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

August 10, 2016

Re: OSTI-2016-01064-F

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

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

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

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

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

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

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

Sincerely,

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

Cold Fusion Panel

October 25, 1989

To: DOE Cold Fusion Panel

From: Larry R. Faulkner

Subject: Report on Symposium in Hollywood, Florida

At last week's meeting of the Electrochemical Society in Hollywood, Florida, there was a symposium on cold fusion. During the spring, at the height of interest in this topic, the Society's Executive Committee felt that it would be appropriate to create a forum for work in this area during the fall meeting. As Vice President of the Society, I was asked to organize it. We determined early to offer the event as a "recent news" symposium, so that abstract deadlines could be pushed back to September 1. Moreover, only short abstracts were required.

A total of 26 contributions were scheduled. Barry Miller and I attended the whole symposium on behalf of the Panel. Below, you will find the abstracts, together with my commentaries on the presentations.

"Observation of Neutrons from Cold Nuclear Fusion", by J. N. Harb, W. G. Pitt,. D. N. BENNION, E. P. Palmer, J. B. Zirr, G. L. Jensen, and S. E. Jones, Chemical Engineering and Physics Departments, Brigham Young University, Provo, UT 84602

Sensitive neutron detectors were used to investigate cold fusion in two types of electrochemical cells. Low-level bursts of neutrons above background were observed in cells with concentric palladium electrodes. There was no evidence for excess heat or tritium production, although the expected heat generation rates were near the sensitivity limit of the cell. An additional cell capable of very high current densities did not show any measurable evidence of fusion at current densities up to 220/cm^2. Alternate electrode designs and electrolyte compositors have and are being tested, but with no positive results as of this writing. A non-electrochemical system has yielded tentative results of neutron bursts with D_2O prepared Portland cement.

Commentary on Bennion paper: Bennion was involved in electrolytic systems featuring a variety of cells, including one with very large electrodes and power densities. In the largest cell, there was no excess heat, no tritium, no neutrons, and no He. A smaller system yielded no excess heat, and no tritium, but a few neutrons were reported (2.4 n/1000 s vs. 1.7 n/1000 s for H_2O background. In a system with a "broom" Ti electrode, bursts of 5 n were detected at startup.

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"Attempts to Observe Cold Fusion in Pressure and Electrochemical Experiments", by M. A. BUTLER, D. S. Ginley, J. E. Schirber, and R. I. Ewing, Sandia National Laboratories, P. O. Box 5800, Albuquerque, NM 87185

The apparent discovery of cold fusion at low levels by Jones and other workers has excited tremendous attention. Neutron production near the background level is difficult to confirm. We report underground experiments with electrochemical cells and pressure bombs simultaneously utilizing three neutron detectors (background 10±1 counts summed over all three detectors). No coincident events have been observed in all three detectors (with appropriate relative intensities) in over 500 hours of run time.

Commentary on Butler paper: Butler described elaborate attempts to detect neutrons from electrolytic cells and pressurized bombs in a shielded, underground counting chamber. They worked with background counts on the order of 10 n/hr, but Butler commented that solar flares can cause an order of magnitude variation in neutron background. Multiple detectors were used in a coincidence mode. Butler showed artifacts in single detectors that might be erroneously interpreted as neutron bursts.

"Search for Cold Fusion of Deuterium in Palladium", by J. JORNE, Dept. of Chemical Engineering, University of Rochester, Rochester, NY 14627 and J. Toke, Dept of Chemistry, University of Rochester, Rochester, NY 14627

Attempts are being made to identify fusion products during the loading of palladium with deuterium. Palladium is being charged with high quantities of deuterium by two methods: (1) Electrolysis of heavy water. (2) Pressurizing palladium in deuterium gas at low temperatures. Effects of current density, temperature, surface and catalytic activities and pretreatment are being studied by positioning neutron counters around the cell to monitor an excessive high energy neutrons and Y-radiation, beyond the background level.

Commentary on Jorne paper: Jorne focused on neutron counting at electrolytic cells. Previously he had only negative results, but recently saw bursts on the order of 18 n/s upon changing the current density. There was no coincidence detection in most experiments, but there was remote background monitoring via a second detector. When this detector was brought into a coincidence detection scheme, Jorne was unable to provide monitoring of the background. Lately, Jorne has been unable to get the positive effect.

"Neutrons and Tritium from Cold Fusion in Pd-D", by R. ALQASMI, The United Arab Emirates University, Chemistry Dept., Post Box 15551, Alain, United Arab Emirates, K. Albertsen, H.-G. Cnotka and H.-J. Schaller, Institut fur Physikalische Chemie der Universitat, Kiel, FR Germany

This report concerns the observation of neutrons and tritium from cold fusion in Pd loaded electrolytically with deuterium. In order to achieve high deuterium concentrations, the electrolysis was carried out at 20, 0, and -80°C, successively using an alcohol acid bath as the electrolyte. Sporadic neutron emissions significantly above the background level were observed in three long time experiments. Measurements of the tritium levels in the electrolyte solutions point to a generation of tritium.

Commentary on Alqasmi paper: Despite the abstract, Alqasmi said in his presentation that they are not convinced that the enhanced tritium levels are real. He now ascribes the apparent enhancement to chemiluminescence. The main point of his talk to report a neutron burst observed on one occasion. Normal background was about 200 counts/hr. In this burst, the count rose to 2000/hr.

"Anomalous Heat Production on Electrolyzing Heavy Water Solutions of Lithium Deuteroxide with Palladium Cathodes", by, A. J. Appleby, Y. J. Kim, O. J. Murphy, and S. SRINIVASAN, Center for Electrochemical Systems and Hydrogen Research, Texas A&M University, College Station, TX 77843-3577

Electrolysis of heavy and light water solutions of lithium and sodium deuteroxide have been carried out for extended periods of time using palladium cathodes of various sizes and geometrical shapes. For palladium cathodes in light water solutions, and platinum cathodes in heavy water solutions, no anomalous heat fluxes were measured using a Tronac microcalorimeter. However, for palladium cathodes in lithium deuteroxide-containing heavy water solutions, of various lithium isotope compositions, anomalous heat fluxes were measured by the calorimeter. Anomalous heat production rates were quenched, or considerably reduced, on exchanging sodium deuteroxide solutions for lithium deuteroxide solutions. Liquid scintillation counting analysis performed on heavy water solutions, after measuring anomalous heat production rates, gave only background levels for tritium in solution. Similarly, analysis for helium isotopes in bulk palladium cathodes (after the evolution of anomalous heat) corresponded to background levels.

Commentary on Srinivasan paper: Srinivasan provided no data that were not already available to the Panel. His latest results came from experiments dated July 4. There was a discrepancy between his private conversations, in which he claimed a positive heat effect of 2-4%, and his public presentation, in which he claimed effects on the order of 15% of corrected input power.

"Investigation of Phenomena Related to D_2O Electrolysis at a Palladium Cathode", by R. ADZIC, D. Gervasio, I. Bae, B. Cahan, and E. Yeager, Case Center for Electrochemical Sciences, Case Western Reserve University, Cleveland, OH 44106

Measurements have been performed to check on the Fleischmann-Pons phenomenon. They involved calorimetric measurements in a glass cell of the type used by Fleischmann and Pons also a battery type Tronac calorimeter, determination of the D/Pd ratio by coulometry, Li determination in the Pd electrode and neutron radiation measurements. The total energy balance of a closed cell obtained with the Tronac calorimeter showed excess heat production over an extended period for Pd in LiOD in two cells. Much smaller excess heat was found with Pd in LiOH in one cell. The D/Pd ratio was 1:1 for 0.25 mm wire. Li was found to penetrate 200 nm into Pd bulk. Five-fold enhancement of tritium over that in D_2O was found with one cell, while the neutron measurements were inconclusive.

Commentary on Adzic paper: Adzic claimed a 3-8% excess heat effect in closed cells, but added that "adequate statistics are not yet available." This group also sees an amplification of T by a factor of 5, but they did not seem to be sure that this result was beyond the expected enrichment. They further reported that they were unable to make a "conclusive" detection of neutrons.

"Experiments in Search of Electrochemically Induced Cold Fusion" by U. LANDAU, W. M. Lynes, D. Roha, R. Saini, and S. Rochel-Landau, Case Western Reserve University, Chemical Engineering Dept., A. W. Smith Building, Cleveland, OH 44106

Calorimetric measurements, tritium balances and neutron counts in electrochemical cells with deuterium charged palladium cathodes are reported. Control experiments in similar cells but with H_2O-LiOH electrolyte are also provided. Data analysis shows that although a number of the deuterium charged experiments indicate excess power, the signal to noise ratio is too low to unambiguously prove the presence of a non-conventional energy source. A greatly improved experimental system with significantly better accuracy and sensitivity is being designed and will be described.

Commentary on Landau paper: Landau did not present data beyond that available to the Panel in early July. In private conversations with him, some of us had criticized his analysis of results on several counts. Among them was a complaint about the lack of statistical analysis of his data, even to a degree that could be applied from the primary data themselves. Landau uses a graphical analysis scheme in which the excess heat shows up as a non-zero intercept. It was evident even from his graphs that the uncertainty in the intercept was rarely, if ever, significantly different from zero and that his conclusion favoring the existence of excess heat was unsupported by his own results. The main point of the Landau's presentation in Hollywood was a recognition of this point. He admitted that only in rare cases was the intercept larger than a couple of standard deviations of its own value. However, he was unwilling to retreat completely from his earlier contention of positive heat effects. I note, however, that he calculates only a lower estimate of the intercept's standard deviation by using the point scatter.

"Search for Evidence of Cold Fusion by Radiation Detection and Calorimetry", by E. DARCY, D. Young, G. Badhwar, and A. Konradi, NASA-Johnson Space Center/EPS, Houston, TX 77058

Two experiments to verify cold fusion are presented. First, with sensitive radiation detectors, no levels above background were found in vented cells containing 0.1 M ^{6}LiOD with three Pd geometries through weeks at 500 mA/cm^2. Second, precise heat balances are being performed on closed cells with recombination with a high power (20W) heat conduction calorimeter from Hart Scientific, Inc. Cell design and charging sequence are per the proportions and advice of previous work claiming excess heat. Details or apparatus, procedures, and analysis of results will be presented.

Commentary on Darcy paper: This paper was withdrawn. I do not know whether the withdrawal reflects retraction of the abstracted conclusions.

"Experiments in Search of Nuclear Reactions and 'Excess Heat' in Metal Deuterides", by S. GOTTESFELD, R. E. Anderson, D. A. Baker, C. R. Derouin, F. H. Garzon, C. A. Goulding, M. W. Johnson, E. M. Leonard, T. E. Springer, and T. Zawodzinski, Los Alamos National Laboratory, MEE-11, MS D429, Los Alamos, NM 87545

We have investigated claims of nuclear reaction phenomena in low-temperature metal deuterides by monitoring Pd-D electrolysis cells and Ti-D high-pressure vessels with high efficiency ^{3}He and NE-213 neutron detectors in a very low background radiation environment and by calorimetry in a custom-designed differential heat flux calorimeter. Neutron emission was investigated using a multiple detector configuration with cross-correlation between individual detectors. No above-background multiple-detector-correlated events were measured in several weeks of monitoring five different Pd-D electrolysis cells or LN_2-quenched Ti-D pressure vessels. The calorimetric experiments measured the total heat flux from a Pd-D electrolysis cell containing a recombination catalyst, which allows for the collection of enthalpic data on a closed system. No energy flux above the input power was observed to date.

Commentary on Gottesfeld paper: This group was wholly negative. The abstract is reasonably descriptive. They saw apparent neutron bursts early, but later traced them to detector malfunctions.

"Calorimetric Measurements of Anomalous Power Produced by Cathodic Charging of Deuterium into Palladium" by, R. A. ORIANI, J. C. Nelson, S. K. Lee, and J. H. Broadhurst, Dept. of Chemical Engineering and Materials Science and Dept. of Physics and Astronomy, University of Minnesota, Minneapolis, MN 55455

Seebeck-effect calorimetry on the cathodic charging of deuterium into palladium has confirmed that excess power is produced whereas it is not produced with hydrogen. With appropriate experimental parameters the excess power can be higher than 50 watts per cm^3 Pd, and the integrated excess power can amount to more than 2.2 MJ/cm^3 Pd over eleven hours. Such magnitudes are very difficult to rationalize in terms of chemical reactions. At present we have no evidence of nuclear reactions.

Commentary on Oriani paper: Oriani received perhaps the greatest degree of public attention. He described a system involving an open cell in a heat flow calorimeter similar in principle to that employed by Law and others. He stated that early runs with Pt electrodes in H_2O and D_2O, and even with Pd electrodes in D_2O gave essentially perfect power balances (after correction for 100% venting of D_2 and O_2). However, he later acquired two rod electrodes from Bockris at Texas A&M, and these rods, operating in a novel experimental configuration, were observed to produce excess heat. Other experimental changes included (a) the use of a spiral Pd anode and (b) the acidification of the solution with D_2SO_4. Acidification was not characterized quantitatively. The amount of excess heat varied with time and possibly with current

density. One data point was said to represent an absolute heat excess (above even that corresponding to full recombination). The total excess was 200 kJ out of about 1.5 MJ input during the entire run. There were no gammas, no neutrons, no x-rays, and no tritium. Lack of recombination was not verified in any experiment by direct measurement. It was inferred from the controls, by the good heat balances observed there. Oriani's positive results all came from two runs, one weakly positive, and the other yielding the most thoroughly discussed data. The calorimeter was damaged in a gas explosion after the most interesting run, so no follow-up has been done.

"Electrolysis of LiOD in a Sealed Cell", by J. MCBREEN, Dept. of Applied Science, Brookhaven National Laboratory, Bldg. 801, Upton, NY 11973

The electrolysis of 1 M LiOD was investigated in a cell with a Pd foil (20 mm x 40 mm x 25) cathode and a 100 mesh Pt anode. The cell was built like a conventional alkaline electrolyser with an asbestos diaphragm and was run a 1 Amp in a cell containing 120 ml of electrolyte. Sealed operation over a four month period was achieved using a fuel cell electrode to recombine the gases. Measurements included cell voltage, electrolyte temperature and tritium content of the electrolyte. The effect of anions on the entry and egress of hydrogen in Pd was investigated separately. These results will also be presented.

Commentary on McBreen paper: McBreen used a cell design in which the cathode was separated from the anode by a diaphragm for reasons that were not entirely clear. He also employed a closed configuration and a catalytic recombiner. After 4 months there were temperature excursions of 7-8^{o} lasting up to 15 hr. Thermal effects were not quantified beyond this simple observation. McBreen claimed that after 100 days the tritium level had risen. It yielded 2.5 cps before electrolysis and 3.3 cps afterward. Uncertainties, presumed by me to be one standard deviation, were about 10%. McBreen seemed to think the rise in tritium was real, but it is not clear that it was statistically significant. Since the cell was a closed system, electrolytic fractionation of tritium is not a factor.

"Exploratory Experiments Concerning Anomalous Thermal Effects in the Palladium-Deuterium System", by W. A. ADAMS, E. E. Criddle, and V. S. Donepudi, Electrochemical Science and Technology Centre, University of Ottawa, Ontario, Canada K1N 6N5; B. E. Conway and G. Jerkiewicz, Dept. of Chemistry, University of Ottawa, 32 George Glinski, Ottawa, Ont., Canada K1N 6N5, J. Hebert, Dept. of Physics, University of Ottawa, 34 George Glinski, Ottawa, Ont., Canada K1N 6N5; C. 1. Gardner and F. Szabo, Dept. of National Defense, 101 Colonel by Drive, Ottawa, Ont., Canada K1A 0K2

Electrolysis experiments (in H_2O and D_2O solvent) have been conducted to observe the anomalous thermal and possible cold fusion effects that were recently reported by Fleischmann and Pons. Different variables studied include: i) well-characterized palladium electrodes with differing metallurgical structures, ii) electrolyte composition, iii) cell configuration, and iv) the role of recombination. Experimental observations include: i) thermal balance by calorimetry, ii) particle emission, i.e., neutron flux and other possible products, iii) electron-optical analyses of palladium surfaces prior to and following deuterium charging, and iv) analyses of gas streams during deuterium and hydrogen charging.

Commentary on Criddle paper: This group studied pairs of open cells of identical structure operated in series. One cell carried normal water and LiOH, whereas the other carried D_2O and LiOD. The main measurement was the difference in temperature between the cells. The claim is that the temperature difference rose after a while, in a manner compatible with the production of excess heat in the cell containing D_2O. The condition persisted for 14 days. A peculiar result was that the conductivity of the LiOD in D_2O became comparable to that of the LiOH in H_2O after operation in these cells for 91 days. This result indicates that the electrolyte composition was changing differentially in the two cells, so different thermal histories would follow. Under questioning, Criddle surmised that the change in composition came from carbonation of the electrolytes. This group also looked for tritium but found no significant increase upon electrolysis.

"Effect of Microstructure on the Incorporation of Deuterium into Palladium" by J. W. FLEMING, H. H. Law, P. K. Gallagher, J. Sapjeta, D. Loiacano, and W. F. Marohn, AT&T Bell Laboratories, Rm 6E-226, Murray Hill, NJ 07974

The effect of microstructure of palladium wires on the incorporation of deuterium was studied by comparing the behavior of annealed and cold-worked samples. Deuterium loading induced greater volume change of the cold-worked samples over the annealed ones. The rate of deuterium loading is also faster in the cold-worked wires.

Commentary on Fleming paper: Interferometric dilatometry was used to measure changes in length of Pd electrode upon loading by deuterium. The rates of elongation were sensitive to the metallurgical preparation of the sample. There was an attempt to correlate length changes with the degree of D loading in the metal, but there were questions about the degree to which loadings were accurately measured, because there was a significant elapsed time between the withdrawal of the electrode from the cell and the measurement of weight loss by TGA. The highest loadings reported here were about 0.72.

"Diffusion of Deuterium in Palladium", by H. H. LAW, P. K. Gallagher, J. Sapjeta, and R. Stoffers, AT&T Bell Laboratories, Rm 7D-218, Murray Hill, NJ 07974

The diffusion of deuterium in palladium foil has been investigated using an electrolytic cell coupled with a mass spectrometer. Diffusion flux is found to be significantly higher when the deuterium content is above 0.4, presumably when the Pd deuteride is essentially all in the beta-phase. A model for the diffusion mechanism is proposed.

Commentary on Law paper: Law's presentation indicated significantly higher diffusivities for D and H in moderately to highly loaded Pd, relative to the corresponding numbers in lightly loaded Pd. The values given by Law were questioned pretty heavily by the audience, because Law evidently gave no

consideration to possible rate control at steady state through a Pd membrane by interfacial kinetics.

"*In Situ* Measurements of the H(D) Thermodynamic Activity and Stoichiometry in Pd during Electrolysis", by P. ROSS and H. Sokol, Materials and Chemical Sciences Division, Lawrence Berkeley Laboratory, Berkeley, CA 94720

A variation of the classic experiments on Pd charging by Frumkin were performed in alkaline electrolytes. Membrane potentials were measured using Pd diffusion tubes having a strain gauge and reference electrode sealed in the "backside" (zero current) of the tube. Frontside (current carrying) and backside potentials and inner hydrogen (deuterium) pressures were measured in 1M KOH(D) and LiOH(D). In KOH, the backside potentials were identical to those reported by Frumkin in 1M H_2SO_4, e.g. - 42 mV or p_{22} = 21.5 atm, corresponding to a Pd/H stoichiometry of 0.82 $\pm$ 0.02. This activity and stoichiometry was independent of current density, even for current densities as high as 1 A/cm^2. In KOD, the maximum stoichiometry was slightly lower, 0.78 $\pm$ 0.02. In LiOH(D), very different behavior was observed for the frontside potentials, which became very cathodic (- 1000 mV) at high current density without any corresponding change in the backside potential. Post-mortem analysis of the Pd cathode surface by RBS revealed penetration of Li into the surface region (1-10 m) during electroysis at high current density. It is suggested that Fleischmann et al.[2] misinterpreted very negative Pd cathode potentials as indicating very high H(D) activity, which our measurements indicate is not the case.

Commentary on Ross paper: I thought this paper presented some interesting new results. Ross sought to explain the exceptionally large membrane potentials reported by Fleischmann and Pons in their original paper. The figures given there gave rise to the early speculation about effective pressures of 10^{27} atm or so. Ross showed pretty carefully that the large "membrane potential" was not actually a membrane potential at all, but instead arose from the incorporation of Li into the Pd on the current carrying face of the electrode. In fact, the electrochemical characteristics of that face required several weeks to begin to show alteration and then showed a time dependence thereafter. This effect could explain some of the reported rises in cell temperature after long periods. It would be interesting to know if the time profiles of the effect for H_2O and D_2O systems differ. I asked Ross about this, but he didn't know the answer offhand.

"Neutron-Induced Chain-Reaction Processes for Electrolysis with Metal Deuterides", by Y. E. KIM, Dept. of Physics, Purdue University, West Lafayette, IN 47907

Tritium production and excess heat generation observed by Fleischman, Pons, and Hawkins (FPH) and others in electrolysis experiments cannot be explained by known single-step nuclear reactions such as cold deuterium fusions, since the reaction cross-sections and rates are too small at room temperature. However, a combination of known nuclear reactions can form a set of closed chain reactions which can become self-sustaining at a critical stage under favorable conditions and geometries, as in the well-know case of neutron-induced fission chain reactions. The FPH effect is described in terms of chain-reaction processes involving (1) neutron-induced fission-fusion chain reactions and (2) neutron-induced photonuclear chain reactions in palladium deuteride. Experimental evidence and tests of each hypothesis for the FPH effect are described.

Commentary on Kim paper: This paper was withdrawn, because the author could not raise the funds to attend.

"A Loose Deuteron Cluster Model for Cold Nuclear Fusion", by R. T. BUSH, and R. D. Eagleton, Physics Dept., California State Polytechnic University, 3801 West Temple Ave., Pomona, CA 91768

A loose deuteron cluster model (stoichiometries around 1) is presented to attempt to account for cold nuclear fusion. The model accounts for "anomalies" observed in the production of excess heat, neutrons, and tritium.

Commentary on Bush paper: This paper was withdrawn because the authors felt that their results were unsound.

"Comparison of Thermal Measurements on Two Fast Mixed-Conductor Systems: Deuterium and Hydrogen in Palladium", by S. Crouch-Baker, T. M. Gur, G. Lucier, M Schreiber, and R. A. HUGGINS, Dept. of Materials Science & Engineering, Stanford University, Stanford, CA 94305

Fleischmann and Pons reported that they had observed excess heat generation, neutron and gamma ray emission, and the presence of tritium in electrochemical experiments in which deuterium had been inserted into palladium electrochemically (1). They claimed that this could not be explained by any known chemical or electrochemical effect, and proposed the existence of an hitherto unrecognized form of atomic fusion related to the presence of deuterium inside palladium.
A number of experiments have been undertaken using isoperibolic calorimetry to study the electrolysis of the deuterium palladium and hydrogen-palladium systems, employing experimental conditions as comparable as possible in order to reduce the possibility of thermal influences due to chemical or metallurgical effects. The results of these measurements, as well as some pertinent experimental considerations involved in the measurement of thermal effects in systems of this type will be presented.

Commentary on Huggins paper (presented by Gur): This paper was essentially the same as the presentation given to the Panel when some of our members visited Stanford last June. The only new element was a description of a new calorimetric system featuring a closed cell. However, no results with this system were provided, except for calibration runs.

"Calorimetric Studies of Electrochemical Incorporation of Hydrogen Isotopes into Palladium". by J. W. Fleming, H. H. LAW, J. Sapjeta, P. K. Gallagher, and W. Marohn, AT&T Bell Laboratories, Murray Hill, NJ 07974

The formation of palladium hydride and deuteride by electrolysis has been studied with high precision calorimetry using sealed cells with *in situ* recombination of gaseous products as well as open cells. Palladium electrodes prepared by different methods were studied. No unexpected excess heat was observed. The error in the energy balance is estimated to be less than 2%.

Commentary on Law paper: No excess heats were observed in a wide range of closed cells featuring Pd from different sources and treated according to different protocols.

"Calorimetric and Kinetic Observation of D_2-Pressurized LiOD/D_2O/Pd Cells", by M. C. H. MCKUBRE, S. I. Smedley, F. L. Tanzella, and R. D. Weaver, SRI International, 333 Ravenswood Ave., Menlo Park, CA 94025

Thermal and electrochemical kinetic processes were examined in a cell compromising a Pd cylindrical cathode, concentric Pt coated Ni anode, containing 0.1N LiOD in D_2O. Experiments were performed using an electrochemical overvoltage to load deuterium into the Pd lattice, at a cell voltage below that of electrolysis of D_2O, in an overpressure of D_2 gas.
Results will be described of anomalies in both the thermal behavior evidenced by the calorimetric response, and the kinetic behavior obtained from the interfacial impedance, measured as a function of D_2 loading, as inferred from axial resistance measurements of the Pd electrode.

Commentary on McKubre paper: McKubre seemed to present nothing beyond what the Panel saw in its visit to his laboratory in June. His basic experimental design is very different from that used by everyone else in this field, and he has taken great pains to maintain a close watch on all experimental variables. He reports effects that amount to bursts, but these were observed in a single run that ended with the cell shorting across the thin gap of electrolyte. I was not able to find out what recent results he has had, if any.

"Investigation of 'Electrochemically Induced nuclear Fusion of Deuterium'--Heat Measurement", by T. R. JOW, E. Plichta, C. Walker, S. Slane, and S. Gilman, U. S. Army Electronics Technology and Devices Laboratory LABCOM, Power Sources Division, Fort Monmouth, NJ 07703-5000

Fleischmann and Pons reported that nuclear radiation and excess heat were observed when deuterium atoms were forced into a cathodically polarized palladium electrode. We have been conducting experiments to attempt to verify the caloric claims under similar electrochemical conditions, but utilizing a conduction calorimeter rather than the quasi-adiabatic instrumentation of the original investigation. The palladium cathodes were wires which were subjected to a variety of metallurgical and surface treatments. Control experiments were run using Pd/LiOH, H_2O and Pt/LiOD, D_2O electrodes. Thus far, the controls and all other experiments have yielded caloric outputs which are in close agreement with simple theory excluding significant 'excess' heat.

Commentary on Jow paper: Jow used open cells in a heat flow calorimeter and studied a wide variety of experimental conditions. Within 2% precision, this group found that the evolved heat matched the input electrical power (corrected for the enthalpy of vented gases) under all studied conditions.

"An Electrochemical Calorimetric Search for Evidence of Cold Fusion", by M. H. MILES, K. H. Park, and D. E. Stilwell, Chemistry Division, Research Department, Naval Weapons Center, China Lake, CA 93555

Two different types of calorimetric cell designs were used in this attempt to detect excess enthalphy during the electrolysis of LiOD/D_2O using Pd cathodes. Control experiments were run side-by-side using water in place of D_2O, Pt cathodes in place of Pd, or by reversing the direction of the cell current. No significant differences were detected between the Pd/D_2O cells and the controls. For example, in one series of experiments the ratio of heat out to Joule heat in was 1.00 ± 0.04 for one type of calorimetric cell using a Pd cathode in LiOD/D_2O. For the other type of calorimetric cell, this ration was 1.065 ± 0.04 in LiOD/D_2O compared to 1.075 ± 0.07 in LiOH/H_2O. Studies on different sources of Pd are in progress.

Commentary on Miles paper: This group used an experimental design similar to that of Landau. In studies obviously lasting over months, they measured no excess heats, as they report in the abstract. However, just before the meeting, they developed positive effects. They had begun some experiments with a new Johnson-Matthey Pd rod of 6.35 mm dia., and after 10 days of electrolysis, they developed a heat excess in long-lived bursts of 10-20%. The cell temperature was about 1.5° above where it should have been for no excess. The needed make-up water did not fall behind the amounts expected during the bursts, but actually seemed to increase. No evidence other than this was provided for the absence of recombination. This rod has not yet been run in a control configuration, or even in a second trial under the same conditions. This group obviously was prepared initially to make a negative report, but now seem to think positive effects might be real.

"Electrochemical and Calorimetric Investigations of the Pd-D System", by L. REDEY, K. M. Myles, D. Dees, M. Krumpelt, and D. R. Vissers, Electrochemical Technology Program, Chemical Technology Division, Argonne National Laboratory, 9700 South Cass Ave., Argonne, IL 60439-4837

The purpose of these experiments was to investigate the energetics of the electrochemical formation of Pd-D and Pd-H systems and to find those conditions of cell operations that may produce excess heat useful for energy production. We have performed two series of experiments: (1) differential comparison of temperature deviations between $(H_2)PdH_x/LiOH$ saturated $H_2O/Pt(O_2)$ and $(D_2)PdD_x/LiOD$ saturated $D_2O/Pt(O_2)$ cells and (2) calorimetric measurements of $(D_2)PdD_x/LiOD$ saturated $D_2O/Pt(O_2)$ cells under varying electrochemical conditions. The D/Pd ratio was monitored during the investigation. Effect of current density and duration of electrolysis on heat generation, value of x, gas recombination, electrode potential, and current efficiency were investigated. No excess heat was observed in long duration experiments within the substantial high sensitivity (13 mW/cm^2 area of Pd) and precision limits of the specially designed constant heat-loss-rate calorimeter.

Commentary on Redey paper: The Argonne effort on calorimetry was summarized by Redey. Their system was of novel design, at least with respect to others used in this field. It was based on the maintenance of a constant heat loss rate between two constant-temperature sinks. No positive effects were seen in any experiment. Open cells were used, but data were provided indicating precise agreement between the make-up water volume and the expectation based on Faraday's law. It appeared that the precision of the calorimetry was roughly 5%.

Energy Effects during the Electrolysis of D_2O with Pd and Pt Electrodes", by V. C. NONINSKI and C. I. Noninski, Laboratory on Electrochemistry of Renewed Interface (LEPGER), P. O. Box 9, Sofia 1504, Bulgaria

Calorimetric experiments are carried out while the electrolysis of D_2O using Pt and Pd electrodes is taking place. It was found out that more energy is being produced than the energy spent for the process. This fact is a confirmation of a similar energy effect, firstly discovered by Fleischmann and Pons. The analysis made of the effect leads to the conclusion that no other explanation can be given for now than the initial proposition made by Fleischmann and Pons that it is due to nuclear fusion.

Commentary on Noninski paper: The authors did not appear for this presentation, so the paper was not given.

"A Flow Calorimeter used in Duplication of 'Cold Fusion'", by N. HUANG, Q. H. Gao, and B. Y. Liaw, Hawaii Natural Energy Institute, University of Hawaii at Manoa, 2540 Dole St., Honolulu, HI 96822; B. E. Liebert, Dept. of Mechanical Engineering, University of Hawaii at Manoa, 2540 Dole St., Honolulu, HI 96822

A flow calorimeter was used to measure the excess heat in a Pons-Fleischmann-type "cold fusion" cell with arc-melted 99.9% Pd as cathode in 0.1M LiOD-D_2O (99.9%) electrolyte. The heat rate was derived from the temperature difference of a steadily circulating water flow between the inlet and outlet of a Dewar flask. Excess heat generation was observed after 30 days of operation.

Commentary on Huang paper: Even though the abstract suggests that positive heat effects were seen, the data shown in the slides indicated that the precision of the measurements was comparable to the claimed excesses. In individual runs there were periods of apparent heat deficiencies as well as excesses, but the scatter of the points was such that one could not regard either effect as statistically significant.

"Cold Nuclear Fusion Research at Cal Poly Pomona: Some Preliminary Data", by R. T. Bush and R. D. EAGLETON, Physics Dept., California State Polytechnic University, Pomona, 3801 West Temple Ave., Pomona, CA 91768; and J. F. Stephenson, Chemistry Dept., California State Polytechnic University, Pomona, 3801 West Temple Ave., Pomona, CA 91768

California State Polytechnic University, Pomona, has begun an experimental effort of cold fusion and preliminary results should be available by conference time. The production of heat and tritium will be checked for a variety of charging currents (above and below 1A) sample masses (1 to 15g), and sample geometries. An attempt will also be made to monitor the emission of neutrons and to detect excess He^4.

Commentary on Eagleton paper: This group had no results. They described only how they plan to go about their experiments.

Richard L. Garwin
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October 25, 1989
(Via Airborne Express)

Mr. John P. Schiffer
Physics Letters B
Editor of Experimental Intermediate-
Energy and Heavy-Ion Nuclear Physics
Physics Division - 203
Argonne National Laboratory
Argonne, IL 60439-4843

Dear John,

Enclosed is the package of materials I just received by Airborne Express from P.K. Iyengar.

I am sending this also by Airborne Express to Jake Bigeleisen, Darleane Hoffman, and Bill Woodard.

I have two other items of interest. I have just talked with Gerd Friedlander, who I gather is going to talk to the Cold Fusion Panel Monday (where I will not be). They now see clearly He3, and find it roughly equal to the T output in their experiments. Certainly, according to Friedlander, it is at least 80 percent of the T yield. As a matter of general interest, the acceleration of clusters of light water gives a yield about 5 percent that obtained with heavy water clusters (both against a deuterated polyethylene target). Interestingly, with heavy water clusters against a normal polyethylene target, they see less than one percent of the yield obtained with deuterated polyethylene target.

I have spoken with Mike Miller, of Menlove's group at LANL. Over the last 6 weeks, about as many runs with hydrogen as with deuterium; no bursts with hydrogen, and more deuterium runs give nothing than give bursts. Still no separation of counters.

Finally, I have a letter from from Etienne Roth, who says that absolutely no positive results have been observed in France.

I am now puzzling over Iyengar's papers.

Also Adjunct Professor of Physics at Columbia University
(Views not necessarily those of IBM or Columbia)

भारत सरकार
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BHABHA ATOMIC RESEARCH CENTRE

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: 551 49 10
RES : 551 61 63
TLX : 011-71017-IN
GRAM : BARC-CHEMBUR

Dr. P. K. Iyengar
Director

Ref:DIR- 468 -89

October 19, 1989

Dear Dr. Garwin:

Thank you for your letter of October 16th. As requested I am sending you a copy of the paper B-3, which will be in our compilation.

During my visit to U.S.A. I had occasion to discuss cold fusion with a number of people especially those at A&M University, College Station, Texas, Texas University at Austin and Brookhaven National Laboratory. I am pretty sure now that there is good confirmation of our results from the experiments done at A&M Texas. The work done at BNL using cluster beam also indicates tritium channel dominant compared to the neutron channel. I do not particularly think the explanation on the basis of impact fusion is strictly correct. The clusters probably disintegrate and provided deuterons of energy from a few electron volts to a few KeV. It is not surprising that the results have shown only tritons and protons.

You are also probably aware of a Russion paper on the high isotope ratio 3Helium/4Helium in technical grade metals, published in 1978. It is surprising that they make this statement "It can be assumed that 3He in metals is formed as the result of processes which occur right in the metal and these processes lead to the formation of either -3He, T, or T and -3He together. An analog of processes of this type is the nuclear reation which takes place with the interaction of deuterium mesic atoms with ordinary deuterium atoms at thermal energies μ-d + d -- 3He + n + μ-." It looks as if cold fusion was invented **as early as in 1978!**

The question of reproducibility has engaged the attention of many. I am afraid most of the chemists deal with very small electrodes which is one of the reasons for their poor success. I feel if large surface and large volume electrolytic cells are used, the success rate is higher because conditions necessary for cold fusion reaction either at the surface or in the volume is more likely to be achieved. What these conditions are, I do not venture to predict, but perhaps will be revealed by many experiments being done at present.

We have looked at some of the deuterated titanium targets obtained for low energy nuclear physics experiments from Amersham a long time ago. Presently they show auto-radiographs from tritium formed on the target, similar to the one described in my paper. We have also verified this by detecting titanium x-rays

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was more uniform. Unlike deuterated titanium targets, the intensity of fogging reduced very rapidly i.e. within a couple of days the activity reduced below measurement level.

MEASUREMENTS OF X–RAY EMISSION

The characteristic X–rays emitted from the deuterated metals (Ti and Pd) were studied with the help of a Si – Li (Silicon – Lithium drifted) detector by the Nuclear Physics Division. The detector had a beryllium window of 75 μm thickness. The X–rays of Ti ($K\alpha$ = 4.5 Kev, $K\beta$= 4.9 Kev) were observed in case of conical (Fig.4) and disc (Fig.5) sample. The intensity of conical sample was much more than that of disc sample. The deuterated Pd–Ag foils indicated the X–ray peaks (Fig.6) corresponding to titanium presumably because of a small amount of titanium impurity picked up by the foils from the D_2 loading chamber which had earlier been used for loading of Ti samples. We did not observe the L X–rays of palladium or silver.

LIQUID SCINTILLATION COUNTING

This was carried out at the Health Physics Division using the facilities described in Ref.(4). The sample was simply dropped into a vial containing liquid scintillator cocktail and the tritium activity was counted by two photomultiplier tubes in coincidence. The typical activities were 50 to 1000 Bq as compared to a background of less than 0.2 Bq. No correction was applied for possible quenching/shadowing effects.

RESULTS AND DISCUSSION

The fogging observed in autoradiographs (Fig 1,2 & 3) is the combined effect of tritium and characteristic X–rays of the host material. The radiograph of the disc sample (Fig.1) indicates evidence of tritium localised in the form of micro structures. These spots are unevenly distributed on the face of the titanium, there are about 60 to 70 spots in all. On correlation with the X–ray counts under the peak (K X–ray peak) and liquid scintillation counting result it was found that the each emitting spot corresponds to 10^8 to 10^9 atoms of tritium, In comparison the total number of deuterium atoms loaded in the disc sample was $10^{19}$to 10^{20}. The X–ray images (Fig.3) in case of Pd–Ag foils were uniformly fogged and intensity of fogging was reduced very rapidly with time unlike ~~tritium~~ Titanium. This type of change in pattern was attributed to the high mobility of tritium in palladium as compared to that in titanium. Observation of K X–ray peaks of titanium (Fig.4, 5 & 6) by Si–Li detector was the result of excitation of K–shell by tritium or tritium β. L X–ray of palladium or silver was not observed because of low fluorescent yield for L X–ray and the detector window (75 μm) was too thick to allow observable quantity of L X–rays. Liquid scintillation counting further confirms the presence of tritium in the samples. It may also be pointed out that the tritium observed even on the surface of the samples exceeded the total quantity of tritium contained in the deuterium gas used to load the samples, therefore, the gaseous tritium, even if preferentially absorbed by the samples can not explain this phenomena. Undeuterated metallic targets machined out of same titanium rods did not indicate any detectable tritium signal.

SUMMARY AND CONCLUSION

(i) Our experiments based on the titanium, obtained from Misra Dhatu Nigam, India and palladium from Mathley Johnson, U.K. Only a few samples showed cold fusion out of a number of tested samples. The cold fusion may be very rare and random phenomena and occurs in selected samples. This perhaps is the reason for controversy regarding the existence of cold fusion.

(ii) The autoradiographs show the fusion sites in the sample. The fusion may have taken place during the loading of sample. Though we fail to locate new sites after first imaging, the fusion phenomena even after loading period can not be ruled out. The fusion may be taking place at the same regions or at any other region with low activity not enough for detection.

(iii) The neutron measurements [5] have not given any positive result of neutron production during or after loading of deuterium. This is quite surprising since in the case of usual D–D fusion reaction producing tritium or neutron, has almost equal cross–section for both the

channels. Even if the neutron production is inhibited due to some unknown reasons, the tritium produced at 750 Kev, reacting with surrounding deuterium, is capable of generating fusion neutrons by the reaction

$$D + T \longrightarrow {}^4He + n\ (14\ MeV)$$

The neutrons produced by above process will be 10^{-4} times the tritium atoms generated. Our detectors were sensitive enough to detect these neutrons if produced. Similar results have also been obtained from our electrolysis based cold fusion experiments [6].

(iv) It was observed that the there was no correlation between the amount of gas loaded and the tritium activity of the samples. Some samples of Pd black, Ti sponge and zirconium powder absorbed very high amount of deuterium but failed to show any cold fusion. In our experiments all the samples that exhibited cold fusion were loaded with deuterium much below the quantity required for phase change.

(v) The samples exhibiting cold fusion seem to have some kind of aging effect. Samples which had shown cold fusion (i.e. autoradiograph) in the first loading also generated tritium in the second deuteration. But failed to show any evidence of cold fusion in the subsequent deuterations.

Acknowledgements

Authors sincerely wish to express their gratefulness to Dr. P K Iyengar, Director, BARC for his keen interest and constant guidance in the present work. We are also thankful to M S Krishnan, S Malhotra, S Shrikhande and K C Mittal for deuteriding the targets. We also grateful to V S Ramamurthy and Madan Lal for the measurement of characteristic X–rays. T S Iyengar has carried out tritium measurements described in this paper the authors would like to express their thanks to him.

References

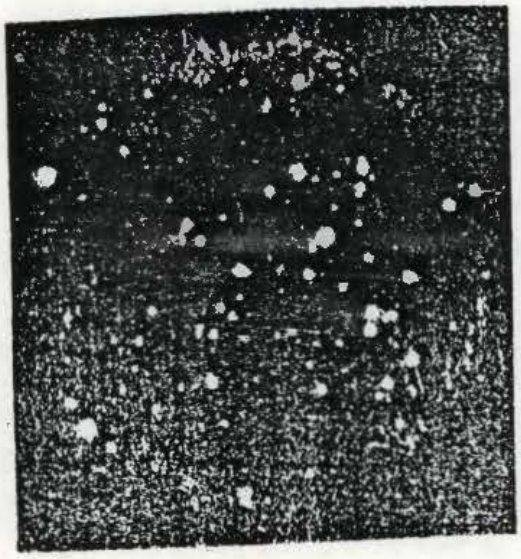
[1] De Nino et al, 1989, Submitted for publication to Europhysics Lett.
[2] Mittal K C and Srikhande V K, 1989 BARC studies in Cold Fusion, BARC report B2.
[3] Krishnan M S et al, 1989, ibid B4.
[4] Murthy T S and Iyengar T S, 1989, ibid A8.
[5] Shyam A et al, 1989, ibid A3.
[6] Krishnan M S et al, 1989, ibid A1.

FIGURES

Fig. 1 Autoradiograph of deuterated Ti disc sample
Fig. 2 Autoradiograph of deuterated Ti conical sample
Fig. 3 Autoradiograph of Pd–Ag foil
Fig. 4 Si–Li detector signal of Ti disc target
Fig. 5 Si–Li detector signal of Ti conical target
Fig. 6 Si–Li detector signal of Pd–Ag foil

Table. I

Sample Identification No.	CS003	DS001	PS001
Figure No.	2	1	3
Material	Ti	Ti	Pd–Ag
Shape of Sample	cone	disc	Foils
Sample Mass (mg)	187	998	110
D_2 Absorbed (mg) D_2 Loading process	0.07 Ref.2	0.42 Ref.2	0.73 Ref.3
Date of D_2 Loading	9–6–89	14–6–89	22–8–89
Date of Exposure	14–6–89	23–6–89	24–8–89
Exposure Duration	24 Hr	66 Hr	88 Hr
Si–Li Result (10^5 Bq) Date of Measurement	1.3 16–6–89	0.29 16–6–89	2.96 24–8–89
Liq–Scin. Result (Bq) Date of Measurement	850 15–6	28 26–6	xx xx
Repetition Rate for Autoradiography	3	9	4



2mm
Fig.1

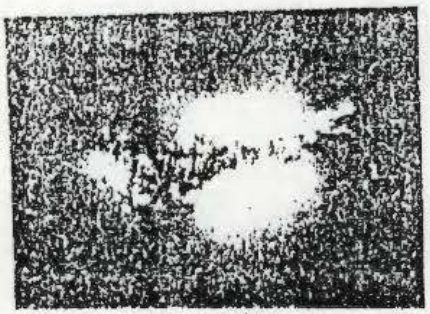


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Fig.2

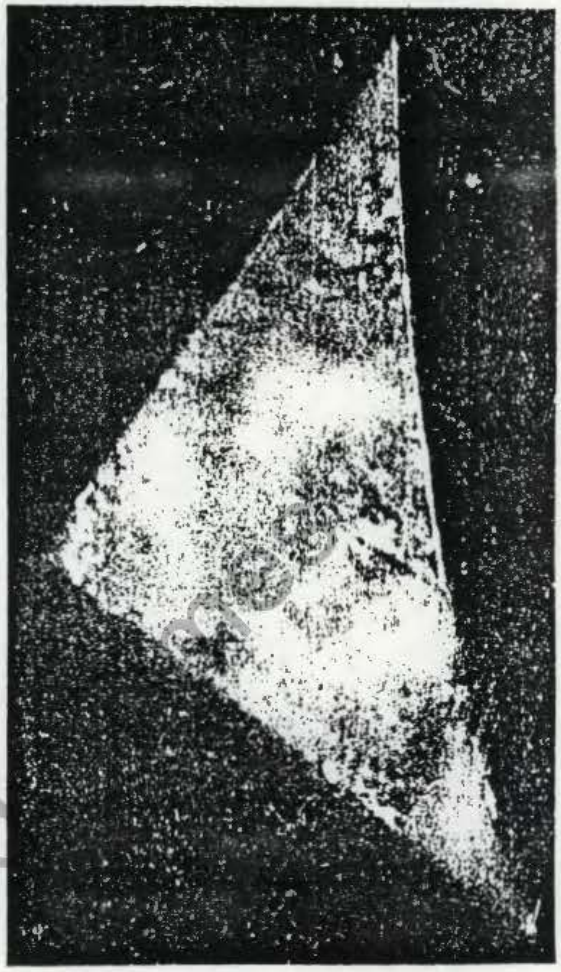

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Fig. 3

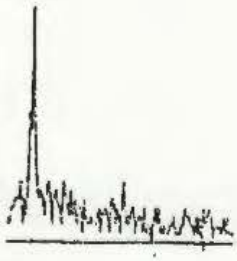

Fig.4

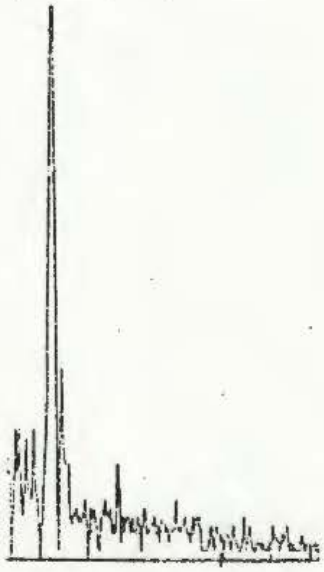

Fig.5

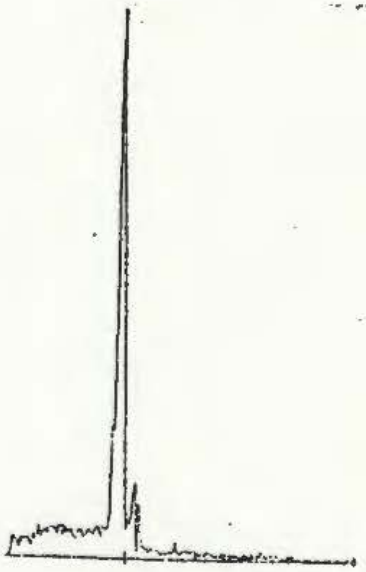

Fig.6

AUTORADIOGRAPHY OF DEUTERATED TARGETS FOR SPATIALLY RESOLVED DETECTION OF TRITIUM PRODUCED BY COLD FUSION

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INTRODUCTION

For the last few months, hectic activity is underway in various laboratories to study the Cold Fusion phenomenon. De Nino et al [1] reported emission of neutron from titanium metal loaded with deuterium gas under pressure. Similar experiments have been conducted at Trombay. We report here evidence of cold fusion in D_2 gas loaded Ti and Pd targets through the use of autoradiography for spatially resolved detection of tritium. Our study employed three different techniques to observe tritium :

(i) Autoradiography using X–ray films.
(ii) Characteristic X–ray measurement of titanium, excited by the tritium β.
(iii) Liquid scintillation method for tritium β counting.

LOADING OF DEUTERIUM

Titanium and palladium metal samples of various shapes and sizes were loaded with deuterium by two different ways. In the first method [2] individual titanium target was heated by R F heating up to a maximum temperature of $\simeq 900^{\circ}$ C in vacuum and then in deuterium gas atmosphere to absorb deuterium. In the second method [3] the foils of palladium (Pd–Ag alloy) were heated (by ohmic heating) up to a temperature of 600° C in vacuum (10^{-5}mm of Hg) and then in D_2 gas. The deuterium gas used for loading had a tritium content of ≤ 3 Bq/cc.

AUTORADIOGRAPHY

Autoradiography is a simple and elegant technique of detecting the presence of radiation emitting zones. This technique has the advantage of being free from any electromagnetic interference (pick ups, discharge pulses etc), has relatively high sensitivity as it can integrate over long exposure times and can give very useful information in the form of space resolved images. In order to achieve good resolution of the image, the sample was kept very close to the X–ray film. Standard medical X–ray film of medium grain size (10 to 15 μm in diameter) on cellulose triacetate base was used for this purpose. The exposure time used for the deuterated samples varied from 18 hours to a few days. At times a stack of several films was used. In some cases films were placed on both sides of the sample. For latent image formation we used IPC (India Photographic Company Ltd.) made 19B developer and IPC made fixer. The developing time was typically 4 to 5 minutes. Out of many samples which had absorbed D_2 gas, only a few showed latent image of the object. The obtained results are tabulated in Table–I.

The radiographs (Fig.1) of deuterated titanium disc target showed several spots randomly distributed within the sample boundary. The occurrence of spots all along the rim of the mechined targets is very intriguing. The X–ray image (Fig.2) of a conical target showed periphery of the cone. Repeated measurements over a period of one month, with same sample with varying exposure time gave almost identical pattern and intensity distribution of spots, indicating that the radiation emitting regions were well entrenched in the face of the titanium lattice. The fact that the second film of a stack of films exposed to the target also indicates similar though less intense spots, rules out the possibility of any kind of chemical reduction reaction caused by the deuterium or hydrogen in the target being responsible for causing the spots.

The latent image (Fig.3) of Pd–Ag foils however exhibited a different kind of behaviour. The images though indicated variation in intensity and some spots but on the whole the fogging

by using silicon detector. The calculation on the basis of radioactivity seen at present and assuming a constant rate of DD cold fusion, the figure of 10^{-12} per DD pair per second is obtained. This checks with the figure that one obtains in the electrolytic cell. I am sure this could be checked since several of these targets must be available in the laboratories. The amount of tritium activity is such that it could not have been present at the time of loading with deuterium gas.

It seems to me certain that there is no way of escaping the fact that DD cold fusion does occur in metallic lattices. It is like discovering that neutron induced fission takes place in uranium. It took a long time before appropriate conditions were invented to make it a source of energy. Perhaps it will take some time before we can obtain appropriate conditions in the metal lattices, or perhaps using external agents by which energy production along with tritium will be feasible. There is definitely a hope.

"Science Marches On".

With best wishes,

Yours sincerely,

(P.K. Iyengar)

Encl:

Dr. Richard L. Garwin
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Yorktown Heights, NY 10598
U.S.A.

Sincerely yours,

Richard L. Garwin

Encl:
10/19/89 LTR P.K. Iyengar to RLG (including attachments). (101989.PKI)

cc:
> J. Bigeleisen, Stony Brook (Via Airborne Express).
> D.C. Hoffman, Berkeley (Via Airborne Express).
> W.L. Woodard, ERAB (Via Airborne Express).

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COLD FUSION

P.K. Iyengar

Energy production in the universe is mostly based on nuclear reactions especially fusion reactions of light elements. Energy production in the sun on the basis of fusion of hydrogen, its isotopes and elements upto carbon have been theorized. It is quite natural to expect that there will be a large variety of nuclear reactions which will lead to the production of nuclear energy. This would depend upon the mass of the object, the temperature and other forces which can contribute to density changes and changes in the potential barrier for interaction between nuclei. In fact even before the discovery of the neutrons, scientists had predicted and even tried to prove that nuclear energy could be produced by fusion of hydrogen atoms. It was only after detailed accelerator based research in nuclear physics, that the cross-sections and Q values were available. This enabled many conjectures to be made. In fact there is a whole area of fusion reactions in astrophysics in which one can imagine various types of nuclear interactions and modes of decay for energy production. The collapse of the binary stars and neutron stars turning to black holes are the ultimate phases of the production of fusion energy. It is, therefore, very interesting to renew our acquaintance with yet another new methoding of fusion energy production in the context of the prediction of cold fusion.

There is a set of nuclear reactions involving hydrogen isotopes which can lead to production of energy, as well as continuing the chain reactions in special circumstances. These

are listed below:

$p+p \quad - \quad D+e^{+}+$

$p+D \quad - \quad He^{3}+e^{+}+e^{-}$

$D+D \quad - \quad He^{3}+n$

$D+D \quad - \quad p+T$

$D+T \quad - \quad He^{4}+n$

However, from our familiarity with accelerator based nuclear reactions, scientists were content to imagine that fusion reactions can take place only on the basis of overcoming potential barriers due to the electro static interaction which requires relative motion of the particles with considerable energy. From the analogy of what has happened in the sun, thermo nuclear fusion was therefore considered as the most appropriate technique. We are aware that in the last 4 decades many attempts have been made to obtain conditions appropriate for thermo nuclear fusion. The technique of generating high temperatures of 100 million degrees in small volumes using the principle of inertial confinement was adopted in the creation of the thermo-nuclear bomb. The same principle is borrowed and adapted in making fusion reactions possible in small pellets using lasers, electron beams and heavy ion beams. The principle of the confinement of plasma through magnetic fields in special configurations were also invented in the early 50s. Of these the

Tokamaks has been the major success. It has almost reached the stage of breakeven in energy production. However, the large size and the expensive equipment needed to attain even this breakeven stage has led to doubts about its commercial viability. Small and more elegant methods have been developed. The plasma focus, exploding wire and shock wave compression are all techniques which are under experimentation with the sole aim of producing neutrons from the fusion reactions. It is interesting to note that in this process considerable innovations and cross fertilization of new ideas have taken place in the last couple of decades. Therefore, any addition to these new ideas is welcome and must be tried out. What we are going to discuss today is an innovation which on the face of it looks so simple that it seems too good to be true. It has also generated considerable speculations on the process which cause fusion in the solid state at low temperatures. The basic problem is essentially to bring together ions of hydrogen isotopes at distances of few Fermis so that fusion takes place. In this connection it is worth recalling the attempts to bring together hydrogen nuclei to a distance at which the spontaneous fusioning rate would increase considerably. The most effective method has been the replacement of the orbital electron by a μ mason. Because of its heavier mass, it is able to squeeze the nuclei in a molecule and increase the probability of fusion. It also has an additional advantage. Because of its longer life time, freed after the reaction it can catalyze more fusions. Almost 200 catalyzed fusions per μ mason have been observed. It is the same scientists who were concerned with μ mason catalyzed fusion that have also reported cold fusion in a

lattice of palladium.

From the point of view of understanding physics behind cold fusion one needs to discuss the structure, and rearrangements when hydrogen is absorbed in the lattice of palladium, titanium or other alloys used for the storage of hydrogen. The fact that enormous quantities of hydrogen are stored in such lattices first gave an indication that perhaps the internuclear distances can be brought down in such lattices. The electrolytic cell mainly performs the function of infusing hydrogen ions into the metal. The exact positions that the hydrogen ions take, their relative distances, and their interaction with the host atoms especially with respect to the orbitals, the diffusion rates and their sensitivity to other environmental conditions need to be known before one can explain the fusion rates that are claimed to be observed.

Many attempts have been made by the theorists to quickly evaluate the fusioning rate in such lattices. Many of these theories are based on the extension of the well known theory for catalyzed fusion extended to a situation in which both the internucleon distances and the height of the potential barrier are varied. Both will have the effect of increasing the fusion rate from 10^{-64} per second/per pair to something of the order of 10^{-23} per second per pair which is required in order to produce the right amount of energy. Whether it is possible to have such drastic changes in the probability phenomenon which is

essentially dictated by quantum mechanics is very debatable.

I would like to invite your attention to one of the unaccepted involvement of the theory of quantum mechanics to such a problem. Several years ago Rand McNally had invented the basic phenomenon of cold fusion and in fact he had named it in 1983 to quote in his own words "The problem of neutron transfer in solid media is no longer an elementary binary collision process involving Coulomb barriers and brief collision times but rather one in which the nuclei are continuously in each others proximity. Since the ^{136}Xe has a slow neutron capture radius approaching that of the inter atomic distance the nuclear barrier would perhaps be grossly reduced. Thus it is remotely possible that some combination of natural processes may permit barrier penetration to occur much more readily and a nuclear reaction to ensue". He also invented the term called de Broglie interaction length which is based on the fact that the de Broglie wave length of particles with small kinetic energy are in fact very long. In fact it is this property that provides an extraordinarily high cross section for absorption of slow neutrons by certain nuclei. It is in this connection that one must know what the de Broglie interaction length and effect of the extension of the de Broglie waves will be in the case of a hydrogen atom with very small energies in a palladium lattice. What would happen to the charge distribution of such a deuteron and its effect like polarisation are too speculative. Perhaps if the charged distribution has dimensions of the order of de Broglie interaction length, then the potential barrier due to Coulomb interaction could be made

much smaller. If it is so then cold fusion should be much more probable at very low temperatures.

From the experimenters point of view the proof of the cold fusion must come from a demonstration of the number of neutrons, He^3, tritium, gamma rays, He^4, etc., which could be the end products of the reactions. Unfortunately experiments performed so far have used very small electrodes and small cells There have not been sufficiently large experiments which bring out unqualified proof of the number of neutrons or radioactivity from this process. Our attempts in different groups here in Trombay have all shown, reliable data on neutron and tritium production. There will be reports of this later. It is also interesting that the same reactions can be simulated by introducing high density deuterium ions in a lattice by simply compressing deuterium gas into titanium. The Italian group has in particular succeeded in producing neutrons by this method. It is not necessary to doubt the capability of the group in Frascati.

I will not go into greater detail at this stage. It is necessary for us to involve ourselves deeply into understanding the mysteries of this new phenomenon. It is not our expectation that this will become an energy source tomorrow or the day after. After all even in 1939 when the neutron induced fission was discovered in uranium it took several years to find out the method of producing fission energy. Without detailed measurement of the number of neutrons produced in fission by the Columbia

University group and the invention by Fermi, the physics of heterogeneous multiplying system, the nuclear reactor would not have been invented. Similarly one has to explore and understand the basic mechanisms of fusion in a lattice and find out how this could be used either to produce energy directly or to produce neutrons and tritium or both in a sustained manner. It is too early to make time targets but when one is familiar with the nuclear technology one could foresee how it will change our efforts in many areas. "A world of copious neutrons" was the topic of a conference in 1982. The fast reactor essentially uses the reactor as a source of extra neutrons, so does the fission fusion hybrid. It is therefore quite conceivable that cold fusion may turn out to be yet another source of neutrons for applications.

It has been conjectured that there are other channels by which the fusion reactors can produce energy with almost little or no radioactivity. This is a very interesting concept which can be used effectively to produce fusion energy in small devices. Availability of neutrons for isotope production is yet another area for applications. Neutron therapy & neutron radiography, both can be done with portable neutron source to great advantage. Above all the source of such energy and neutrons depends only on the sea water which is available in plenty and the technology to separate and concentrate deuterium from water is well known.

I would therefore end this introduction by quoting from what Dr. Bhabha said at the first International Conference in

Peaceful Uses of atomic energy in 1955. He predicted that "in a couple of decades from now fusion energy will become possible and that will ensure energy production for man's needs as long as there is sea water on this earth". It is, therefore an historical occasion for us to renew our effort in research and development in an area so vital for human prosperity. I welcome you all to this one day discussion and I hope this will stimulate the interest of scientists and engineers from various disciplines to put their work in such a way that we lead in an evolving technology.

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Dear Alan:

I write further in addition to the remarks I made concerning specific aspects of your paper at the NSF-EPRI meeting on cold fusion.

The following refers to the fact that you began by pointing out that the evidence available to the committee was largely that which corresponded to the Santa Fe meeting.

This meeting was held about two months after the Discovery. It is now seven months after, and in the intervening five months much evidence has come to pass which I think you and your committee may not have yet weighed.

Thus, in my view, the principal evidence for the existence of nuclear reactions in the deuterium evolution on palladium arises from the observation of tritium formation. This has been done at Case-Western, Gaineville, Texas A&M, Los Alamos and Rockwell.

It has also been reported from the Barba Atomic Research Institute, the Institute of Petroleum in Mexico, three groups in the U.S.S.R. (reported by Fritz Will from General Electric), three groups in China (reported to me from Professor Cai at the University of Peking) and two groups in Korea (reported to me personally during my recent trip there).

It is in the nature of things that publications from all these works cannot yet be available. However, I'm sure that were you to write to these groups and make inquiries, they would be likely to send you facts and results ahead of publication time.

I think that, in view of the widespread confirmation of the results first obtained in Gainesville and at Texas A&M, any explanation of these results in terms of contamination is in the category of "extremely unlikely".

Strong additional evidence is given, in my view, by the observations of O'Grady at NRL that the ratio of the isotopes of palladium is changed when deuterium is evolved at them, but not when hydrogen is similarly evolved.

As to the reports of neutron production at the electrodes, these have grown so frequent that I have lost count of them. There are certainly more than 20 laboratories which have reported this. There were seven new ones in Kyoto alone.

What are the remaining difficulties to the acceptance of cold fusion as a scientific field worthy of serious investigation on a wide scale?

1) The most important barrier is the emotional denial and non-objective reactions of many fusion physicists. Reasons for this behavior, of course, are too obvious for me to describe to you.

2) There is a difficulty in the fact that, as yet, the results cannot be switched on and the phenomena cannot be manifested as desired. However, it would not be true to say that it is adventitious and capricious. The confirmations found follow a pattern.

As far as tritium is concerned, we here have had positive results from 14 electrodes out of 20. The key is waiting a very long time. We have never seen tritium under five days at high current density (with earlier preliminary charging over several weeks). In some cases, we have had to wait as much as 50 days before the tritium has finally switched on.

Further, we have learned that tritium can switch off again, and then (it is probably present as DT), the sparging with deuterium makes it fade so that unless daily observations are made the bursts can be missed.

Thus, one understands the people who have made experiments which they describe as negative. They did not pursue the work for a sufficiently long enough times, and in addition (but more curiously) did not go above about 0.1 amps per square centimeter in current density.

A good example of all this is in the work of David Williams at Harwell, which has been examined in detail by John Appleby.

A conversation between Williams and myself took place in the presence of Bob Sherman in my room. A speakerphone was used. Thus, there is witness to statement by Williams in which he declared that he would have to report the phenomena as non-existent on the basis that they were inconsistent and he could not obtain reproducible results.

I asked him then directly whether he saw any heat bursts, and he replied, approximately, "Of course, I see bursts, but I cannot know when they are coming and they are not regular".

He made a similar remark on the neutrons. When he came to the tritium, he reported a doubling of tritium concentration but admitted that the D_2O which he had used was of World War II vintage and had enormous concentrations of tritium to start with.

If one blindly wants simple reproducibility at demand as the touchstone for the existence of the phenomena, the evidence does not yet exist.

However, there are now so many reports which are similar in magnitude of

heat observed (mostly 10-30%, occasionally 200%), neutrons observed (mostly a few times background, occasionally up to 10 times), and tritium (between 10^3 and 10^7 disintegrations per minute per milliliter).

These observations have sufficient comparability to act as a basis for the assertion that nuclear activity of the electrodes repeatedly observed.

To finish the statement and make my position entirely clear:

a) I have no position at all on any possible usefulness of these effects. However, if the bursts can be repeated and controlled, they appear to produce energy at about the same density as that of a nuclear reactor.

b) There is no doubt that work on cold fusion is difficult to do (and here I think Fleischmann and Pons did mislead us). The major trouble is the boredom of not seeing anything for 2-3 weeks, and sometimes more, and the necessity of having 10-20 electrodes of going at the same time.

These difficulties, however, have little bearing on the question: Does fusion sometimes occur at electrodes? Here, I must now give my firm opinion in the positive sense.

Last of all, there is the nuclear theory and the difficulties of the branching ratio not being 1, etc. Had you heard Teller speak in Washington, these problems would worry you less than they probably do. He treated the contradictions calmly, and spoke of necessary revisions to nuclear theory which would have to be made, and even made some very erudite statements about quarks and possible crossing of the Gamow barrier by such entities, (As they are much smaller in mass then a deuteron, the tunnelling would be much more probable).

However, in a general way, Science advances by taking facts first. All we should be doing at the moment is to be making quite sure that those facts are solid and trying to obtain enough liberality of funding to enable us to find out how to reproduce them more frequently and in an easier manner.

With good wishes to your committee, which I fear is dominated by persons who have an extremely strong vested interest in denying the existence of the phenomena, and who have often no appreciation of the weight of evidence which now exists. In England, they tell school children the story about King Canute. He said he was so powerful, he could stop the tide coming in. The legend says he sat on the shore until -- My hope is, - you'll prevent the members of the ERAB Committee getting wet.

Sincerely,

J. O'M. Bockris