in a moderator. "Fast" neutrons are identified by coincidences between the proton recoils on the plastic scintillator and the neutron capture in the He^3 counters. Three plastic cosmic-ray shields approximately surround the detector (in an unclear geometry). The entire apparatus is a cave with a 100 m rock overburden (which is not very much) to reduce cosmic rays. This manuscript does not contain enough details, and no dimensions to verify efficiencies. The various pulse categories relating to cosmic rays, neutrons etc. are said to be differentiated from their pulse forms, but no details are given. Excess neutrons (defined by plastic-He^3 coincidences and visual pulse shape discrimination are claimed at a rate of ~6 cts/hr (3 times background) are observed when the deuterided foils are activated by heating. The background is determined by filling the cell with hydrogen. It is not stated whether a background run was

made with deuterium loading but no activation. A rough test was made that the neutrons indeed came from the direction of the cell. The repeatability was claimed to be ~40%. The rate, after correcting fro efficiencies is claimed to be ~ ~50 +- 15 cts/hr. Suspiciously, the He^3 counters, when run without coincidence, had a ~10 times higher counting rate, almost all “background”. Where did these background neutrons come from? Perhaps from the rock surrounding the detector? No indication is given about the ratio of true to chance coincidences and their time distribution. What time and pulse shape cuts were made? Assuming that the deuterided measurements really did show an excess, one possible scenario would be the scattering of cosmic rays form deuterium in the cell which can produce recoil neutrons and protons. What is, e.g., the neutron rate if the cosmic ray anticoincidence detectors are turned off? In summary, the experiment is not described in sufficient detail, has not made enough checks and has not addressed possible alternate explanations to demonstrate convincingly that LENR has occurred.

In its present form this paper would not be acceptable to a peer reviewed journal.

<u>(2) Charged-particle emission in coincidence and single by Young et al.</u>

Two experiments have been presented, one a singles experiment detecting protons, the second one coincidence measurements between protons and tritium.

The first claims the observation of ~ 3-MeV protons from the d+d ➔ T + p reaction in a “partially loaded” TiDx foil at a rate of 2171+-93 counts/hr, > 400 times the background rate. The protons emanate from a region 12 µm deep in the TiDx foil. The experiment utilized a plastic/glass sandwich scintillator array glued to a large photomultiplier mounted on one side of the foils. The different decay times of the plastic and glass pulses produced different pulse shapes.

The paper gives the light output of the plastic scintillator but not of the glass scintillator (neither material is identified) but the latter can be guessed from the so-called burst spectra and is perhaps twice as high as in the plastic. Nothing is said in the paper about the cosmic ray events and their rejection, as well as of the subtraction of radio-activities that are mentioned without further detail. The proton spectrum in the plastic scintillator shows an essentially symmetric peak when one would expect a significant low-energy tail on the proton spectrum. This is even truer for the events in the glass scintillator which are thought to be also protons, presumably related to the LENR and thus the high energy end of the spectrum. Why do these events form a symmetric peak?

The data rate is tremendous: >500 events in 15 minutes! How does this agree with the neutron rate (~6 events/hour) reported in the experiment (1)? (Proton and neutron channels have about the same widths). Thus a large number of checks can easily be done. A background run was done before the TiDx run begun showing essentially zero background, but what about a run *without* deuterium in the foils, or with hydrogen loading? The actual run produced a strong yield that doubled during 1 hr and then decreased to 1/2 over another hour. Two weeks later the same foils produced no statistically significant effect. In summary, the information given in the paper does not permit an in-depth check of the experiment and in view of the lack of reproducibility more elaborate check runs should have been made than are reported in the paper. In addition the paper does not discuss in a quantitative way the observed reaction rate per deuteron atom. In a presentation it was stated that the Coulomb barrier would have to be screened by 1.5 keV to explain the rate, an unbelievably large correction.

The group then proceeded to a p-t coincidence experiment. Two Ti foils each 25 micro think were d loaded in an oven in to a d/Ti ~1 ratio and then chemically treated with Lithium Deuteride, D2O and $D2SO_4$. These foils were placed between two large Si detectors. No activation current was used in this experiment! The Ti surfaces were treated in strange ways to “activate” them, which I disregard for this discussion. . Data were taken with both d loaded and h loaded foils. While the latter showed a small background, the d loaded foils seem to show ~9-keV tritons hitting one detector and 1.7 -MeV (degraded) protons in the other detector in time coincidence. The coincidence rate was now 9 +-1 event/day, with a 2.6 +-0.6 background rate. This is ~ 1/5000 times the rate reported for the first experiment and is claimed to be due to the thin foil. However, the first experiment also claimed only a narrow range inside the foil as a starting point of protons. Perhaps the deuteration was much less or the absence of activation current reduced the rate? The biggest discrepancy is between the two-dimensional spectra without cosmic-ray anticoincidence, and a “better spectrum” taken later with cosmic ray anticoincidence. Whereas the first showed events that could be associated with ~1.7-MeV protons and 0.9-MeV tritons (although the events really covered the phase space more or less monotonically down to much lower energy), the later 2-dimensional spectra showed very few events above 1 MeV. The events clustered in a statistical distribution at much lower energies. This is not understandable even to the authors. I should add that the paper is poorly written, hard to follow, and the figures are almost illegible. Background runs (with hydrogen instead of deuterons) are compared with the deuterided runs but the claimed excess for the latter is invisible in the figures. The best that this paper can claim is that there are possibly coincidences of charged particles. No chance coincidence spectrum is shown. Whether the events are protons, tritons or cosmic rays remain open.

3. The experiment by Lipson et al.
In a Russian experiment deuteron and hydrogen loaded metals seem to show d+d reaction protons at ~ 3 MeV and 1 MeV tritons, and low level emission of very energetic alpha particles with energies >12 MeV. Samples were D loaded Au/Pd, Al/Pd Ti,Pd, Nb, Ta foils and the particles were only observed in metals with high h/d affinity. Activation energy was provided by a either d-d glow discharge or electrolysis. These alpha energies require a separate explanation since no known nuclear reaction would produce them. Nevertheless, they are cited as evidence for LENR occurring in these hydrogen (!) and deuterium loaded foils. Two experimental techniques were used: A first set used Si detectors. They observe clearly the 8-MeV radon peak with an unloaded foil, and a more intense spectrum between 0 and 4 MeV with d loading. These energies make no sense for a d+d ➔ T + p reaction. A weak excess claimed around 2.5 MeV is not statistically significant. A later run with a better Si detector shows quite a different spectrum with the radon peak clearly visible and then a very few events between 10 and 12 MeV.
A second set then used CR-39 plastic detector foils, in which protons and alpha particles could be differentiated by their track widths. I could make no sense of the spectra observed with these track detectors. They showed a few high energy (8 to 16 MeV) events over 650 hours (!) with no statistically significant difference between foreground and background. Ti and Pd d-loaded and glow charge-activated appeared to have more counts than other metals. It is claimed that these energetic alphas are accompanied by d and p tracks from the d+d reaction. However, these high-energy alpha particles must be radioactive contaminants. No d-loaded spectrum seems to be statistically significant. This experiment is not evidence for LENR.

4. Other experiments: A recent German experiment used a low-energy (down to 6 keV) deuteron beam to study screening energies of deuterium in metals, detecting the recoiling tritons and He^3 nuclei. First, they find the ratio of the He^3+n and T+p reactions equal, as is observed at higher energies. They determine a screening potential as high as 350 keV, which is lower than that invoked by Jones et al, but still exceeds solid state expectations by factors of 5 to 10. Measuring reaction yields at these low energies is tricky because it is hard to determine how many d's the beam particle encounters in the metal. The experiment should be repeated

A Japanese experiment claims to produce, with a 350-keV deuteron beam, a 3-body reaction (d+d+d, presumably sequential) in d-loaded target foils that exceed normal expectation by a factor of 10^{12}! These experiments will be repeated at NRL. It is not clear how any screening effect could explain these rates.

Answer to Question C: There is no evidence that the branching ratios in the decay of He^4 formed in the d+d reaction are altered in a solid state environment at low energies, down to a few keV It is very hard to see how this could happen since these decay ratios follow from very basic facts about the interaction involved and decay barriers. Therefore, lack of observing any high-energy gamma rays or neutrons is strong evidence against nuclear reactions being responsible for any excess of energy that is claimed in electrochemical experiments. We heard of an almost desperate effort to provide a model in which the lattice coherently absorbs the 26 MeV that need to be accounted for, if the dominant channel would be emission of He^4 +γ (the latter not being observed). Any analogy with the Moessbauer effect, where the lattice takes up the recoil momentum of an emitted γ-ray is misleading and unrealistic by orders of magnitude. In addition the dominant particle emission channels would still have to be suppressed by some lattice effect. It does not appear to me worthwhile to pursue such theoretical models further.