# FUSION facts

A Monthly Newsletter Providing Factual Reports on Cold Fusion Developments

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*Fusion Facts* Now Reports on Both Cold Fusion and Other Enhanced Energy Devices.

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## ERRATA

Due to problems with our collator, some copies of *Fusion Facts* for July 1992 were sent without pages 5 & 6. We have enclosed an extra page 5 & 6, in case your copy was defective. Our apologies.

## *A. COLD FUSION ATTRACTING INVESTORS*

Shortly after the University of Utah announcement of the discovery of cold fusion (by Professors Pons and Fleischmann, March 23, 1989), Hal Fox confidently predicted it would take only two years to commercialize. The professors and Fox both have been critized. However, Pons and Fleischmann were right (cold fusion is real), Hal Fox was wrong (commercialization is taking longer).

Sometime later, *Fusion Facts* defined that 300% excess heat is a suitable measure for a device to have commercial potential. The rationale is: high-cost electrical energy is being supplied and lower-cost heat energy is being generated. The ratio of cost is about three to one (e.g., electric power cost versus natural gas cost.) Several experimental teams are obtaining results that are close to 300% excess heat in both Pd and Ni electrochemical cells. Cold fusion, therefore, is finally showing commercial potential. In addition, the recent announcement by MITI and the more favorable press reception given to Takahashi's work and to its replication (see NEW MEXICO, this issue) has generated increased interest in the funding of cold fusion projects.

Since the Pons-Fleischman announcement other key events have been: 1) replication by over 200 laboratory groups in more than 25 countries; 2) evidence that the nuclear reactions produced tritium about a million times more than neutrons; 3) demonstration of the importance of a high D/Pd loading ratio; 4) discovery of the importance of resonance conditions; 5) discovery and replication of light-water excess heat experiments; 6) verification of effects predicted by models; 7) successes using Pd/Ag alloys; 8) determination that $^4$He is a nuclear byproduct of cold fusion; 9) announcement and international replication of the Takahashi experiment; 10) discovery of many anomalies that mark cold fusion as a new science; and 11) refusal of a few scientists to recognize the validity of peer-reviewed, widely-replicated, experimental evidence.

"I'll believe it when I read it in the *Wall Street Journal*," was the comment of a businessman sitting next to me on an airplane trip in the U.S. in 1990. In the early days of cold fusion, a news release to the *Wall Street Journal* or to the *New York Times* and to similar publications would be checked for believability with "friends" at Princeton, Harvard, Yale, MIT, Columbia, and sometimes Cal Tech and receive a rather, negative reception. The fact that replication of the early work in cold fusion was not easy and that nearly all of the "important" schools had tried and failed to reproduce significant positive results led to the belief among many scientists that it was a "Utah Effect." Positive reports were dismissed as errors or delusions. Skeptics refused to read the peer-reviewed literature. Therefore, corporations showed little interest in cold fusion and none in funding development work.

Thanks to the continued efforts of many (about 200 - 300) dedicated scientists around the world, research work in cold fusion continued; professional meetings were held; papers were prepared, peer-reviewed, and published; and the news gradually spread that cold fusion was real but not fully understood. Several valiant editors have provided a forum for cold fusion. Chief among these are *Fusion Technology*, *21st Century Science and Technology*, *Nuovo Cimento*, the *Journal of Electroanalytical Chemistry*, and this newsletter. Among the non-technical press, the *Wall Street Journal* has been fair in its presentations.

Serious treatment of the reality of cold fusion began to increase shortly after the Second Annual Conference on Cold Fusion held in Como, Italy, June-July (1991). The replication of both light-water electrochemical cells and of the Takahashi experiment during 1992 (and still continuing) has spurred a gradually increasing interest in cold fusion by sophisticated investors. One of the foremost and far-sighted groups has been the Electrical Power Research Institute in the United States (an estimated $3 million spent). Modest funding of cold fusion has been achieved in Spain, Italy, the former USSR, India, Taiwan, China, and Japan. In terms of international patents filed, Japanese entities have filed about one-half of the patent applications.

In the United States, at least three government energy laboratories and another three or four Department of Defense laboratories are still doing some work in cold fusion. All have published positive results. In terms of on-going work in cold fusion in universities in the United States, Texas A&M; Cal Poly - Pomona; Univ. of Hawaii; and the University of Utah all have some modest funded programs. Limited, private research work is being done at an estimated six other U.S. universities and colleges. In addition, several (an estimated ten) U.S. corporations are carrying on with small-scale research and development programs.

At least six funding groups are seriously considering the funding of further development of cold fusion. Several large corporations are considering funding local university groups who have made significant developments in cold fusion. A few entrepreneurs are working in the area. A few large corporations are funding or planning to fund cold fusion research. The Fusion Information Center (Salt Lake City, Utah), publishers of *Fusion Facts*, and *Fusion Briefings*, have had the privilege of helping its callers save many thousands of dollars in research funds by providing direct linkages between corporate R&D managers and research groups who were already working in similar fields but not known to those who called the Center for information. We are pleased to be able to provide this modest service to subscribers involved in the development of cold fusion.

**The Salt Lake City, Utah based Fusion Information Center (FIC) will continue to help provide released database information with its subscribers in its continuing efforts to speed the progress of cold fusion. FIC must honor the confidences that are from time-to-time entrusted to us. However, to be forthright with our readers and callers, we announce that FIC has a vested interest in Fusion Energy Applied Technology [FEAT], Inc.** FEAT is a Utah company dedicated to the commercialization of cold fusion.

FIC will do its best to make recommendations to its callers about other companies who are also interested in developing cold fusion energy systems. FIC can only share information that it receives and is permitted to share. If you have any ventures in enhanced energy systems, FIC will be pleased to receive such information and handle such news as being "for publication" or "for verbal referrals only." *Fusion Facts* is willing to publish advertisements from both entrepreneurs and funding entities if you will contact our office.

**In summary**, the new science of cold fusion is approaching that stage at which development of commercial applications makes both scientific and commercial sense. Regardless of the fact that there are still many scientists and engineers who are, as yet, unconvinced of the reality of the new science of cold fusion, there are dramatic developments that have laid the basis for commercialization of this new science. Some of these experimental results are still pending publication. As fast as we can obtain permission to share the latest work with you, you will read the summary in *Fusion Facts*.

**NEWS RELEASE - CLUSTRON SCIENCES CORP.**

On Agust 10, 1992, we received a Press Release from Dr. Eugene Mallove and Russell George concerning the funding of Clustron Sciences Corporation by Venture America, located in the Washington, D. C. area. The

company, Clustron Sciences Corporation (CSC) is based on a hitherto unpublished "new model of the atomic nucleus, which will revolutionize a wide range of energy and materials technologies." The predecessor of the CSC organization, Nuclear Science Research Corporation (NSCR), developed this unique and comprehensive atomic nucleus model. The Nucleon Cluster Model (NCM) was conceived by Ronald A. Brightsen who is the Chief Executive Officer of CSC.

A scientific paper describing the application of the NCM to the conventional fission processes has been submitted to *Fusion Technology*. In addition, Brightsen and Eugene Mallove have submitted a scientific paper to *Fusion Technology* which describes the use of the NCM for explaining cold fusion. Our request for copies of these papers was not filled by publication deadline.

## B. THE UPPSALA SYMPOSIUM
**Courtesy of Dr. Peter Glück**

ABSTRACTS OF SELECTED PAPERS FROM THE INTERNATIONAL SYMPOSIUM ON METAL-HYDROGEN SYSTEMS, FUNDAMENTALS AND APPLICATIONS, UPPSALA, SWEDEN, JUNE 8-12, 1992.

Gregory Jerkiewicz (Univ de Sherbrooke, Dept of Chem, Sherbrooke, Canada), "Electrochemical and Chemical Aspects of Metal-Hydrogen Systems."

AUTHOR'S ABSTRACT

Various recent papers on metal hydrides demonstrate that there is a great deal of misunderstanding surrounding electrochemical aspects of metal-hydrogen systems. In the present paper, the author divides numerous hydrogen compounds into major categories (metallic, ionic, covalent) with respect to the nature of their chemical bonding. Attention is also given to the electrochemical behavior of some noble (Pt, Pd, Rh, etc.) and non-noble (Ni, Fe, etc.) metals in the "electrochemical environment" at various potentials. Special emphasis is put upon such processes as: hydrogen evolution reaction (HERA), corrosion, surface poisoning, oxide formation/reduction prior to H adsorption by metals/alloys and during H desorption, and some selective aspects of other metal-hydrogen systems. The main idea behind this paper, is to demonstrate the complexity of chemical and electrochemical behavior of numerous metal-hydrogen systems and to combine the widely-scattered knowledge and references into one self-consistent review.

L. Stauffer & H. Ezzehar (Lab de Physique et de Spectroscopie Electronique, Mulhouse, France), and H. Dreysse (Lab de Physique des solides, Vandoeuvre-Les-Nancy, France), "Stability of Hydrogen Clusters Near a Transition Metal Surface."

AUTHORS' ABSTRACT

Due to the importance of the surface properties, hydrogen clusters near a metallic surface provide an exciting challenge. The determination of multi-atom interactions is essential in order to describe the hydrogen arrangements when the hydrogen concentration increases in a metallic host. Moreover, the initial stages of the growth of hydrogen near the surface can be understood in this way. In a tight binding model, using generalized phase shifts, we proposed recently an unified framework to obtain the electronic structure of impurities clusters in a metallic host. The cluster interactions with different orientations relative to the surface can now be exactly computed. Various applications of this formalism will be presented. Particularly:

* Applied to the H/Pd(001) and H/Ni(001) systems, it allows us to understand the difference of the behavior between the Pd(001) and Ni(001) surfaces under H chemisorption. In the case of H in Ni we obtain very small interaction energies and so can explain the L.E.E.D. measurements indicate no formation of ordered layers. In the case of H in Pd, we explain the experimentally observed c(2x2) order and describe the initial stages of the growth.
* The H/Pd(111) system is a very complex system, with two possible adsorption phases. The energies are very close. The importance of the various cluster interactions will be discussed in detail.

Y.Q. Lei, D.L. Sun, Y.L. Chen, J. Wu, & Q.D. Wang (Dept of Materials Science and Engineering, Zhejiang Univ, Hangzhou, Zhejiang, China), "Diffusion Coefficients and Trapping Density of Deuterium in Annealed and As-Cast Palladium."

AUTHORS' ABSTRACT

The electrochemical method was employed to measure the apparent diffusivity of deuterium in palladium in the temperature range of 298-328 K. On the basis of theoretical analysis of the interaction of deuterium with traps in palladium and the present measurement, the lattice diffusivity, the trapping density of deuterium and the binding energy of deuterium with traps were determined. The lattice diffusivity of deuterium in well-annealed palladium in the temperature range studies can be described by an Arrhenius relation with a preexponential factor $D = 1.05 \times 10^{-2}$ sq cm per sec and an activation energy E = 24.2 kJ per mol. The deuterium

trapping density for as-cast and well-annealed palladium is 1.86 x $10^{18}$ per cu cm, 1.74 x $10^{18}$ per cu cm, respectively and the mean binding energy of deuterium with deuterium traps for the two cases in 24.75 kJ per mol, 24.74 kJ per mol, respectively.

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B. Hjorvarsson (Dept of Physics, Univ of Uppsala, Sweden), "Hydrogen Vibrations on Surface; meV Energy Shifts Studied by MeV Nuclear Resonance Reactions."

AUTHOR'S ABSTRACT

The vibrational motion of hydrogen on surfaces is a topic where a considerable disagreement between theory and experiments exists. An example of this is hydrogen on the Pt(111) surface, where the assignment of the vibrational modes can not be considered as settled [see Peter J. Feibelman & D.R. Mamann, *Surface Science*, **182** (1987) pp 411-422 and refs therein]. The most frequently applied methods for vibrational studies at surface, IRA and EELS, monitor the energy difference between vibrational levels, but the vibrational modes are not uniquely determined. An alternative route is to use the Doppler broadening in Narrow Nuclear Resonance Reactions (NNRR) to determine the angular dependence of the vibrational amplitude. By combining IRA (EELS) results and the information obtained by angle resolved NNRR, an unambiguous assignment of modes is obtained. In this work the formalism and possibilities of NNRR for vibrational studies is revealed and examples of vibrational determination are given.

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Derek Lewis (Dept of Inorganic Chem, Royal Inst of Tech, Stockholm, Sweden), "Simultaneous Thermal, Electrochemical and Radiation Events in Experiments on the Fleischmann-Pons Effect."

AUTHOR'S ABSTRACT

This paper describes new data on the novel phenomena discovered in 1989 by Fleischmann and Pons [*J. Electroanal. Chem.*, **261** (1989), 301 & err. **263**, (1989), 187] and by Jones et al. [*Nature*, **338** (1989), 737], and their colleagues, i.e., respectively, evolution of excess enthalpy and emission of neutrons during electrolysis of deuterium oxide at cathodes of metals that can form metallic hydrides. Careful reviews have been published recently (E. Storms, *Fusion Technol.*, **20** (1991), 433 and V.A. Tsarev & D.H. Worledge, ibid, 484], of the experiments on the Fleischmann-Pons Effect and the Jones Effect that had been reported up to the middle of 1991. They lead to the conclusion that the main body of evidence which has been reported substantiates the reality of the effects. The new data were obtained in a series of experiments at Stuidsvik, on the electrolysis of deuterium oxide at a palladium cathode. They contain marked regularities and coincidences in thermal, electrochemical and radiation phenomena during the course of the electrolysis. These show that the process yielding the excess enthalpy occurs at the cathode/electrolyte interface. It is also seen that this process is closely coupled with another process in the bulk of the cathode, which gives rise to sporadic, low-level emission of neutrons but no measurable heat. The Fleischmann-Pons Effect is temporarily quenched by external radiation much above background and by the internal radiation of the Jones Effect, perhaps through changes in the structure of the electrochemical double-layer caused by the hydrated electrons produced by radiolysis of the deuterium oxide.

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F.A. Lewis, S.G. McKee & R-A McNicholl (School of Chem, Queen's Univ, Belfast, N. Ireland), "Limits of hydrogen contents introduced by electrolysis into palladium and other transition metals."

AUTHORS' ABSTRACT

In the last few years there has been renewed interest estimations of upper limits of equivalent hydrogen pressures which correspond to hydrogen contents that can be introduced by electrolysis into palladium and palladium alloys. Earlier and recently somewhat neglected studies [L. Kandler, et al., *Z.Phys. Chem. Lpz.*, **180A**, 281 (1937) and J.C. Barton & F.A. Lewis, *Z.Phys. Chem. Neue Folge*, 33, 99 (1962)] of electrodes of palladium with *catalytically active surfaces*, have indicated upper limits of hydrogen pressure of the order of 100 bar (about 10& Pa.). This range of upper limit was complementary with general evolutions of hydrogen gas in the form of bubbles that increasingly supplement diffusional loss of dissolved hydrogen molecules through the Brunner-Nernst interfacial layer of solutions. In these earlier studies, the equivalent hydrogen pressures were derived from extrapolations back to times of interruption of electrolytic cathodisation of functions of pressures derived from subsequent measurements of open circuit potential. An alternative means of estimating equivalent hydrogen pressures has been to measure electrical resistivities of palladium alloys actually during electrolysis [see J.C. Barton, et al. ibid.]. This method can apply where values of such resistivities have been independently correlated with pressure by high pressure gas phase technique measurements. Estimates of pressure limits from resistivity measurements have provided a means of measurement essentially independent of surface catalytic activity. They have indicated values of effective upper limits of equivalent hydrogen pressures of about 5,000 bar in cases where surfaces are *catalytically inactive*. In the *catalytically active* surface case, the upper limit of about 100 bar actually provides an effective restriction on the range of pressure experimentally derivable by the techniques of conversion from electrode

potentials still currently utilized [R-A. McNicholl & F.S. Lewis, *J. Less-Common Met.*, 172-174, 160 (1991)] to determine p-n relationships of palladium alloys. Results from latter such studies [ibid] of palladium alloys with Ti, Zr, Nb and Sc effectively constitute additional confirmation of upper limits in the region of 100 bar for hydrogen-active surfaces.

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H. Uchida, Y. Hamada, Y. Matsumura, and T. Hayashi (Dept of Applied Physics, Faculty of Engineering, Tokai Univ, Kanagawa, Japan), "Effects of Current Density and Cold-Working of Pd Electrode on Radioactive Emissions in the Pulse-Modulated Deuteriding-Dedeuteriding Reactions."

AUTHORS' ABSTRACT

In previous reports [H. Uchida et al., *Proc. of 40th Int. Soc. Electrochem Meet.*, Kyoto, Japan, Sept. 22, 1989, Rept. 22-01-13-G and H. Uchida et al., *J. Less-Common Met.* 172-174, (1991) A40-41], we pointed out that the pulse-modulated deuteriding-dedeuteriding of Pd at current densities over 600 mA/sq cm is effective to generate excess radiation (gamma- and/or X-rays) compared with background (BG) or with those of the KOH cell. The excess radiation could be almost continuously measured using GM counters about in 20-30 min after the initiation of the electrolysis. This effect was not found in the boiling the $D_2O$ used or in the pulse-modulated hydriding-dehydriding of Pd in the KOH cell. This work reports the investigation of the effects of the density of pulse-modulated current and of the cold-work on the excess radiation. The main observations were:

(1) Pd electrode was cold worked up to 49.4% by rolling at room temperature. In the changes of current density from 350 to 4,000 mA per sq cm, no distinct difference in the excess radiation was found among the samples rolled by 0, 32.8, and 49.4 %. However, the excess radiations of the cold-worked samples yielded much higher GM counts and more frequent excess radiation during the electrolysis. The excess radiation could be measured for a while even after the electrolysis;

(2) The increase in the current density up to 4,000 mA per sq cm was effective to generate excess radiation with higher GM counts.

These measured effects were not originated from electric circuits or any external signals because the real-time BG measurements at distanced sites and electric noises were carefully controlled.

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R.N. Kuz'min, A.P. Kuprin, P.O. Revokatov (Lomonosov Moscow State Univ, Dept of Physics), "The Accumulation of Tritium in the Surface of Pd and Ti Deuterated Samples."

AUTHORS' ABSTRACT

The beta-spectra from the surface of the model Ti samples marked with tritium and from Ti and Pd samples deuterated in different techniques were obtained. It was found that in the latter case the quantity of tritium in the surface layer increased with time of aging of the samples. Both causes exhibited the non-stationary diffusion. The plate-type Ti and Pd samples were deuterated electrolytically in $D_2O$ and in $D_2$ gas. The typical activity of $D_2O$ before electrolysis was about 5 Kb/mL, and 30 Bk/mL after. The background was about 1-2 Bk/mL. The model samples were Ti plates, deuterated in $H_2SO_4$ marked with tritium with the typical activities 0.01 to 0.1 Ku/mL. It was found that some deuterated samples exhibit beta-activity with spectrum similar this one for beta-decay of tritium. The spectra from deuterated samples were registered during two years. The activity and the form of this spectra changed with sample aging. The changing of the spectra form reflects the non-stationary diffusion in the surface layers of samples. The time dependence of the activity S(t) consisted of interchanging parts with increase and decrease. The inclination of decreased parts of S(t) curve was similar for difference samples and equal to $(3.6 \pm 0.7)\%$ per month. This one for parts with increase exhibited no correlation between different samples. The typical extent of the growth of the activity was about 100% from initial value and for one Pd sample achieved the maximum - 700%. The model samples did not exhibit similar effects. It could be supposed, that the observed accumulation of tritium in Pd and Ti is the result of cold nuclear fusion in Pd and Ti substance.

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R.N. Kuz'min, A.P. Kuprin, P.O. Revokatov, (Lomonosov Moscow State Univ, Dept of Physics) and J. B. Skuratnik (L.J. Karpov Res Inst of Physics and Chem), "The Non-Stationary Diffusion of Tritium in the Surface Layers of Titanium."

AUTHORS' ABSTRACT

The method of investigation of diffusion in a surface layers of solids is suggested. The data, which exhibits the change of the depth profile of tritium content in the surface of titanium with sample aging are given. The plate-type Ti samples were deuterated electrolytically. The electrolyte was marked by tritium water and displayed the activity 0.01 to 0.1 Ku/mL. The deuteration time was varied from 5 to 5400 seconds. The beta-spectra were registered with the flat-type gas-flow proportional counter in a 2 pi-solid angle in a different time interval after deuteration. The maximum analyzed depth was about 1 micrometer. The background was more than two powers more than the typical activity of samples. The time dependence of the form and the activity of the registered

spectra was observed. The activity decreased due to the outflow of tritium from the sample. The change of the spectra form reflects the change of distribution of tritium within the thickness of the surface layer. For each spectrum the average energy $E_{av}$ was calculated as the characteristic of average depth of tritium disposition in surface layers. It was found that $E_{av}(t)$ dependencies displayed periodically form [sic] with period varying from 30 to 60 days for samples with different deuteration time. The value of $E_{av}$ varied in the limits of 4-6 KeV. This effect possibly could be explained by the interaction between the diffusion flow and the defects of sample surface.

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B. Escarpizo, J.F. Fernandez, F. Cuevas, J. Tornero, C. Sanchez (Dept Fisica Aplicada, Univ Autonoma, Madrid, Spain), "Anomalous X-Ray Diffraction in Electrolytically Deuterated Titanium."

Ti plates about 10 x 10 x 1 mm have been deuterated by electrolyzing heavy water ($D_2O$) with two Pt anodes and the Ti plates as cathode. Electrical currents between 0.2 to 2.0 A. have been passed through the cell for around 2,000 hours. After electrolysis deuterium concentration and distribution into the plates was investigated by using X-Ray diffraction. In parallel, by using R.B.S. (Rutherford Back scattering) it has been found that the deuterium concentration remains constant in the plates up to about 100 micrometer depth and then it goes to zero very abruptly. Results on the application of R.B.S. will be published elsewhere. X-Ray diffraction patterns of the deuterated plates have been obtained at different depths by progressively removing thin layers of the plates. It has been found that preferred $TiD_x$ crystallization parallel to (100) is produced and the $tiD_x$ structure largely depends on the original Ti plate texture. Results are discussed in terms of both the starting material and the deuteration process.

*Fusion Facts* thanks Dr. Glück for sending copies of the above abstracts. We look forward to reading and reviewing the complete papers for our readers.

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## C. CHUKANOV REPORTS ON BALL LIGHTNING SYMPOSIUM - JULY 28-30, 1992.

**By Dr. Kiril Chukanov**

*Fusion Facts* is an unique newsletter because it provides to the world fast, concise information on Enhanced Energy Devices. I am currently working in two branches of excess energy generation: ball lightning and cold fusion.

Recently, July 28-30, 1992, at UCLA, Los Angeles, California, the Third International Symposium on ball lightning was held. The first was held in Tokyo in 1988 and the second in Budapest in 1990. For the second symposium I sent an abstract of my work but this is the first symposium I have been able to attend. During the past year I have been in close communication with all the official investigators in the field of ball lightning. They are now aware of my claims that ball lightning is a new state of matter and a potential source of energy. During 1988 and 1991 I made two unsuccessful attempts to have this type of information about my work published in *Nature*.

The following is my report on the Third International Symposium on ball lightning:

**ORGANIZATION.** In terms of contact with the media, this symposium was the worst of the three. The first symposium in Tokyo was widely reported in both scientific and popular media (newspapers, magazines, radio and TV). For the third symposium only the Japanese TV were present to report on the conference. We even had some problems with the hired security guards but thanks to the efforts of R. Golka, I was able to demonstrate my work.

**PARTICIPATION.** Many of the papers were submitted by European investigators but due to current economic problems only one Russian scientist (E. Manykin from the famous Institute "Kurtchatov") participated. One of the members of the International Committee on Ball Lightning, Professor G. Egely (Hungary) was unable to attend. Chinese scientists had submitted several papers but one You-Suo Zou was able to attend. All papers from the absentee scientists were presented by their colleagues or by the president of the ball lightning committee (Prof. S. Singer, USA).

**HIGHLIGHTS OF PAPERS.**

OBSERVATIONS OF BALL LIGHTNING. G.C. Dijkhuis (Zeldenrust College, Netherlands); Erling Strand (Norway); A.Kh. Amirov, V.L. Bychkov, A.Yu. Stridjev (Inst for High Temperatures, Moscow); You-Suo Zou (Dept of Meteorology, U. of Hawaii); and A.G. Keul (Australia) presented eye-witness accounts and some photographs from their countries. These investigators have attempted statistical analysis of the dimensions and lifetimes of ball lightning. Of particular interest was the report by E. Strand. He showed pictures and a videotape of different observations of unknown lights in the sky that were interpreted as ball lightning. In a report presented by Prof. Y-H Ohtsuki (Waseda Univ, Japan) it was shown that about 90% of UFO sightings are natural ball lightning.

THEORETICAL WORK. There is nothing new under the sun. The theoretical models are mainly either chemical or plasma models. The most interesting chemical model is the cluster model of ball lightning presented by Prof. B. Smirnov (Russia). This model is supported by Prof. S. Singer. Most of the scientists, in this field, explain ball lightning as a special stable state of plasma, for example, Prof. P. Handel (Univ. of Missouri); J. Nachamkin (USA); E. Manykin (Russia); D.B. Muldrew (Canada), X.H. Zheng (Univ. of Cambridge, UK); and You-Suo Zou (China). Based on the contemporary quantum mechanics, these theoretical models try to explain the special stability of the ball lightning. However, ball lightning presents several unusual properties which cannot be explained by classical quantum mechanics and the theory of ordinary plasma. Obviously we need a new way to think about the science of ball lightning. [See Chukanov's paper. Ed.]

LABORATORY-MADE BALL LIGHTNING. According to the official data, only four groups in the world are now working on laboratory replication of natural ball lightning. These are K.L. Corum et al. (Batelle, Columbus, Ohio); Robert K. Golka (Brockton, Massachusetts); Yoshi-Hiko Ohtsuki (Waseda Univ., Japan); and Kiril B. Chukanov (U/U Research Park, Utah). The first three groups are trying to replicate natural ball lightning, especially to create relatively stable fireballs. Corum's group has successfully replicated one of the experiments made by N. Tesla in Colorado Springs. They create ball lightning (about 1 inch diameter) using high voltage RF discharges (2 MV, 450 KHz). The lifetime of these balls are several seconds. Unfortunately, K.L. Corum was unable to attend the conference. R. Golka stated that K.L. Corum refused to show him his device.

Robert Golka's experiment is the best laboratory experiment replicating natural ball lightning. Golka presented videotapes on two experiments. The first used a 150 KWh transformer to provide approximately 10,000 amperes at 15 volts. Using an under-water plastic tank (about 3' x 4' x 1') he shorts two electrodes (Cu and Al) under a half inch of water. The result is the formation of ball lightning in the water. Small-sized metal drops are probably created also. Golka's second experiment is more convincing. The ball lightning is created using powerful submarine batteries and shorting the voltage using aluminum and copper electrodes.

Very interesting experiments are performed by the Ohtsuki group. They use an experimental microwave apparatus (2.456 GHz, maximum power of 5KW.) In a cavity resonator they create small fireballs which can move against the airflow and last for 2 to 3 seconds. I asked Ohtsuki about energy measurements from the balls but he said no measurements had, as yet, been made of the energy produced by the balls.

CHUKANOV'S PRESENTATION. I presented a report including both theory and experimental data, pictures of ball lightning, and video tapes of my work both in Bulgaria (1987-1990) and in California (1990-1992) and a demonstration of my laboratory device. To me, ball lightning is a new (unknown until my work) state of matter which I call material continuum. Ball lightning can produce excess energy (more energy output than used to create the ball lightning). This excess energy is possible because the phenomena violates the Law of Energy Conservation. I have proven my theory with practical laboratory experiments. All of the unusual properties of ball lightning are explained by my theory. Our sun is a huge "ball lightning" also. My theory indicates that the main source of energy in the cosmos stems from these phenomena. The laboratory device shown at the symposium demonstrate different properties of ball lightning. Measurements show, without doubt, that excess energy is produced. My presentation made an impact on several of the government attendees. It is hoped that several national laboratories will replicate my work.

The authors and titles of the papers presented were as follows (listed alphabetically):

A.Kh. Amirov, V.L. Bychkov, A.Yu. Stridjev (Inst. for High Temperatures, Moscow), "Databank on the Ball Lightning Observations for PC."

Kiril Chukanov (U/U Research Park, Salt Lake City, Utah), "Ball Lightning - Energy Source of the XXI Century."

K.L. Corum, J.F. Corum & J.F.X. Daum (Batelle, Columbus, Ohio), "Recent Experiments with Ball Lightning and Directed Discharges."

G.C. Dijkhuis (Zeidenrust College and Convectron N.V., The Netherlands), "Statistics and Structure of Ball Lightning."

Robert K. Golka, Jr. (Brockton, Massachusetts), "Laboratory Produced Ball Lightning."

A.I. Grigor'ev, S.O. Shiryaeva, I.D. Grigor'eva (Yaroslavl Univ., Russia), "About the Possibility of Ball Lightning to Spontaneous Dividing in Two."

Hiroshi Kikuchi (College of Science & Technology, Tokyo, Japan), "Similarities and Differences Between Ball Lightning, Triggered Lightning, and Crop Circles Phenomena."

D.W. Muldrew (Ontario, Canada), "Small-Size Ball Lightning."

Yoshi-Hiko Ohtsuki (Waseda Univ, Japan) & Hideho Ofuruton (Tokyo Metropolitan College of Aeronautical Engineering, Japan), "Nature of Fireballs Produced by Microwave Interference."

Hans Rau and Betty Trafford (Aachen, Germany), "A Microwave plasma ball reactor -- experiment and simulation."

S.A. Sall (State Optical Inst. St. Petersburg, Russia), "Ball Lightning as a Converter of Environment Heat into Current and Radiation."

Leon A. Steinert (Physical Synergetics Inst., Long Beach, California), "On Theoretical Interpretation of Ball Lightning by Analogy with Exploding-Wire Phenomena."

Erling Strand (Norway), "Ball Lightning tracks, and latest news from Norway."

A.P. Verduta (Moscow), "Microwave Theory of Ball Lightning. Possibility of the Portable Self-sustained Fusion Reactor with Microwave Plasma Trapping." Second Title: "Observation of Low Frequency Subterrawatt Power Ion-sound Wave Vircator Action in REB-AIR Injection Experiments."

X.H. Zheng (Dept of Engr'g, Univ. of Cambridge, UK), "Lifetime of Ball Lightning."

You-Suo Zou (Dept. of Meteorology, Univ. of Hawaii at Manoa), "On the Formation of Fireballs in Lab and in Atmosphere."

## D. NEWS FROM THE UNITED STATES

### CALIFORNIA - *SCIENCE* REPORT
**Courtesy of Dr. Samuel P. Faile**

Constance Holden (Editor of Random Samples), "Fusion Explosion Mystery Solved," *Science*, Vol 257, No 50666, 3 July 1992, pg 27.

EDITOR'S COMMENTS

Except for two slightly derogatory asides: "particularly the die-hard fans of cold fusion," and "the small but dedicated cold fusion community," this report from *Science* is actually factual, informative, and unemotional. The article relates the report from SRI about the findings of the investigation into the death of Dr. Andy Riley. The article reports that the Electric Power Research Institute has agreed to let McKubre publish his team's cold fusion results in the open literature. The article ends with the statement that papers should be forthcoming from this SRI lab in the near future. Thanks *Science*, for this cold fusion report. We trust that we will see further unemotional reports on cold fusion in the future.

### IOWA - HYDRIDING MAGNETS
**Courtesy of Ron Smith**

K.A. Sschneidner, Jr. (Director, RIC), "Rosy Future Predicted for Bonded Magnets," *Rare-earth Information Center INSIGHT*, Vol 5, No 8, August 1, 1992.

EXCERPT

The following paragraph from the RIC Insight is quoted: "The Mitsubishi Materials Crop. process is called HDDR, which stands for hydrogenation, decomposition, desorption, and reaction. In the HDDR process the starting NdFeB material is initially hydrided (H) at low temperatures and during the heating to above 650 C it absorbs more hydrogen and decomposes (D) to form $NdH_2$ + alpha Fe + $Fe_2B$. Then this mixture is dehydrogenated (D) below 1000 C by pumping off the hydrogen to reduce $NdH_2$ to neodymium metal, which then reacts (R) with alpha Fe and $Fe_2B$ to form the anisotropic NdFeB powder. The trick is to keep the temperature below 1000 C in the last step, otherwise the powder will be isotropic."

### MASSACHUSETTS - NEUTRON TRANSFER THEORY

Peter L. Hagelstein (MIT), "Coherent and Semicoherent Neutron Transfer Reactions I: The Interaction Hamiltonian," *Fusion Technology*, Vol 22, No 1, Aug 1992, pp 172-180, 1 Figs, 18 Ref.

AUTHOR'S ABSTRACT

The interaction of Hamiltonian describing coherent neutron capture and neutron removal from nuclei in a lattice is presented. The explicit development of the interaction Hamiltonian leads to a new nonlinear phonon operator that enforces the physical constraint that the center-of-mass coordinates for the initial- and final-state nuclei must coincide to within fermis in order for a transfer to occur. An immediate application of the model to the Mossbauer limit of neutron capture shows increased phonon coupling relative to predictions from Lamb's theory.

AUTHOR'S DISCUSSION

We have presented an interaction Hamiltonian that describes the capture of a neutron, or the removal of a neutron, from a nucleus that is embedded in a lattice. Our treatment differs from previous models that describe

radiative neutron capture in a lattice, and from Mossbauer transitions, in that the constraint imposed by the requirement that the initial- and final state c.m. coordinates approximately coincide is included explicitly in the mode. It has been well known that an observable energy shift occurs in the Mossbauer line shape due to the difference in the initial and final lattices (the mass of the emitting nucleus changes by the gamma energy divided by $c^2$), and no one would doubt that an analogous energy shift must occur in the case of neutron capture; to date, no satisfactory model for the associated phonon emission has appeared in the literature.

In the case of nonresonant radiative neutron capture, the constraints that arises in arranging for an alignment between the initial and the final microscopic nuclear c.m. positions results in an associated phonon operator that is nonlinear in the phonon coordinates. The presence of this operator will not generally alter the line shape significantly from the line shape that would be predicted from Lamb's theory, which neglects neutron recoil effects completely. But under conditions where the photon and neutron momentum is matched, the new theory would predict significant and potentially observable off-resonant gamma emission.

EDITOR'S COMMENTS

We would like to suggest to Prof. Hagelstein the experimental evidence from L.P. Nielsen, et al. (Phys Rev B, 15 Dec 91, page 13156) in which they show an extraordinary alignment of H-induced reconstruction of Ni(110). This data coupled with D. Fischer, et al. (Phys Rev Lett, 4 May 92 pg 2786 - see Abstract this issue under ENGLAND) where they report that potassium adsorbs on top of Ni(111) gives an interesting picture of how a Ni surface might be structured to participate in nuclear reactions. We would like to ask Dr. Hagelstein if this experimental data, together with his enhanced Hamiltonians could help explain the nuclear processes causing transmutation of potassium (or rubidium) in light-water, potassium carbonate, nickel-cathode, electro-chemical, cold-fusion cells.

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## MASSACHUSETTS - H & Ni CATALYSIS
**Courtesy of Dr. Samuel P. Faile**

A.D. Johnson, S.P. Daley, A.L. Utz, & S.T. Ceyer (Dept of Chem, MIT), "The Chemistry of Bulk Hydrogen: Reaction of Hydrogen Embedded in Nickel with Adsorbed $CH_3$," *Science*, Vol 257, No. 5067, July 10, 1992, pp 223-225, 4 Figs, 18 Refs.

AUTHORS' ABSTRACT

Studies in heterogeneous catalysis have long speculated on or have provided indirect evidence for the role of hydrogen embedded in the catalyst bulk as a primary reactant. This report describes experiments carried out under single-collision conditions that document the distinctive reactivity of hydrogen embedded in the bulk of the metal catalyst. Specifically, the bulk H atom is shown to be the reactive species in the hydrogenation of $CH_3$ adsorbed on Ni(111) to form $CH_4$, while the H atoms bound to the surface were unreactive. These results unambiguously demonstrate the importance of bulk species to heterogeneous catalytic chemistry.

EDITOR'S COMMENTS

The authors conclude that they have documented a new mechanism for a surface reaction between an absorbed and an absorbed species. Their final comment is, "This mechanism may be operable in many catalytic hydrogenation reactions." ***Fusion Facts* suggests that this mechanism may be operable in catalyzing many nuclear reactions.** Remember that many cold fusion scientists have been saying that this new science of cold fusion will lead us to learn more about the structure and nature of matter than any recent discovery. Or as Sir Isaac Newton might comment, "On the shores of the sea of Truth, we have found another bright pebble to examine."

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## MINNESOTA - H EXCHANGE REACTIONS
**Courtesy of Odell Hobbs**

Stu Borman (Staff Writer), "New Technique Models Dynamics of Hydrogen Exchange Reaction," *Chem and Engr'g News*, Vol 70, No 31, August 3, 1992, pp 25-26.

EDITOR'S REVIEW

The lead-in to this article states: "Ability of theory to simulate experimental data for reaction between deuterium atoms, hydrogen molecules advances." This article is derived from an article in the July 24, 1992 issue of *Science* [257, 519 (1992)]. Scientists who were involved in the theoretical work came variously from Univ of Chicago (Daniel Neuhauser), Sandia Nat'l Labs (Richard S. Judson), Univ of Houston (Donald J. Kouri), and Soreq Nuclear Res Center in Yavne, Israel (Michael Baer). The experimental chemists are David E. Adelman, Neil E. Shafer, Dahv A.V. Kliner (now with Univ of Minn.) and Richard N. Zare of Stanford Univ. The new approach is based on time-dependent wave packet method. The experimental results showed remarkable agreement between theory and experiments at some energy levels and the need for theoretical refinement at other energy levels. The approach uses a new quantum mechanical method to

simulate the reaction dynamics between deuterium atoms and hydrogen molecules. Some of the theoretical considerations may have some bearing on the catalysis of nuclear reactions. The idea of the wave-packet allows one to take into account all of the energy variations, according to the article. Some of the cold fusion models may find some concepts in this work that would be transferrable to cold fusion models.

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## NEW MEXICO - TAKAHASHI REPLICATION

Rex Graham (Staff Writer), "Cold Fusion Experiment Yields Power - Los Alamos Researcher Duplicates Earlier Result," *Albuquerque Journal*, pg A1 & A4.

### EDITOR'S COMMENTS

This newspaper article reports on the experimental work at Los Alamos National Laboratory performed by Dr. Edmund Storms that partially reproduces the work by Japanese scientist Akito Takahashi of Osaka University. The author twists reality a bit with, "marking the first time a promising cold fusion experiment has been reproduced in a second laboratory." Later in the article Graham writes, "Pons and Fleischmann claimed to have discovered a cold fusion reaction that produced excess power, but labs around the world were never able to duplicate their results." After over three years reporting on the various replications of different cold fusion phenomena from over 25 countries, *Fusion Facts* finds this to be an interesting statement. This reporter quotes Dr. Storms saying, "I think this [experiment] proves beyond a shadow of a doubt they deserve a good deal more credit." In terms of commercialization, Storms is cited as expecting it will be awhile but will probably be commercialized in Japan where, "they are way ahead of us on this."

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## NEW MEXICO - REPLICATION PAPER
**Courtesy of Dr. Edmund Storms**

Edmund K. Storms (NMT Division, Los Alamos National Laboratory), "Measurements of Excess Heat From a Pons-Fleischmann Type Electrolytic Cell Using Palladium Sheet," submitted to *Fusion Technology*, also published as LAUR: 92:2390, dated July 2, 1992, 32 manuscript pages, 17 figs, 31 refs.

### AUTHOR'S ABSTRACT

Two pieces of palladium sheet similar to that used by A. Takahashi were loaded with deuterium in a Pons-Fleischmann-type electrolytic cell and measurements were made of heat production. One sheet produced a steady increase in excess power that reached 7.5 watts (20% of input power) before the study was interrupted. A second similar sheet from a different batch of palladium did not produce any measurable excess power. There was a difference between the loading behavior, the maximum stoichiometry, and the presence of excess volume in the deuteride made from these materials. The first sheet contained 0.8% excess volume after having been deloaded from its maximum D/Pd ratio of 0.82 to 0.73, and the second sheet contained 13.5% excess volume while at its maximum ratio of 0.75. The high excess volume in the latter case is an indication of internal cracks that reduce the required high D/Pd ratio.

### EDITOR'S COMMENTS

Dr. Storms has made a valuable contribution to cold fusion science in two ways: By replicating the work previously by A. Takahashi and by making a careful study of the difference between two samples of palladium plate (one producing excess heat and the other not). The measure of the unusual degree of excess volume of the second and non-working sample provides a better insight into some of the factors that may reduce the ability of some experimenters to have successful results. Storms notes, in his conclusion, the following, "...In any case, the highest concentration of deuterium must exist at the surface [of the cathode] . . . Consequently, reporting heat production on the basis of cathode volume not only conflicts with this expected behavior but also gives the false impression of a much higher heat-producing rate than was actually observed while using the usual small samples. Based on the thin film studies of Bush and Eagleton, the reaction is expected within 5 micro-meters of the surface and the power densities are much higher than those reported using the total cathode volume. The Pd was charged using about 0.13 amp until about 65 hours of loading. Storms then began to vary the current from 0.13 amps (30 minutes) to 2.77 amp (60 min.) After 90 hours of operation excess heat was measured. It is expected that this paper will be presented at the Nagoya Conference and also will be printed in *Fusion Technology* in a near-future issue. We commend Dr. Edmund Storms on his excellent paper.

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## NEW YORK - TAKAHASHI & MITI

Jerry E. Bishop & Jacob M. Schlesinger (Staff reporters), "U.S. Researcher Claims to Replicate Japanese Experiment in 'Cold Fusion'," *Wall Street Journal*, July 27, 1992, pg B3.

### EDITOR'S COMMENTS

This article reports on Dr. Edmund Storms replication of the Takahashi experiment in cold fusion using thin-plate Pd. In addition the writers report that B. Stanley Pons and Martin Fleischmann met with representatives of

several Japanese companies in a closed meeting. The result, as reported by one Japanese attendee was that Pons and Fleischmann presented, "some interesting data, convincing us to accept that cold fusion is taking place." Meanwhile, the article reports, the Japanese Ministry of International Trade and Industry (MITI) is considering funding "new hydrogen energy." The article also reports that Dr. Francesco Celani, in Italy, and Dr. Hideo Ikegami (National Inst. for Fusion Science, Nagoya) are also making progress in replicating Takahashi's work.

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**NEW YORK - CF MYSTERY IN JAPAN**

William D. Marbach (Developments to Watch Editor), "In Japan, Yet Another Cold-Fusion Mystery," *Business Week* (Science & Tech section), July 27, 1992, pg 83.

EDITOR'S COMMENTS

Marbach reports that MITI will request less than $2 million for cold fusion development. Insiders in Japan's cold fusion supporters, says the article, suggest that MITI is downplaying cold fusion so as not to rile the Japanese hot fusioneers.

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**NEW YORK - INTERNATIONAL HOT FUSION**
**Courtesy of Dr. Samuel P. Faile**

William J. Broad (Staff Writer), "Quest for Fusion Power Is Going International," *New York Times*, July 28, 1992, page B8.

EDITOR'S COMMENTS

William Broad reports that Japan, Russia, the U.S., and the European Community agreed to spend $1.2 billion on the International Thermonuclear Experimental Reactor (ITER). If built the experimental reactor would be about 10 stories tall and cost an estimated $6 billion. Broad mentions that hot fusion is so popular because the top two inches of Lake Erie contain sufficient quantities of deuterium that could produce more energy than all of the earth's know oil reserves. William J. Broad has yet to discover that cold fusion is real and can also use Lake Erie. We finally quit sending him copies of *Fusion Facts* after learning that he can only read hot fusion reports.

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**NEW YORK - GE REPORT ON CF**
**Courtesy of Carol White**

R.H. Wilson, J.W. Bray, P.G. Kosky, H.B. Vakil, & F.G. Will (GE Corp Research, Schenectady), "Analysis of Experiments on Calorimetry of LiOD/$D_2O$ Electrochemical Cells," to be published in *J. Electroanalytical Chem.*, 39 manuscript pages, 9 Figs, 9 Refs (none dating after 1990).

AUTHORS' ABSTRACT

In this paper we present a detailed analysis of calorimetry with heavy water electrolytic cells, especially of the type described by Fleischmann, Pons, et al. in recent publications. We also summarize our own experiments, which involve calorimetry of electrolytic cells of various designs. None of our experiments has yielded any excess heat or radiation products within the detection limits. We evaluate the data and methods of Fleischmann, Pons, eta al. and, where sufficient data are available, conclude that they significantly overestimate the excess heat. This is in part because they did not include in their calibration calculation the change in input electrochemical power to the cell resulting from the calibration heater power. An additional significant overestimate of excess energy occurs when the calibration is made at cell temperatures above about 60 C, due to the increased evaporation of heavy water during the calibration. Furthermore, we find unexplainable inconsistencies in the data on light water controls, as reported by Fleischmann and Pons. While our analysis shows their claims of continuous excess heat generation to be significantly overstated, we cannot prove that no excess heat has been generated in any experiments.

EDITOR'S COMMENTS

Those who have read the literature have learned a lot about cold fusion since this work was performed. Later, Dr. Will, former director of the National Cold Fusion Institute was able to produce tritium in nearly every experiment in the final days of the NCFI work. We have learned that *J. Electroanal Chem.* will also publish comments from Pons and Fleischmann about this paper.

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**NEW YORK - NOT COLD FUSION**
**Courtesy of Dr. Samuel Faile**

Maurice Goldhaver (Dept of Physics, Brookhaven Nat'l Lab, Upton), "Cold Fusion - Not Nuclear," *Science*, Vol 257, page 310, July 17, 1992, in letter to Editor.

EDITOR'S COMMENTS

Goldhaber criticizes Peter Hagelstein's theoretical work on cold fusion by saying that if neutrons emitted during cold fusion were absorbed by the palladium lattice that isotopes would be produced that would emit beta- and gamma-rays and that the intense gamma rays would have been noticed by researchers who looked for gamma-rays from cold fusion. Goldhaber writes, "...since neither such a radioactivity nor tritium, helium, or neutrons have been

found," cold fusion cannot be nuclear. Perhaps Goldhaber gets his latest information only from *Science*, *Nature*, and *The New York Times*. He could have talked with one of his own colleagues at Brookhaven who measured tritium in his cold fusion experiments but who also stated that "people seem to undergo personality transformations" when positive results about cold fusion are broached. (The quote from Dr. James McBreen.) If Goldhaber has problems with the Pons-Fleischmann type of cold fusion, he will really object to hydrogen/alkali-metal nuclear reactions!

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**NEW YORK - COPENHAGEN INTERPRETATION**
**Courtesy of Dr. Samuel P. Faile**

Don Reed, "Towards a Structural Model for the Fundamental Electrodynamic Fields of Nature," *Extraordinary Science*, Vol 4, Issue 2, Apr/May/Jun 1992, pg 28-33, 2 Figs, 22 Refs.

EDITOR'S COMMENTS

In this article Don Reed searches for some explanation for over-unity energy producing devices. He believes that progress will require that the Copenhagen Interpretation of quantum mechanics be revised. The article introduces the use of the "forgotten mathematics of biquarternion (screw) algebra, the linear complex, and the cylindroid." Reed suggests the following, "by imputing to the electron a field structure based upon phase-locked electromagnetic waves in quadrature, R. Jennison has not only mathematically shown an electrodynamic basis for its inertial properties and relativistic nature, but his work has borne out the fiction of the *static* Coulomb field." Reed also suggests that the work of Hestense, Puthoff, and Furutsu have shown that the Copenhagen Interpretation is untenable because the ZPF (zero-point vacuum fluctuations) is a real phenomenon which may provide a better basis for a more rigorous quantum mechanics. It may be appealing to some of our more mathematically-talented readers to determine if the production and control of "electron beads" (as taught us by Ken Shoulders) is consistent with the Copenhagen Interpretation of quantum mechanics. *Fusion Facts* would be pleased to print short discussions on this subject.

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**NEW YORK - BOOK REVIEW**

William C. Gough (Foundation for Mind-Being Research), Reviewing Too Hot to Handle: The Race for Cold Fusion, in *Fusion Technology*, Vol 22, No 1, Aug 1992, pp 188-191.

EDITOR'S COMMENTS

In the opinion of most cold fusion researchers, this book by Frank Close provides little of merit for those who know that cold fusion works and is a new science. However, the reviewer has made a cogent point in his closing paragraphs of the review: ". . .I encourage those working in cold fusion to have the courage to follow their convictions and inner guidance. I encourage those funding such research not to succumb to group pressure or concerns about ridicule but to carefully review the validity of data and provide the funding to seek answers to remaining questions. Finally, I encourage the scientific community to recognize that our current science is based on assumptions, some hidden, and that we should not feel the possibility that nature may give us evidence that could require a major change in those assumptions. Rather, we should all encourage this quest into the unknown." Well said!

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**UTAH - TRITIUM IN PALLADIUM**

Krystyna Cedzynska & Fritz G. Will (U/U NCFI), "Closed-System Analysis of Tritium in Palladium," *Fusion Technology*, Vol 22, No 1, Aug 1992, pp 156-159, 4 Figs, 1 Tables, 7 Ref.

AUTHORS' ABSTRACT

A closed-system procedure for the analysis of tritium in palladium has been developed that has a sensitivity and accuracy of $5 \times 10^7$ tritium atoms, corresponding to one tritium atom per $10^{13}$ palladium atoms for a typical 0.1-gram palladium sample. The technique involves palladium dissolution in acid, distillation of the tritiated water, and catalytic oxidation of tritium gas to tritiated water, followed by liquid scintillation counting. This technique is not subject to false tritium findings from a variety of chemical factors or environmental influences that may affect the results of open-system analytical procedures. The closed-system procedure has been applied to nearly 100 as-manufactured palladium wire samples of various lots and sizes from two difference sources. None of these samples show any tritium contamination within the detection limit of $5 \times 10^7$ tritium atoms. By comparison, others, employing an open-system procedure, have reported tritium contamination in as-manufactured palladium 10,000 times larger than the values obtained by this closed system method.

AUTHORS' CONCLUSIONS

We have developed a closed-system analytical technique for the analysis of tritium in palladium that features high reliability and accuracy. This technique employs a distillation and catalytic gas recombination procedure that

leads to the quantitative determination of the tritium content of palladium with an accuracy and sensitivity of 5 x $10^7$ tritium atoms. Application of this technique to 90 palladium wire samples of various lots and sizes, manufactured by two different suppliers, yields no tritium contamination in any of these samples. While our results do evidently not exclude the possibility of rare spot contamination of palladium with tritium, such contamination is exceedingly unlikely both in view of our results and the details of the palladium manufacturing process. The closed-system analytical procedure described here is not affected by the shortcomings of open-system techniques, such as falsely low or high tritium readings. Application of the closed-system procedure for tritium analysis of palladium prior to use in cold fusion studies is advisable to ascertain that there is no possibility for tritium contamination.

EDITOR'S COMMENTS

This work has previously been reported in the final report for the NCFI, and at the July 1991 Cold Fusion Conference in Como, Italy. The author's conclusions should be carefully explained to those who believe that palladium contaminated with tritium is responsible for some of the anomalous tritium measurements made after electrochemical operation of a Pd/Li/$D_2O$ system. The two palladium manufacturers whose palladium was tested were Hoover & Strong (66 samples) and Johnson Matthey (24 samples).

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**UTAH - CF CLAIMS CHALLENGED**
Chem Abstracts, July 27, 1992

Steven E. Jones (BYU, Provo), "Current issues in cold fusion research: heat, helium, tritium, and energetic particles," *Surf. Coat. Technol.* **1992**, Vol 51, No 1-3, pp 283-289, 18 Refs.

AUTHOR'S ABSTRACT

Claims of excess heat and helium production are compared with evidence of energetic nuclear particle emissions in deuterided solids. A solid quantitative basis for connecting the two effects is absent. In particular, notions of lattice heating without production of energetic debris are challenged.

Scott D. Tiffany (staff writer), "Y professor disputes Los Analmos experiment," *The Universe*, (BYU student newspaper), July 29, 1992.

EDITOR'S COMMENTS

Tiffany interviewed Dr. Steven Jones (well-known BYU professor and "father" of muon-catalyzed cold fusion) and quotes him as saying that "Edmund Storms' claim that he generated power by cold fusion without any signs of nuclear ash is not scientifically possible." We have read Dr. Storms excellent report and he does not claim "he generated power by cold fusion with no signs of nuclear ash." Further Jones is directly quoted, "When you have nuclear reactions, you have nuclear ash. If you don't have ash commensurating, then it isn't energy. That is a firm scientific principle." By a copy of this issue to Professor Jones, we are asking him to define the "nuclear ash" in the following nuclear reaction: $^2D + {}^6Li \rightarrow 2\ {}^4He$ + energy. From the many positive results that have been reported in peer-reviewed papers on excess heat from cold fusion experiments, it is apparent that $^4He$, lattice heating, X-rays, are the predominant nuclear by-products of Pd/Li/heavy- water cold fusion. In addition, there are some neutrons and a few million times as much tritium according to many reports.

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**PENNSYLVANIA - ION IMPLANTATION**

Moishe Garfinkle (Drexler Univ, Philadelphia), "Ion Implantation as a Definitive Means of Investigating any Possibility of Intracrystalline Nuclear Fusion," *Fusion Technology*, Vol 22, No 1, Aug 1992, pp 160-163, 1 Fig, 14 Ref.

AUTHOR'S ABSTRACT

The recently reported detection of helium (albeit minuscule) with equivalent heat production using an electrochemical process at the University of Texas and at the Naval Weapons Center at China Lake were both attributed to intracrystalline nuclear fusion, which again brings to the fore this most controversial of subjects. However unlikely this fusion process, it is contended that an electrochemical process is particularly unsuited to the task of substantiating intracrystalline nuclear fusion because simultaneous thermochemical processes are occurring that can overshadow possible modest nuclear processes. Moreover, the presence in the electrolyte of extraneous reagents such as dissolved oxygen and salts further complicates interpretation of results. In light of these observations, an investigative method utilizing deuteron implantation with concurrent spectrometric analysis of reaction products is proposed.

AUTHOR'S CONCLUSIONS

Essentially, ion implantation provides all of the conditions thought necessary for the detection of possible intracrystalline nuclear fusion: nonequilibrium concentrations of deuterium with an appropriate crystal structure. Moreover, because employing an ion beam source permits direct control of the beam composition, whether $^H$, $^2D$, or $^3T$, any reaction products can be directly

compared with reactants in real time, and the effect of target composition and metallurgy can be readily investigated. Compared to electrochemical means, ion implantation with concurrent spectrometric analysis can constitute a highly advantageous means of investigating the possibility of intracrystalline nuclear fusion and delineating between various possible processes by direct measurement of reaction products.

EDITOR'S COMMENTS

Garfinkle suggests that if nuclear reactions occur within a crystal lattice, the effect could be readily replicated by ion implantation of hydrogen, deuterium, or tritium. In his article Garfinkle lists four possible reactions involving hydrogen and deuterium but he fails to list the possible nuclear reaction of deuterium with lithium-6 to produce helium-4. If, as suggested by Robert T. Bush's new TRM, the nuclear reactions are hydrogen/alkali metals, then this ion implantation experiment might well provide another negative result which would be insufficient to prove a case against cold fusion. In light of the increasing number of experimenters who are replicating light water, nickel cathode electrochemical cells and measuring reasonable quantities (20% to 300%) of excess heat, it is suggested that ion implantation may not be the final answer to the reality of cold fusion.

## E. NEWS FROM ABROAD

### CANADA - NEUTRONS FROM Pd & Ti
From Chem Abstracts, June 29, 1992

J.S.C. McKee, G.R. Smith, J.J.G. Durocher, H.L. Johnston, M.S. Mathur, J.K. Mayer, A. Mirzai, Y.H. Yeo, A. Hempel, et al. (Univ Manitoba), "The role of fractofusion in the creation of anomalies in neutron production from deuterium-implanted solids," *Nucl. Instrum. Methods Phys. Res., Sect B*, 1992, Vol B67, No 1-4, pp 448-451, in English.

AUTHORS' ABSTRACT

Although most of the evidence for cold fusion has finally proved to be unsupportable [sic], repeated observations of short bursts of neutrons from deuterium-loaded Pd and Ti samples suggest that anomalies exist in the behavior of D-implanted solids that may require explanation. The Manitoba group has searched for anomalous emissions following the direct implantation of Pd, Ti, and In with deuterium, and have observed neutron emissions at a level which may exceed the expectations of standard warm fusion calculations. A specially designed analytical system is now in place to study the behavior of thin samples of Pd and Ti when implanted with unique $D_2$+ ions at a concentration of about $10^{25}$ per cu cm. The phenomenon of fractofusion may contribute to the cross-section for fusion in these processes. Microcracking in the alpha phase of Pd is generated by the beta-alpha transformation. However, until a definite contribution to the cross-section for neutron production, additional to that anticipated from standard nuclear reaction calculations is confirmed, the fractofusion process mentioned above cannot be quantified.

[In view of the first sentence in the abstract, one wonders what literature on cold fusion the authors have read lately. Ed.]

### ENGLAND - K ON Ni - LOLLIPOPS
**Courtesy of Dr. Peter Glück**

D. Fisher, S. Chandavarkar, I.R. Collins, R.D. Diehl, P. Kaukasoma, M. Lindroos, "Top Site Adsorption for Potassium on Nickel(111)," *Physical Rev Letters*, Vol 68, No 18, May 4, 1992, pp 2786-89.

AUTHORS' ABSTRACT

We have used dynamical low-energy diffraction (LEED) to determine the adsorption sites and the geometry of the surface region for the p(2x2) overlayer of potassium adsorbed on Ni(111). The structure consists of potassium atoms adsorbed on top of the Ni atoms with vertical reconstructions of Ni atoms in the first and second substrate layers combined with a slight horizontal reconstruction of the first substrate layer. The potassium-nickel bond length is found to be 2.82 $\pm$ 0.04 Angstroms corresponding to a rather short effective potassium "radius" of about 1.57 Angstroms.

EDITOR'S COMMENTS

Note the similarity with this and L.P. Nielsen, et al. paper (Phys Rev B, 15 Dec 91, page 13156), in which it was shown that the Ni atoms could be moved a considerable distance to create a row of Ni-H-Ni-H-Ni... atoms. Note that this paper also cites some reconstruction of the Ni atoms. As Dr. Peter Glück says, "And these surfaces, if fueled with hydrogen or deuterium are moving, working for us. I can see the deuterons desperately struggling for a few active sites." This paper, together with Nielsen's paper should certainly help the CF theoreticians and modelers augment their understanding of the geometry of nickel-catalyzed cold fusion. In a mental picture, one could visualize a row of K on Ni - like lollipops - awaiting an energetic hydrogen wavicle to resonate down the lollipop line and trigger a transmutation of K to Ca with a reverse-Mossbauer coupling of the resulting released energy into the crystal lattice. Apparently, the events also produce (at least sometimes) a soft X-ray gamma.

## FRANCE - AN OFFER FROM PONS

Associated Press, "Pons says it's too late for a deal with U," *Deseret News* (a Salt Lake City, Utah newspaper), July 24, 1992.

### EDITOR'S COMMENTS

In two articles in two days in Salt Lake City newspapers, it was reported that a group of investors desired to buy cold fusion patent rights from the University of Utah. In this second article, Dr. Pons was interviewed in Nice, France and reported that the original offer was made on behalf of the Japanese firm, Technova. This article reports that John Morris, legal counsel to the U/U, is continuing efforts to negotiate a licensing agreement to give Pons and Fleischmann development and marketing rights on any technology stemming from the former patent applications whose ownership Pons and Fleischmann share with the U/U. The article quotes Pons as saying, "There is no negotiation, all negotiations stopped (July 16)." Apparently, the offer from Pons and Technova was rejected on the basis that there was an end cap to the total amount that Technova was willing to pay. *Fusion Facts* has previously speculated that the value of the Pons/Fleischmann/U of U patents (if and when they issue from the U.S. patent office) could be worth many millions in revenue over the life of the patents.

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## FRANCE - Ag/Ni SUPERLATTICES

Courtesy of Dr. Samuel P. Faile

"B. Rodmacq (Centre d'Etudes Nucleaires de Grenoble), B. George, M. Vaezzadeh, & Ph Mangin (Univ de Nancy I), "Giant magnetoresistance in Ag/Ni superlattices," *Physical Review B*, Vol 46, No 2, pp 1206-1208, 3 figs, 23 refs.

### AUTHORS' ABSTRACT

A comparison of the evolution of magnetoresistance and magnetic coupling in the Ag/Ni superlattice system is presented as a function of the silver thickness, for a nickel thickness kept at 8 Angstroms. This system exhibits a magnetoresistance effect as large as 28% and gives rise to an ideal thickness of 10-12 Angstroms for antiferromagnetic coupling. No further clear oscillations are observed. Both the position and amplitude of the oscillations are in agreement with recent theoretical calculations.

[Maximum effect is observed using a Ni thickness of about 10 to 13 Angstroms. Ed.]

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## FRANCE - POLYCRYSTALLINE Ni/Ti

Courtesy of Dr. Samuel P. Faile

M. Bouhki, A. Bruson, & P. Guilmin (Univ of Nancy I), ""X-Ray Diffraction Study of Amorphization along Interfaces in Polycrystalline Ni/Ti Multilayers," *Solid State Communications*, Vol 83, No. 1, pp 5-9, July 1992, 5 Figs, 20 Refs, In English.

### AUTHORS' ABSTRACT

Ni/Ti multilayers with composition modulation wavelengths (35 to 110 Angstroms) were annealed at 473K during 8 hours and studied by X-ray diffraction. Upon annealing, an amorphous phase developed along the interfaces, concurrently with dissolution of Ti in crystalline Ni. The diffusion coefficient was determined, using methods described by Hollanders et al.

### AUTHORS' CONCLUSION

Annealing at 423 K of polycrystalline Ni/Ti multilayers lead to amorphization at the Ni/Ti interfaces. Simultaneously, Ti dissolves in crystalline Ni. This dissolution induces an increase of Ni lattice spacings and compressive stress leads to a decrease of the Ti spacing. Product phase formation along the Ni-Ti interfaces is exhibited by the decrease of the integrated intensities of the crystalline Ni and Ti reflections. The interdiffusion coefficients measured, at 473K, in the amorphous phase ($4.3 \times 10^{-23}$ sq m per sec for composition modulation wavelengths of 110 Angstroms and $4.4 \times 10^{-23}$ sq m per sec for 80 Angstroms) are in good agreement with that measured at 523 K by Hollanders et al.

[It may be of interest to test this multilayer Ni/Ti material in heavy-water electrolysis. Ed.]

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## INDIA - PROGRESS AT BARC

Courtesy of the author.

R. Sambasivan (Editor), "Low energy emission from palladium loaded with hydrogen or deuterium," *J Sci Ind Research*, Vol 51, Feb 1992, pp 128-129, in English.

### EDITOR'S COMMENTS

Sambasivan included a copy of this article with a request for *Fusion Facts*. The article cites the BARC (Bhabha Atomic Research Centre) work as follows: "At a time when scientists in advanced countries are disillusioned with cold fusion experiments (even funding agencies like NSF have drastically cut their allocations for such projects in USA) it is heartening to find a revival of interests through the systematic investigations conducted by scientists at Bhabha Atomic Research Centre (BARC),

Bombay, under Prof. M. Srinivasan." The article reports on some of the gas-loading work and the measurements of byproducts of nuclear reactions. Some of the results of the plasma focus work at BARC are summarized. In addition, the article discusses some of the radiography used by BARC scientists to track suspected nuclear reactions. Sambasivan notes that BARC scientists are viewing an alternative to cold fusion as explaining observed results - $H_2/D_2$ - plasma focus and Pd-lattice interactions. We will be pleased to include Sambasivan in our mailings of *Fusion Facts*.

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**INDIA - HYDROGEN STORAGE**
**Courtesy of Dr. Samuel P. Faile**

P. Mandal, K. Dutta, K. Ramarkrishna, O.N. Srivastava (Dept of Physics, Banaras Hindu Univ) & K. Sapru (Technology Innovation Products, Troy, Michigan), "Synthesis, characterization and hydrogenation behavior of Mg-xwt.% FeTi(Mn) and $La_2Mg_{17}$-xwt % $LaNi_5$ - new hydrogen storage composite alloys," *J of Alloys and Compounds*, Vol 184, No 1, June 5, 1992, pp 1-9, 6 Figs, 15 Refs.

AUTHORS' ABSTRACT

New hydrogen storage materials with higher capacity and better suited for applications have been successfully synthesized. The hydriding behavior of the new composite materials, Mg-xwt.%FeTi(Mn) and $La_2Mg_{17}$-xwt.%$LaNi_5$, were studied for various values of x (x + 10, 20, 30, 40, and 50). The Mg-xwt.%FeTi(Mn) materials were activated under a hydrogen atmosphere (about 33 kgf per sq cm.) and an optimum storage capacity of about 3.5 Wt% corresponding to room temperature hydrogenation was established for x = 40. This high storage capacity - almost double the storage capacity of the well-known FeTi(Mn) - has been observed under ambient conditions. The $La_2Mg_{17}$-xwt.%$LaNi_5$ materials were activated at higher temperatures (about 360 C) in a hydrogen atmosphere. An optimum storage capacity of 4 wt.% in terms of pressure and composition was observed for $La_2Mg_{17}$-xwt.%$LaNi_5$ at 350 C. In comparison with the native ingredient $La_2Mg_{17}$, much faster (nearly three times) kinetics were found. In order to understand the hydrogenation behavior and the high storage capacity, structural-microstructural and chemical analyses of these composite materials were carried out. From the structural investigations it has been found that all the synthesized materials are multiphase. The composite material Mg-FeTi(Mn) was found to contain $FeTi_{1-x}$, magnesium, titanium and Ti-Mg phases. The higher storage capacity (about 3.5 wt.%) in the case of Mg-40%FeTi(Mn) is probably due to FeTi-Mg complexes. The hydrogen molecule is split at the FeTi surface and diffuses into the magnesium matrix via FeTi. In the case of $La_2Mg_{17}$-xwt.%$LaNi_5$, the composite material consists of $La_2Mg_{17}$, $MgNi_2$, nickel and $LaNi_3$ phases. Because of the presence of nickel and nickel-containing phases (e.g. Mg-Ni), it is assumed that the dissociation of hydrogen is easier and hence the system $La_2Mg_{17}$-xwt.%$LaNi_5$ has better kinetics than its counterpart $La_2Mt_{17}$ alone.

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**ITALY - MOSSBAUER & STIFFNESS**
**Courtesy of Dr. Samuel P. Faile**

T. Bressani (Univ of Torino), E. Del Giudice (INFN, Milano), & G. Preparata (Univ of Milano), "What Makes a Crystal Stiff Enough for the Mossbauer Effect?" *Il Nuovo Cimento*, Note Brevi, Vol 14D, No 3, March 1992, pp 345-349, 13 Refs.

AUTHORS' SUMMARY

The Mossbauer effect is well interpreted and understood in an ideal, infinitely stiff (mathematical) lattice. However we argue that the electrostatic forces that are usually considered in solid-state physics are not adequate to support such a stiffness. This difficulty is seen to be surmounted by a superradiant behavior of the plasma of nuclei of a crystal.

AUTHORS' CONCLUSIONS

We conclude by stressing that the mysterious nature of the Mossbauer effect, that engenders a strong violation of asymptotic freedom in a crystal, has been resolved by assuming that the plasma of nuclei undergoes a "superradiant" dynamical evolution. We believe that this is a further piece of the jig-saw puzzle of coherent electromagnetism in condensed matter that goes into place.

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**ITALY - PROBLEMS WITH QCD**
**Courtesy of Dr. Samuel P. Faile**

L. Gamberale, G. Preparata and She-Sheng Xue (Univ of Milano), "Failure of Perturbative Scaling in SU(3) Lattice Gauge Theory," *Il Nuovo Cimento*, Vol 105 A, No 3, March 1992, pp 309-314, 2 Figs, 11 Refs, In English.

AUTHORS' SUMMARY

The asymptotic freedom (AF) prediction for the energy density of the pure SU(3) Yang-Mills theory in a constant Abelian chromomagnetic field is clearly seen to be inconsistent with a lattice calculation. This failure of AF fully confirms previous results obtained for SU(2), both analytically and by lattice methods. In view of this important confirmation, we conclude that the generally

accepted picture of QCD cannot be maintained in its present form.

AUTHORS' CONCLUSIONS

We end this paper with the follow observations: 1) the AF prediction for the energy density in the presence of an external Abelian chromomagnetic field is clearly seen to fail also for the physically relevant SU(3) case; 2) the terms of the energy density up to $O[(gB)^2]$ have been determined (equation 15). The large values of $a_3$ above imply, as in the SU(2) case, that LGT (lattice gauge theory) calculations of nonperturbative aspects of QCD (quantum color dynamics) in realistic lattices are, in principle, unreliable. We thus conclude that we have now ample and strong evidence that the generally accepted picture of QCD, both in the short-distance (PQCD) and in the long-distance approximations (LGT), does not realize the deep dynamical implications of the QCD Lagrangian. In view of this, the possible resurrection of such picture of color dynamics must necessarily involve the thorough, scientifically objective falsification of the results that have been corroborated by the present analysis.

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## ITALY - H & D DIFFUSIVITY

**Courtesy of Dr. Samuel P. Faile**

B. Coluzz, C. Costa, A. Biscarini & F.M. Mazzolai (Dept of Physics, Univ of Perugia), "Diffusivity of H and D in palladium-platinum alloys via Gorsky relaxation," *J. Phys. Condens. Matter*, Vol 4, (1992), pp 5155-5166, 2 Tables, 8 Figs, 42 Refs, in English.

AUTHOR'S ABSTRACT

The macroscopic diffusion coefficient of H and D has been measured using Gorsky relaxation in the temperature range 225-350 K for H (D) atomic ratios n = 0.03 and n = 0.29, where n = H(D)/(Pd + Pt). The data are analyzed in terms of a diffusion model originally proposed by Griessen and co-workers, which has been adapted to apply to FCC alloys. The site energy calculations show that in the $Pd_{85}Pt_{15}$ alloy only $I_6$ and, to a minor extent, $I_5$ sites are occupied and contribute to diffusion in the temperature range investigated. A match of Gorsky and Zener relaxation data indicates the existence of an upwards deviation in the diffusion coefficient when plotted again 1/T. This deviation cannot be accounted for by classical diffusion models.

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## ITALY - SUPERCONDUCTOR NEUTRONS

Francesco Celani, Antonio Spallone, & Lorella Liberatori (INFN-Lab, Frascati) Fausto Croce & Lucio Storelli (Univ La Sapienze, Rome), Stefano Fortunati & Mario Tului (CSM-ILVA, Rome), & Nicola Sparvieri (Alenia Corp, Rome), "Search for Enhancement of Neutron Emission from Neutron-irradiated, Deuterided, High-Temperature Superconductors in a very Low Background Environment," *Fusion Technology*, Vol 22, No 1, Aug 1992, pp 181-186, 4 Figs, 16 Ref.

AUTHORS' ABSTRACT

Following experiments performed with deuterided high-temperature superconductors (HTSCs) at the underground Gran Sasso Laboratory, the capacity of these materials to absorb deuterium and the role played by nonequilibrium conditions in neutron burst emissions in the framework of cold fusion have been determined. Taking into account that HTSC materials such as $Y_1Ba_2Cu_3O_{7-delta}$ (YBCO) are able to absorb deuterium without destroying the crystalline structure, deuterated YBCO pellets were placed in a neutron radiation field, and thermal cycles were operated. In this double nonequilibrium condition, neutron rate enhancement was sought by selecting "time-correlated" burst-like events. The pellets and high-pressure $D_2$ gas were enclosed in a stainless steel vessel, and thermal cycles (300 to 77 to 300 K) were performed; moreover, for comparison, background and blank runs were performed. A specific acquisition system, able to detect multiple neutron signals in defined time windows, was set up. One thermal cycle run showed a large increase (seven times more, corresponding to >30 standard deviations) of time-correlated events with respect to the blanks. In another run, although no relevant mean value increase in events was detected, one interesting multiple (triple) neutron signal occurred at a temperature (about 95 K) close to the transition from superconducting to the normal state. These multiple events were sporadic (detected twice during four thermal cycles lasting about 3 hours), although the probability that these events were simulated by the background was quite low (one incident expected in 80 h). Similar runs produced no relevant values. Another experiment, at constant temperature (300K), characterized by a heavy $D_2$ gas refill, showed both some increase in time-correlated events and a few triple neutron signals.

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## JAPAN - C.F. GRAVITY DECAYS

Takaaki Matsumoto (Hokkaido University, Sapporo), "Observation of Gravity Decays of Multiple-Neutron Nuclei During Cold Fusion," *Fusion Technology*, Vol 22, No 1, Aug 1992, pp 164-171, 9 Figs, 10 References to other Matsumoto papers.

AUTHOR'S ABSTRACT

The Nattoh model predicted that multiple-neutron nuclei such as quad-neutrons can be produced during cold fusion,

and the gravity decays of the quad-neutrons were recorded on nuclear emulsions. Several different traces that might be produced by the gravity decays of di-neutron and multiple-neutron nuclei have been successfully observed. The mechanisms of the production of multiple-neutron nuclei are also discussed.

AUTHOR'S INTRODUCTION

According to the Nattoh model for cold fusion, a new light particle, the iton, is emitted during the hydrogen-catalyzed fusion reaction. As resulted, strange products of neutron nuclei such as di-neutrons and quad-neutrons should be left, as follows:

$2D + 2e + D \rightarrow {}^4n + i_2 + d$, or
$\rightarrow {}^4H + i_2 + {}^2n$

for double iton emission. [The symbol $i_2$ is the "double iton."] A previous technical note reported that micro-explosions caused by the gravity decay of quad-neutrons were successfully observed on nuclear emulsions. Several different traces that might be produced by gravity decays of di-neutron and multiple-neutron nuclei have been successfully observed. The mechanisms of the production of the multiple-neutron nuclei are discussed.

EDITOR'S COMMENTS

*Fusion Facts* has followed with great interest the experimental work done by Prof. Matsumoto since his first article in *Fusion Technology*, Vol 16, December 1989, page 532, "'Nattoh' Model for Cold Fusion." Judging from the scarcity of similar work, using nuclear emulsions (MA-7B, Fuji Film Co.), we wonder if any other experimental groups are making similar observations. Of course, we realize that the first step is to get a cell that "works" before one can measure the emission of various types of particles. In his article, Matsumoto shows 19 photos in 8 figures that have a variety of "tracks." He groups the type of traces into six groups depending on magnitude and structure. The photographs and type classification system are impressive. He claims that no such traces were found in the control emulsions, therefore, the traces must come from the byproducts of cold fusion. Under the heading, "Mechanisms of the formation of nuclei with many neutrons," Matsumoto proposes some nuclear exchange reactions that are suggested as being involved in the production of multiple-neutron events. Our readers are encouraged to study this work and to see if some of this work can be replicated in other than Pd cells.

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**JAPAN - H IN Pd ALLOYS**
**Courtesy of Dr. Samuel P. Faile**

Y. Sakamoto, F.L. Chen, M. Furudawa & M. Noguchi (Nagasaki Univ.), ""Permeability and diffusivity of hydrogen in palladium-rich Pd-Y(Gd)-Ag ternary alloys," *J. of Alloys and Compounds*, Vol 185, No 2, July 13, 1992, pp 191-205, 13 figs, 30 Refs.

AUTHORS' ABSTRACT

The permeabilities and diffusivities of hydrogen in $Pd_{100-X-Y}(Y(Gd))_XAg_Y$ alloys, where y = 0, 5, 10, 15, 20, and 24 at.% Ag and X (at%) is the Y(Gd) content such that the condition Y + 3X = 24 is satisfied, as possible diffusion membranes for hydrogen purification were measured at temperatures between 523 K and 673 K and input hydrogen pressures between 267 kPa and 667 kPa. From the permeabilities and diffusivities, the solubility constants in the alloys were calculated. The alloys $Pd_{95-X}(Y(Gd))_XAg_5$ with X = 6.3 were the most permeable to hydrogen, and the permeability values were found to be about a factor of 2-2.5 higher than that in $Pd_{24}AG_{76}$ alloy. The permeabilities are associated with the lattice expansion of the initial hydrogen-free alloys. Judging from the hardness measurements of the initial alloys, $Pd_{100-X-Y}(Y(Gd))_XAg_Y$ with the compositions where y is about 15-20 at.% Ag, and x is about 1.3-3.0 at.% Y(Gd), seem to be better materials for hydrogen diffusion membranes than Pd-(23-25) at.% Ag alloys.

[The diffusivity measurements showed that some of the ternary alloys were higher than the $Pd_{70}Ag_{30}$ alloy but less than or about equal to pure Pd. Solubility of hydrogen in the ternary alloys was higher than in Pd or PdAg alloys over the range 250 to 400 C that are reported in the article. Ed.]

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**ROMANIA - NUCLEAR CATALYSIS**
**Courtesy of the author.**

**Peter Glück (Inst. of Isotopic & Molecular Tech., P.O. Box 700, 3400 Cluj-Napoca 5, Romania), "The SURFDYN** Concept: An Attempt to Solve the Puzzles of Nuclear Cold Fusion," Submitted to *Fusion Technology*, 10 manuscript pages, 44 refs.

AUTHOR'S ABSTRACT

The lack of reproducibility of the cold fusion experiments, aggravated by the great diversity and inconsistency of the positive results, imply that these nuclear phenomena are hypersensitive, i.e. correlated to a "chaotic" factor. All the factors considered so far as structure, transformations, defects of the crystal lattice or the bubbles of deuterium, the dendrites, etc., are insufficiently chaotic to explain the known facts. Experimental data suggest that the nuclear reactions take place in active sites on the surface of the lattice, are stimulated by dynamic factors and represent an extreme form of heterogeneous catalysis. Consequently, according to the modern ideas regarding catalysis, the

wanted chaotic factor is the surface dynamics of some metallic deuterides (hydrides). This hypothesis, named the **SURFDYN** concept, is compatible with all the published data, explains the peculiarities of cold fusion and has to be supported by an adequate theory describing the nature and mechanisms of the different nuclear processes.

EDITOR'S COMMENTS

*Fusion Facts* has suggested that the work by L.P. Nielsen, et al. (Phys Rev B, 15 Dec 91, page 13156) in which they show an extraordinary alignment of H-induced reconstruction of Ni(110) could be important in a theory of Ni-catalyzed nuclear reactions. In addition, the paper by D. Fischer, et al. (Phys Rev Lett, 4 May 92 pg 2786 - 2789 -- see abstract page 14 under ENGLAND) adds some further understanding to the geometry of Ni and K atoms. It is believed that this information - of how K attaches to Ni atoms - will be helpful in constructing a more complete model for the catalysis of nuclear reactions. In terms of the "active sites on the surface of the lattice" as mentioned in the above abstract, Dr. Glück sent us an abstract [L. Stauffer & H. Ezzehar and H. Dreysse, "Stability of Hydrogen Clusters Near a Transition Metal Surface" (See page 3, this issue). This paper should provide further insight into H/Pd and H/Ni systems. As pointed out by Glück in his paper, there are many suggestions in the literature that cold fusion takes place on "active sites." For example, the BARC report that only 4 of about 1,000 Ti chips after gas loading showed sufficient "activity" to be measured by autoradiography. Cravens has reported on "spots" of heat developed on a Pd cathode. We do not thoroughly understand chemical catalysis. The idea that nuclear reactions could be catalyzed must be a concept that would be immediately dismissed by many scientists. If further experimental and theoretical work support the concept that nuclear reactions can be catalyzed on a Ni or Pd surface, then we will have made another step toward understanding cold fusion.

**Readers comments on the pros and cons of Dr. Glück's SURFDYN concept is seriously sought by both the author and this editor.** Please, readers, share with us some experiments that can be performed to support or disprove the idea that nuclear reactions between alkali-metals and hydrogen can be catalyzed on a Ni lattice.

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## RUSSIA - C.F. CONTRIBUTIONS

V.A. Tsarev (Lebedev Physical Inst of the Academy of Sciences, Moscow) & D.H. Worledge (EPRI, Palo Alto, California), "Cold Fusion Studies in the USSR," *Fusion Technology*, Vol 22, No 1, Aug 1992, pp 138-155, 20 Figs, 3 Tables, 59 Ref.

AUTHORS' ABSTRACT

Work presented at the First Soviet National Conference on Cold Nuclear Fusion, which took place in March 1991 in Dubna and Moscow, is reviewed. In addition to an integrated view of the experimental and theoretical work, a description is given of some additional contributions that had appeared in the Soviet literature.

EDITOR'S COMMENTS

This article is similar to the presentation made at the Second Annual Conference on Cold Fusion held at Como, Italy in July 1991. This article summarizes over 50 papers, mostly in Russian, that report on work prior to and after the announcement of electrochemical cold fusion at the University of Utah by Pons and Fleischmann (Mar 1989.) The paper treats the following subjects: Nuclear Mechanofusion (first experiments in 1986); Nuclear Chemofusion; Ion Implantation; Correlation Measurements; Gas Discharge Devices; Neutrons from dt reactions; Neutrons during $PdD_x$ Self-Heating; Surface Beta Measurements; Gammas during electrolysis; Cold Fusion Stimulation; Dubna Group Results; Cold Fusion Models; and a list of USSR Institutes active in cold fusion research. Three tables summarize both positive and negative results under Summary of Neutrons and Gamma Measurements (29 positive, 6 not positive); Charged Particles (4 positive, 3 not positive); and Tritium Production (7 positive, 3 not positive). Of the institutes listed 5 are in or near Moscow and others in Novosibirsk, Sverdlovsk;, Arzamas, and Lugansk. No mention is made of continuation of R&D under the new political regime.

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## UNITED KINGDOM - EXCITATION OF $D_2$

**Courtesy of Dr. Samuel P. Faile**

G.R. Darling & S. Holloway (Univ of Liverpool), "Translation-to-vibrational excitation in the dissociative adsorption of $D_2$," *J. of Chem Physics*, Vol 97, No 1, July 1, 1992, 2 Figs, 12 Refs.

AUTHORS' CONCLUSIONS

In conclusion, exploring the dynamics of a 2-dimensional potential energy surface, we have shown that the parameters controlling the degree of vibrational excitation and dissociation are not the same. The relationship between the molecule's turning point and the curvature of the potential energy surface (PES) is crucial in deciding whether or not the molecule emerges vibrationally excited. This implies that the **location** of the activation barrier is decisive. Conversely, it is the **height** and **width** of the barrier that determines the form of the dissociation probability.

## F. SHORT ARTICLES FROM READERS

### COLD FUSION: WHAT'S THE PROBLEM?
**By Mark Hugo**

I commend the following to FF readers: Isaac Asimov, As Far as the Human Eye Could See, Pinnacle, 1987. On pages 163-5 Asimov relates the story of a German chemist, Willstatter, and an American, Summer. Both were investigating enzymes. Willstatter spent six years developing a purification process for enzymes, ran tests, and came up with negative results. Willstater received the 1915 Nobel Prize for Chemistry and had **experimental conclusions** that enzymes were not proteins. Summer, on the other hand, believed enzymes were proteins. Azimov relates:

*What was wrong with Willstatter? He was a first class chemist, and did not make foolish mistakes. Actually, he didn't. He ended up with an enzyme solution showing strong activity, and with very little impurity. That solution, however, contained so few enzyme molecules (very few are needed) that even the most sensitive protein test at Willstatter's disposal had failed to detect anything, because the amount of enzyme was below the detectability limit for the tests. His work was meticulous and his conclusions reasonable--but his is an example of the **unreliability of a negative result!**"*

So what do we have from the critics: "Cold Fusion is not replicatable on demand." There have been numerous failed CF experiments. Why? Let's look at failures from a "random" factor viewpoint. Consider the following elements of a **successful** cold fusion experiments:

1. Use of over pressure of 100 psi $D_2$ gas for a couple days of charging (ala EPRI).
2. Charging current of about 0.060 amp per sq cm for a few days.
3. Cathode surface is best if it appears shiny and polished.
4. Gettering impurities in $D_2O$ using separate electrodes prior to loading.
5. Raising cathode current to over 0.500 amp per sq cm.
6. Vary the current to "trigger" events.
7. Keep electrolyte at low temperatures to prevent boiling at local spots on cathode (which would disrupt the current density).
8. Palladium used must not swell excessively (ala Storms and Cravens). [Excessive swelling would indicate internal fractures which would limit loading.]

Treating the above factors as ON or OFF and with the condition that all 8 are necessary for cold fusion, then how many combinations are possible to achieve negative results? The answer, of course, is 256. Consider the work by Harwell Labs (*Nature*, 1989). Yes, they spent $500,000. Yes, they ran numerous tests. Did they have all of the above knowledge of experimental factors? No. Does their "negative" results prove anything? No!.

Now, consider more "enlightened" experiments. We all know about Takahashi's work, yet the current state of knowledge is that although his original cell is still producing, he has not had any "luck" with two or three new cells. That brings us to the recent report of Dr. Edmund Storms. [See under **NEW MEXICO** in this issue.] Storms ran two different "Takahashi" cathodes. One produced a clean strong 20% excess power (7.5 watts on top of 35 input) on the high power cycle. The other cathode produced nothing. What's the difference? According to Ed, the successful cathode swelled only 1% more than theoretically predicted for a 0.85 D/Pd ratio. This cathode produced excess heat. The other cathode swelled to 14% more. It was a bomb. Why? Here we need to start looking VERY CLOSELY at the concept that all Pd pieces of 99.9% Pd are created EQUAL. They are not! Consider the processing route. Pd is melted and cast into an ingot. This ingot will have the following properties: It will have a grain structure lined up in a particular way, depending upon the cooling temperature gradients. Now, what is the next step towards getting a Pd sheet of 1 mm thick? The ingot is cold worked. Unless the producer deliberately does something unique, the Pd is probably rolled in one direction. Depending which direction the Pd is rolled, in relation to the original grain structure, you will have more or less strain on each grain. You will have different grain boundary and dislocation interactions. All of these factors will probably influence the "charge-ability" of the Pd piece, and whether it charges ideally or not. Thus, one of the secrets of future success will be to either: A. Process Pd ourselves, or B. Find out from the supplier the exact sequence of processing, and demand the same sequence be repeated after a successful cathode sample is found!

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### NEW SOLUTIONS OF MAXWELL'S EQUATION
**Courtesy of Dr. Robert W. Bass**

In a recent discussion about theories that may help to understand the EV (Electrum Validum "strong charge") or "electron beads" (taught to us by Ken Shoulder's patents) and how millions of electrons can stay together and be controlled and directed as a single entity, Dr. Bass mentioned his solutions of Maxwell's equations. In two papers, presented and/or published in 1979 and 1985, Dr. Bass shows that the Maxwell's equations have solutions other than for transverse waves. Here are the two titles and abstracts:

"Self-Sustained Non-Hertzian Longitudinal Wave Oscillations as Rigorous Solutions of Maxwell's Equations for Electromagnetic Radiation," June 15, 1979.

ABSTRACT: In contradiction to common belief, Maxwell's classical theory of electromagnetic radiation does predict the existence of longitudinal electromagnetic waves in vacuo. This note contains what appears to be the first rigorous proof of the theoretical existence of such non-Hertzian self-sustained longitudinal wave oscillations. The E-fields and B-fields in such waves are typically in infinite cylindrical or toroidal configurations and are, together with the vector potential, everywhere parallel. Accordingly, the Poynting vector vanishes identically and no energy is transported along the waves, though it does appear possible in theory to transport force at a distance without attenuation via such waves in vacuo.

"On Toroidal Non-Hertzian (Longitudinal) Electromagnetic Radiation Waves as an Element of Bostick's Self-Confined Plasmoids," October 31, 1985.

ABSTRACT: Bostick's self-confined plasmoids are observed experimentally to contain many internal filaments containing high-intensity magnetostatic fields in toroidal *"force-free"* configurations. It appears not to have been noticed hitherto that such fields need not be static. In fact, appropriate boundary-condition oscillations of such a field will convert it into a hitherto undiscovered *non-Hertzian* (longitudinal) type of self-sustained electromagnetic oscillation which *in vacuo* would be dissipationless and persist indefinitely, perhaps thereby providing the magnetic field which could confine such a low-density plasma. At sufficiently high plasma densities for the plasma to exhibit finite resistivity, such a condition would lead to the rapid *oscillatory* ohmic decay of the self-sustained electromagnetic field derived herein, which could be tested for experimentally in a Bostick-type of self-confined plasmoid of sufficiently high density.

Dr. Bass would be willing to collaborate with someone who is highly skilled in quantum mechanics with the goal of seeking to extend the above work in order to create a mathematical description of the "electron bead" (Kenneth Shoulders, U.S. Patent No. 5,018,180). It is believed that each electron bead and the "necklace" of several beads are both toroidal and highly energetic. It would appear that a non-Hertzian solution to Maxwell's equations may form a basis for describing a stable toroid of electrons in rapid motion that might model the observed electron beads. Hal Fox.

Copies of the above two papers are available from Dr. Robert W. Bass, Inventek Enterprises Inc., 817 Dominik Drive, College Station, TX 77840.

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NEW PATENT APPLICATIONS
**Courtesy of Dr. Peter Glück**

See *Fusion Facts*, Dec 1991, Feb 1992 & April 1992 for first 72 patents. The entries are made in the following order: List number; Patent application No.; Title; Applicant; Date of publication; Priority Date; Abstract.

73. JP 90 285,283; "Method for Nuclear Fusion"; Bridgestone Corp; 22 Nov 1990; 26 Apr 1989; To bring about a nuclear fusion reaction by electrolysis of heavy water, a $H_2$-storing alloy comprising more than 2 elements is used as the cathode. Typical alloys include La ($LaNi_5$, $LaCo_5$, etc.), Fe alloys (FeTi, $Fe_{1-x}Be_xTi$, etc.), Cr alloys ($Cr_{1.5}Ti$), Mg alloys ($Mg_2Cu$, $Mg_2Ni$, etc.), Cu alloys ($Cu_xTi_{1-x}$), Mm (misch-metal) alloys ($MmNi_5$, $MmNi_{5-x}Al_x$, etc.), Ti alloys ($TiCo_xX$, etc.), and alloys ($Ni_5Ca$, etc.). An alloy with a higher $H_2$-storing capacity is preferably used. Solvent heavy water greater than 90% is desirable. An electrolyte to be added to the solvent includes DCl, $DNO_3$, $D_2SO_4$, $DClO_4$, LiCl, $LiNO_3$, NaCl, $NaNO_3$, etc. Preferred electrode potential is 2-20 volts and desirable electric current is 40 mA/sq. cm.to 1 A/sq.cm..

74. JP 91 78,691; "Power generation by cold nuclear fusion"; Hitachi; 03 Apr 1991; 23 Aug 1989; Thermal energy is generated by implanting D in a substance (e.g., Pd) to cause cold nuclear fusion, and the thermal energy is converted into electrical power by thermo-electric means.

75. JP 91 215,785; "Thermal energy generators based on cold nuclear fusion"; Canon; 20 Sept 1991; 19 Jan 1990; A thermal-energy generator based on cold nuclear fusion, contains; (1) a container of D gas: (2) a pari of electrodes, at least one of which is formed of a H-storing metal; (3) a means to apply voltage on the electrodes to cause electrical discharge in the presence of D gas between them; (4) a thermal conductor to transfer heat generated at the electrodes to the coolant; and (5) a converter, to heat, of the kinetic energy of neutrons generated by cold nuclear fusion on the H-storing metal.

76. JP 91 215,786; "Apparatus for cold nuclear fusion using solar energy"; Canon; 20 Sept 1991; 19 Jan 1990; The apparatus contains (1) a solar energy-based electrical generator; (2) a means to generate D by electrolysis of heavy $H_2O$ using electricity from the generator; (3) a means to adsorb D using a metal; (4) a means to contain D generated by (2); (5) a cold fusion device in (4), which comprises a pair of discharge electrodes, at least one of which is made of the H-adsorbing metal; and (b) a device to apply voltage to the electrodes to cause electrical discharge.

77. JP 91 226,694; "Cold nuclear fusion based on electrochemistry in ultrasound field"; Semiconductor

Energy Laboratory Co. (SEL); 07 Oct 1991; 01 Feb 1990; In cold nuclear fusion, in which electrical energy is applied between a pair of electrodes immersed in a heavy $H_2O$-filled tank to cause reaction between the electrodes surface and D, the whole reaction system is placed in an ultrasound field. The method can improve the efficiency of cold nuclear fusion.

78. JP 91 237,397; "Neutron radiographic apparatus based on cold fusion nuclear fusion; Nissin High Voltage Co.; 23 Oct 1991; 14 Feb 1990; A neutron radiography apparatus, which contains a neutron source, and a film on which neutrons from the source is projected across a sample target uses a cold nuclear fusion device as a neutron source.

79. JP 91 237,398; "Radioactivation analyzer using cold nuclear fusion"; Nissin High Voltage Co.; 23 Oct 1991; 14 Feb 1990; A radioactive analyzer which is equipped with a neutron source and a radiation detector is characterized in that the neutron source is a cold nuclear fusion apparatus.

80. WO 91 14,267; "Method and apparatus for nuclear fusion"; Khudenko Boris; 19 Sept 1991; 13 Mar 1990; The present invention relates to a method and apparatus for cold nuclear fusion in which fusionable particles located within an electrolyte are accelerated by local electromagnetic fields in a migrational transport layer. This migrational transport layer can be induced either by creating a cementation system, applying an outside source of current to an electrode system, or a combination of both.

81. WO 91 15,071; "Deuterium energy accumulation"; Drexler, Jerome; 03 Oct 1991; 23 Mar 1990; Method and apparatus are described for promoting electrolyte ionization of heavy water to thereby produce D ions that are accelerated by an electrical field and collected in the interior of an accumulator. Negative and positive electrodes spaced apart, are immersed in the liquid with an approximate constant voltage impressed between them. An ion accumulator substantially surrounds the negative electrode, is formed of an accumulator material through which the ions may flow and has a metal that readily absorbs D at its surface. The accumulator material can absorb a fraction of the D ions that would otherwise flow to the negative electrode. D ions, absorbed into the accumulator material, may produce heat energy by cold fusion therein.

82. WO 91,396; "Deuterium accumulator for energy conversion; Drexler, Jerome; 28 Nov 1991; 17 May 1990; Method and apparatus are described promoting electrolyte ionization of high purity heavy water (containing LiOD) thereby producing D and Li ions that are accelerated by an alternating voltage. These are swept through a matrix of suspended D-absorbing and Li-absorbing particulates and collected in the interior of said particulates. The electrodes are spaced apart and immersed in the liquid with an alternating voltage between them. The matrix of suspended particulates is located between the two electrodes. When the D and Li ions pass through the particle matrix, a fraction of the ions strike the particulates and are absorbed into them. The D and Li ions which are absorbed in the particulates may fuse or otherwise combine to produce heat energy.

83. WO 91 18,397; "Deuterium accumulation energy conversion apparatus"; Drexler, Jerome; 28 Nov 1991; 12 May 1990; "Method and apparatus are described for promoting $^6$LIOD electrolytic ionization of heavy water to produce d and lithons that are accelerated by a.c. voltage and swept back and forth through a D and lithon-permeable and absorptive accumulator and collected in the interior of the accumulator. Two electrically-insulated electrodes are spaced apart and immersed in the liquid with an a.c. voltage impressed between them. The accumulator is positioned between the two electrodes and forms a structure through which the ions may flow and which consists of a material that readily absorbs the D and $^6$Li ions. The accumulator material can absorb a fraction of the d and lithons that would otherwise flow toward the instantaneous negative voltage electrode. The instantaneous negative electrode is electrically insulated from the d and lithons, which cannot pick up a free electron. Then, the d and lithons are not converted to unwanted D atoms and gas. Deuterons and lithons, absorbed into the accumulator may fuse or otherwise combine to produce heat energy.

84. WO 91 19,294; "Distributed deuterium lithium energy apparatus"; Drexler, Jerome; 22 Dec 1991; 25 May 1990; A method and apparatus are described for production of thermal energy through electrolyte ionization of $D_2O$ using a $^6$LiOD electrolyte, with dissolved D gas in the heavy water, and pumping the ionized heavy water over a bed of Pd metal particulates, foils, or porous baffles to collect both D and $^6$Li ions to facilitate ion-ion combination. No electrodes are used to achieve the fusion process. The container is a closed system such as a loop or helix to permit continuous cycling of the ionized heavy water over the Pd ion collector again and again to absorb the maximum number of ions and reuse the kinetic energy of the pumped water flow and the thermal energy added to the heavy water. Porous or perforated baffles are used to contain the Pd accumulator structure when it is in the form of particulates or loose components. Perforated baffles made of Pd may also be used as the accumulator structure.

85. DD (former East Germany) 293,147; "Arrangement for cold fusion in electrochemical fusion cell"; Technische Universitaet Dresden; 22 Aug 1991; 17 May 1989; The

title arrangement comprises a D-containing electrochemical fusion cell with a D oxidizing anode. The evolved gases from the cathode are fed into the anode where D & O recombine to form $D_2O$. This arrangement reduces the loss of heavy water during electrolytic fusion of D.

86. DD 293,148; "Arrangement for cold fusion in electrochemical cell"; Technische Universitaet Dresden; 22 Aug 1991; 12 May 1989; The title arrangement comprises a D-containing electrochemical fusion cell and a D-O fuel cell. The evolved gases from the fusion are fed into the fuel cell where D and O recombines to form $D_2O$. The thus-produced $D_2O$ is fed back to the fusion cell so that there is no loss of $D_2O$ during electrolytic fusion of D.

87. DD 295,939; "Material combination for electrochemically or chemically induced nuclear fusion and method of its preparation"; Humboldt Universitaet zu Berlin; 16 Nov 1991; 10 May 1991; The title method comprises the use of a metal alloy or inter-metallic compound in combination with a Li isotope for absorption of H or its isotopes. The combination provides improved efficiency.

88. EP 461,690; "Cold nuclear fusion thermal generator"; Boeing Co; 18 Dec 1991; 13 Jun 1990; An apparatus for conducting cold fusion comprises an electrical conductive anode,an electric conductive cathode comprised of a constituent for selectively absorbing H and releasing larger nuclei, and B or Li, a vessel for containing the electrodes, an electrolyte; means to vent gaseous reaction products and means to carry away the heat generated by the fusion. A method of producing heat energy comprises the steps of immersing an anode and a cathode in an ionic Ag solution in an electrolytic cell; applying an electric current across the electrodes such that $H^+$ ions are produced and H nuclei are adsorbed by the cathode fusing the adsorbed H with B in the cathode; and withdrawing heat from the cell.

89. FR 2,655,465; "Energy Source"; Shell Internationale Research Maatrchap, B.V.; 07 Jun 1991, 01 Dec 1989; A method and apparatus for production of energy by nuclear fusion is described compromising filling a body with ions or radicals of the isotopes of H, forming at least in a part of the body a metal hydride type lattice, using the above body as a conductor of a capacitance system inside an electrical circuit; the other conductor being connected to a source of electricity; and recovering then energy produced inside the body when electric voltage is applied.

90. BE 1,002,781; "Energy production by nuclear fusion"; Van Den Bogaert, Joannes; 04 Jun 1991; 05 Jun 1989; In this process, in which a fusible material is absorbed in the crystal lattice of H-absorbing material that has a negative electrical polarity, the fusible material is, or is being, absorbed by a H-absorbing material in the form of individual particles having a negative electrostatic charge, after which the polarity of the particles is changed from negative to positive. This process is especially aimed at the control fusion of D, optionally mixed with T, in the crystal lattice of the H-absorbing material at high efficiency. The H-absorbing material is a metal or alloy consisting of, or containing one or more elements selected from Pd, Ti, Zr, V, Th, Mo, Ta, Ni, and Fe. A turbulent aerosol or suspension of colloidal or crystal Pd particles (average particle size 0.1 to 0.001 micrometers) in D is supplied in an upflow through a vertical quartz tube internally coated with an electrically conductive coating or metal foil, e.g. Al or Cu, connected to the negative electrode of a d.c. source. A cooled positive charged plate (anode) is located above the tube, the polarity of the particles containing the absorbed D is changed upon contact with the anode and the positive ions, e.g. triton formed by nuclear fusion are then expelled from the Pd particles. The ions then flow downwards, are neutralized at the cathode in the conical bottom of the reactor, and the Pd particles are then separated from the aerosol in, e.g., a hydrocyclone. The Pd particles may be electrically charged in an insulating oil, e.g., a silicone oil. The heat generated by the fusion is removed by the heat transfer medium with which the anode is cooled.

[Thanks Dr. Glück for sending us these patents.]

## G. LETTERS TO THE EDITOR

### LETTER FROM FAMOUS EDITOR

To: Editor, *Fusion Facts*

I was certainly pleased and felt honored to receive the **Integrity Medal** award from *Fusion Facts* readers. All along I have felt that it was the responsibility of a professional journal editor to remain as impartial as possible and allow authors to publish articles that could get through the review process. This seems like a simple policy to me, but unfortunately I find, in the case of cold fusion articles, editors of other professional journals have not followed these same rules. As a result, I am in the puzzling position of receiving an award for doing what I think is simply my job. At any rate, I am certainly pleased to have obtained this medal and most certainly cherish it.

/s/ George H. Miley, Editor, *Fusion Technology*

LETTER TO READERS
From Hal Fox, Editor-in-Chief.

*Fusion Technology*, A Journal of the American Nuclear Society, has for three years published numerous papers about cold fusion. This publication effort has been performed consistently in the presence of a somewhat skeptical treatment of cold fusion. Those who comprise the Editorial Advisory Board of *Fusion Technology* should be congratulated on their decision to publish cold fusion articles. If you agree with me that *Fusion Technology* has performed an outstanding service to the advancement of science, please write a letter to **The Editorial Advisory Board, c/o of Dr. George H. Miley, Editor, *Fusion Technology*, University of Illinois, Fusion Studies Laboratory, 103 S. Goodwin Ave., Urbana, IL 61801.** Members of the board are: Charles, Baker, Hans Conrads, Donald L. Cook, Donald, J. Dudziak, Charles A. Flanagan, Ehud Greenspan, William Hogan, Robert A. Krakowski, Qui Li Jian, James F. Lyon, Hiromu Momota, L. John Perkins, Douglass Post, Kenneth Schultz, Weston Stacey, and Masaji Yoshikawa. These science heroes receive plenty of adverse comments from cold fusion skeptics. Please send them your commendations. They deserve it.

/s/ Hal Fox

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TWO LETTERS FROM ROMANIA

Dear Hal,

. . .I hope you have received my report from Debrecen and the patents for FF. [See page 21 this issue.] I am sending you copies of the abstracts of some of the conference and poster papers presented at the Uppsala Symposium, two of my colleagues attended. Five of the papers appear of direct interest for cold fusion; the others are just interesting. . . I have requested the full texts from the authors.

A German proverb say, "Der Teufel spricht immer von seinem Schanze." The devil speaks always about his intentions (Schanze). [And the Fox about his tail (Schwanze)? joke.] For me this has been, during the last few weeks, myself speaking about the SURFDYN concept. [A concept that helps explain much of the variability found in CF experiments and includes the idea of catalysis of nuclear reactions.] For example, I have much enjoyed reading in FF (June '92) the Otis Port letter about Fleischmann's lament regarding good and bad electrodes and reproducibility -- it [Fleischmann's complaint] is all so convenient for my hypothesis and the explanation is so easy! Fleischmann's "very large program" can be much smaller if we can use the skills and experience of the catalysts people. We have merely to understand that the "Blue bird is not in the cage, it is on the cage" (poor old Maeterlinck!).

Today, for example, I have found in *Chemical Engineering News*, 70(23) June 8, 1992, some calls for papers for the American Chemical Society's National and Divisional Meetings, information of interest and listing people to be contacted concerning this problem. Unfortunately, I am stuck here so I need the help of the US resident SURFDYN adepts, if any. Here are the places and contacts:

DIVISION OF COLLOID & SURFACE CHEMISTRY
Denver, March 28 to April 2, 1993.

Surface Science of Catalysis: Kinetics of Elementary Steps on Surfaces.
Contacts: R.F. Hicks, Dept of Chem Engr'g/ 5531 Boelter Hall, U of Calif, Los Angeles, CA 90024-1592, phone (213)206-6865; or F. Zaera, Dept of Chem, U of Calif, Riverside, CA 92521, phone (714) 787-5498.

Molecular Processes at Solid Surfaces
Contacts: J.R. Engstrom, School of Chem Engr'g, Cornell U, Ithaca, NY 14852, phone (607) 255-9934; or J.C. Hemminger, Dept of Chem, U of Calif, Irvine, CA 92717, phone (714) 856-6020

San Diego, March 13-18, 1994
Surface Science of Catalysis
Contacts: C.B. Mullins, Dept of Chem Engr'g, Univ of Texas, Austin, TX 78712-1061, phone (512) 471-5817 or A.B. Anton, Chem Engr'g School, Cornell U., Ithaca, NY 14853-5201, phone (607) 255-3629.

It would be very useful to know what are the up-to-date and future up-to-date ideas in re chemical catalysis -- thus we could better understand what are the chances of nuclear catalysis . . . I am now quoting somebody who has brilliantly predicted: "The emergent theories of cold fusion will coalesce into a few candidate theories and some excellent experiments will be designed to prove/disprove these theories. The final proven theory or theories will be surprising to almost everyone." Arthur Kostler. I believe he had SURFDYN in mind! True? Ockham's Razor is O.K., the hypothesis is very simple. We shall see. [Time will tell.]

As regarding the 10 best papers on Cold Fusion, my criteria is the "contribution to thinking correctly in the cold fusion field." I know it is late, our Post is guilty, but the following papers are the winners in my opinion. ... (peer-reviewed papers.)

1. The Bockris paper regarding eight chemical explanations of the F-P effect (Int'l J. of Hydrogen Energy, 14, 11, 771-5, 1989) as well as all the other

Bockris papers. Prof. Bockris teaches us how to be young and creative, he is our great hero. [Bockris please note. See following letter from Bockris. Ed.]
2. The Kaushik paper, "The Big Four (Chips)," as well as all the other Srinivasan BARC papers, showing us the essence, the very soul of the phenomena.
3. The Belsner, et al. paper, i.e. Prof. Huggins' school - the paper which makes the skeptics to tremble.
4. The Miles, et al. paper, a marvelous paper of paramount importance describing a very difficult battle won for cold fusion.
5, 6, 7. The Srinivasan, Storms, and Tsarev's reviews which all contributed to the global and holographic vision of cold fusion, so different, but so equally impressively clever.
8. The Fleischmann-Pons great calorimetric paper; genius, creativity, intelligence, but order, too.
9. The Bush & Bush-Eagleton papers, both theoretical and experimental.
10. The Liaw, et al. papers - we learn cold fusion from Hawaii's volcano's paper.

[Peter lists an abstract of an important paper, "Top Site Adsorption for Potassium on Nickel(111)." . . . And these surfaces, if fueled with hydrogen or deuterium are moving, working for us. I can see the deuterons desperately struggling for a few active sites. [See page 14.]

With all my best wishes to you and for the 3+ year-old child of yours!

/s/ Peter Glück

Just before press time we received the following from Peter Glück (dated July 31, 1992):

I am really very proud to be the proud owner of the "Best Foreign Correspondent Award"! I am doing my best to contribute to the progress in the field of cold fusion . . . I have sent the attached paper [his paper on SURFDYN model for cold fusion] to *Fusion Technology* [we checked and it is in the peer-review process]. . .and to a Congress in Poland (i.e., IV Congress on Theoretical Approach to Catalysis at Interfaces, Krakow) and to a scientist named Zdenik Slanina, who has a cold fusion publication [*Thermochemica Acta*, 156, (1989), pp 285-290.] Dr. Slanina has called my attention to a book, Quantum Chemistry Approaches to Chemisorption and Heterogeneous Catalysis, Editor F. Ruette, Kluwer Academic Publishers, Dordrecht, 1992. Perhaps this book could be useful for finding an answer to the questions: Does nuclear catalysis exist? Can the catalytic effects be so strong? What quantum effect makes it possible?

Can *Fusion Facts* make an inquiry with the great U.S. catalyst specialists? [Readers, we need your help. Who are the predominant U.S. catalyst specialists? Please respond.]

I am waiting both for help from theorists and for critics, even if destructive. . . . I have mentioned to you that I intend to write to the American Chemical Society asking this organization to analyze again its attitude toward cold fusion. As regarding DOE's attitude, a proper quotation is: **Bureaucracies never respond to LOGIC, only to PRESSURE.** (George Lundberg) . . .

Yours faithfully, /s/ Peter Glück.

[Thank you Peter, your letters, ideas, information, and quotations are always a delight to receive. Ed.]

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## LETTER FROM JOHN BOCKRIS

Hal Fox "goofed" and admits it.

Dear Hal,

I was reading *Fusion Facts* the other day and sorry to see you didn't give us anything in your list of awards!

We are rather proud of the priority we have in cold fusion work. We started two days after the Fleischmann and Pons announcement and by May 6th we were measuring tritium. There is a publication if *Journal of Electroanalytical Chemistry*, **1989**, Vol 270, pp 451-458, which I have sent you and this records this date on page 453.

Now the priority we claim, (which you award to BARC) is the first to have made the famous ratio of $10^{-9}$ - $10^{-8}$ for the ratio of neutrons to tritium, i.e. the branching ratio.

Glancing through the paper that we published I see that we register on page 456, 2.1 x $10^{6}$ for the activity of tritium in DPM per ml. But I don't see the neutron activity quoted there. However, our priority is claimed on the basis of the fact that at the Santa Fe meeting which was May, 1989 - well before the BARC publication, - we did announce the ratio as $10^{-8}$ as we saw it at that time. This is referred to on page 458 just before the acknowledgement section. The actual value is not stated there but it arose in the following way: We have the results of Kevin Wolf who was observing 1-3 neutrons per sq cm per second. Our results worked out between $10^{8}$ and $10^{9}$ tritiums per second per square centimeter. And so we were able at Santa Fe to point out the branching ratio so very different from that which normally obtains.

Historically speaking, I think we were the first institution in the United States, after that of Fleischmann and Pons, to come out with a valid announcement of confirmation.

There was an announcement by a certain Charles Martin here before the Santa Fe conference but he then withdrew the claim later on and said that it was false. So I think we do get it [the recognition that Hal Fox omitted and apologizes for.]

Anyway, no hard feeling about your statement but I do want to make clear that we do believe that our Santa Fe announcement of the ratio was the first time this very vital piece of information was brought out.

Sincerely, /s/ J. O'M. Bockris

**An apology from Hal Fox:** *Fusion Facts* has often cited Bockris et al., at Texas A&M as being the first after Pons and Fleischmann to produce cold fusion in a laboratory. As you can see from reading the nominations from Peter Glück, he had more wits than I did. Dr. Bockris: With this apologize, *Fusion Facts* hereby, belatedly, present you with the **FIRST REPLICATORS AWARD. As Peter states, you are one of our heroes. For the sake of historical accuracy, we are pleased to be informed about the branching ratio. I had forgotten that the unexpected branching ratio came up at the Santa Fe conference.** Thanks for your help in keeping an accurate report on these important items.

Note: Dr. Bockris also shared with us a letter which he sent to *Nature* in answer to the ridiculous comments made in *Nature* about the absence of tritium measurements at Texas A&M. See *Nature*, Vol 357, 25 June 1992, page 635, "Out, out brief candle..." and our comments about this article in *Fusion Facts*, July 1992, page 10. If *Nature* refuses to print Professor Bockris' correction to their misleading article, *Fusion Facts* will, with Bockris' permission, share his letter with you.

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## LETTER FROM CHINA

Dear Mr. Fox,

Thank you for your recent issue of *Fusion Facts*, which conveys the important message about the MITI investment. . . . I am still trying to recruit volunteers on my campus for the experimental study of light-water "excess heat." . . . We are interested in Takahashi's experiment also. We are expected more messages about the duplication of Takahashi's experiment in *Fusion Facts*.

/s/ Li, Xing Zhong

[We are always pleased to exchange news with our friends abroad. Prof. Li also pointed out the collating error in our July issue. The correction is being mailed to everyone with this issue. We apologize for our error. Ed.]

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## LETTER FROM MINNESOTA

**Wanted: Better Texts for Oldsters**

In trying to both do experiments, and "educate the stone" (this writer's head) I have been reading "Quantum Mechanics" and "Solid State Physics" texts the last three years. I find that there is a marked lack of QM texts which work out examples in their entirety in the text. I did find a wonderful QM text by W. Greiner. It is titled Theoretical Physics 1, and Dr. Greiner **does** go into great detail in working out the equations. I'm looking for a **similar** text in Solid State Physics. I'm familiar with Kittle and Ashcroft and with Mermin, neither of which are very involved when it comes to working out examples in the text. ("The Proof is left as an exercise for the reader...," is the classic comment made throughout Kittle and Ashcroft's texts. I don't need that, I'm too old for that nonsense. If someone has done the work already--I'm not going to re-invent the wheel.") Last: does anyone have any papers/info on the so-called "muon-catalyzed" cold fusion? I'm curious if anyone knows the lifetime of a "muon" in a solid state matrix. As I understand it, muons in "free space" reactions can catalyze several hundred fusions before being "consumed." Perhaps if their lifetime is orders of magnitude longer in the solid state, and if fusion reactions themselves can liberate muons, our "cold fusion" might be a natural extension of that phenomenon. But I speak out of ignorance, not knowledge, and would appreciate help in this realm.

Yours, /s/ Mark D. Hugo

Note to Mark: Jones and Rafelski did some of the original work in muon-catalyzed cold fusion and there has been a periodic conference on the subject for several years. In a recent summary publication [Rafelski et al., "Cold fusion: muon-catalyzed fusion," *Jrnl of Physics B: Atomic, Molecular and Optical Physics*, Vol 4, 1991, pp 1469-1516, 115 refs.] the authors present a topical overview of muon-catalyzed fusion. In the concluding remarks is the following sentence: "However, as highlighted in this survey, it is generally believed that MuCF is already close to the limits posed by a combination of practical approaches with fundamental laws of physics, and hence further improvement is exceedingly difficult." The last two sentences of this massive "topical review" states the following: "While the chances of success are probably remote, the furor generated by the reports of *cold fusion* show the general appreciation for the importance of unconventional directions in the search for clean fusion energy. It is from this perspective that we must look upon *cold, muon-catalyzed fusion.*" This paper was reviewed in the June 1991 issue of *Fusion Facts*. Ed.

## H. REEXAMINATION OF A KEY COLD FUSION EXPERIMENT: "PHASE-II" CALORIMETRY BY THE MIT PLASMA FUSION CENTER

By Dr. Mitchell R. Swartz, January 14, 1992, revised July 16, 1992. (c) M. Swartz 1992 JET Technology, Weston, Ma. 02193

EDITOR'S NOTE: *Fusion Facts* was asked to publish this information after it had been peer-reviewed. Although this article is longer than we normally publish, it covers an important aspect of the history of the development of cold fusion. We strongly believe that scientists have the right to reject or accept other scientific work based on its merits. We completely reject the right of any scientific group to be immune from criticism regardless of their institution. When a widely circulated peer-reviewed publication is found to be faulty, then we believe that a professional criticism of the work deserves publication. The following article carefully evaluates an MIT paper that has been widely cited (especially by the U.S. Patent Office) as "proof" that cold fusion is not real. As a former worker in the Lincoln Labs, I have great love and respect for MIT, the institution, and regret the events that have led to Dr. Mallove's resignation and to the need for the publication of this article by Dr. Swartz. *Fusion Facts*, will, of course, provide space for a reply to this article from Albagli, et al. Hal Fox, Ed.

### 1.0 INTRODUCTION: ASYMMETRIC CURVE TRANSFORM

In the Spring of 1989, electrochemically-induced excess enthalpy reactions using palladium filled with deuterons from heavy water [1] were initially reported to be very difficult to reproduce. Because of those difficulties cold fusion incorrectly connotes a "failed" technology in the minds of many scientists -- even as a positive literature supporting the phenomenon grows. Experimental papers now involve palladium, titanium, and possibly nickel, with the chief product being excess heat [2,3]. Other products released include tritium [4,5], sparse neutrons [6,7,8], helium$^4$ [9], and possibly heavy elements [10].

Many skeptics [11] cite the published Phase-II data from the Massachusetts Institute of Technology Plasma Fusion Center (MIT PFC [12]). Despite increasing evidence for cold fusion from labs in France, Japan, and Italy, the work-in-progress within the United States declined precipitously by late 1989 and has remained virtually non-existent, in part because of the publication of the Plasma Fusion Center Phase-II data, and the widespread press which it received.

By 1991, scientific criticisms of the PFC data and techniques appeared. The data was criticized as being faulty with respect to its thermal calculations and conclusions [13]. More criticism accrued over differences between an in-house report (July 10, 1989) and the later (July 13, 1989) prepublication report [14]. The most serious complaint has involved alleged "shifting" of the heavy water excess power curves [15].

What follows is an independent examination to both determine if there was a curve "shift," and whether the same method of data conversion was applied equally to the light and heavy water curves. This optical imaging study of the PFC Phase-II curves has revealed many curves purported by the PFC to represent the same experiment. Table 1 sorts them out into several types [see figure 1].

Section 1 is an overview of the PFC Phase-II curve catalog, and presents two measurements of the second baseline shift removed from Type 2 $D_2O$ excess power curve to obtain the prepublication Type 3 (and publication Type $3_B$ [12]) curves. Section 2 presents a critical analysis of the Phase-II methodology which is revealed to be insensitive to steady state ("d.c.") excess heats. More valid processing of the Phase-II data suggests excess heat may have been produced after 48 hours in the range suggested by Fleischmann and Pons experiment [1].

### 1.1 EXPERIMENTAL: IMAGE PROCESSING

The curves reported by the PFC (for both heavy and light water) were captured using image scanners including the raw heater power curves, the preliminary excess power curves (July 10, 1989), later "averaged" curves (July 13, 1989 - ref. [16]), the *J. Fusion Energy* published curves [12], and the March 10, 1992 Memo curves (later published in the PFC May 1992 Appendix [17]).

A Prime Option Scanner [400 dots per inch] was used. The images were stored as 16 bit data files, bitmapped by an Amiga 3000UX onto at least 1240 by 2100 pixels. To facilitate comparison, images were colorized (Professional Art Department (tm), [ASDG]) to enable unambiguous superposition of the different curves by applying each as a brush (Deluxe Paint IV (tm), [Electronic Arts]).

To accurately superimpose the curves, each was put in registration using rules at indicated power and time tics. Proportional control and careful alignment allowed each set of curves to be matched, superimposed, and recaptured. Some of the figures (Figure 2) thus demonstrate two sets of numbers along one axis consistent with the technique.

### 1.2 RESULTS: ASYMMETRIC ALGORITHM

Figure 1 shows how the Type 2 curve was already shifted once from the raw data (Type 1 curves). The Type 3

curve {composed of dots in the July 13, 1989 manuscript [16]} was reported to be a time average of the original data. The published curve {Type $3_B$ [12]} is nearly identical to the (dotted) Type 3 curve [14] and differs only in that the heavy water curve darkens locally between some groups of data points.

Figure 2 shows the matched and superimposed excess heat curves taken from the July 10, and 13, 1989 manuscripts. The upper set is for the light water (cell A). The lower set is for heavy water (cell B). For each set both the July 10, 1989 data (continuous curve in black) and the July 13th prepublication "averaged" data (gray dots) are shown. The two continuous curves (Type 2 from the July 10, 1989 manuscript) have high frequency components.

Examination of the light water curves (Figure 2, upper pair) reveals a very good correlation between the July 10th (Type 2) and the twice-processed (July 13th, Type 3) prepublication curves. This implies there was a negligible (second) baseline subtraction for the light water data.

In contrast, the two heavy water curves (Figure 2, lower pair) have a poorer correlation which imply a significant second baseline subtraction. Almost none of the twice-processed data points overlap the July 10th data beyond 30 hours. The Type 3 curve also appears too wide on its left hand side.

## 1.3 RESULTS: DERIVATION OF THE CURVE SHIFT

An attempt was made to determine what algorithm(s) were used to process the July 10th $D_2O$ curve (once-processed Type 2) to create the prepublication curve (twice-processed Type 3). First, a temporal averaging for the heavy water July 10, 1989 curve was derived. Figure 3 shows three sets of curves. The upper set shows both the low-pass ("time averaged") curve and the PFC heavy water Type 2 (July 10, 1989) curve from which it was obtained. Next various linear baseline shifts were effected upon the low-pass $D_2O$ curve, and are shown in the mid and lower portions of figure 3.

For simplicity, a linear model was used to determine the transformations used, i.e., Type 2 + [A+(B*t)]---> Type 3. A and B are the linear terms for that second baseline shift. A has the units of watts; and B has the units of watts/second.

Several curves were derived using various A and B parameters. The middle set of curves in figure 3 are the superposition of the low-pass curve with four linear baseline shifts [A=0]. The published heavy water data of the PFC is also shown as the darkest dots. The darker gray continuous curve (with the greatest time-varying baseline shift) shows the superposition of A=0 and B= -158 nanowatts/second upon the low-pass curve.

The lower set of curves in figure 3 show the superposition of the low-pass curve with various fixed (non time-varying) baseline shifts [B=0]. The published [12] Type 3 data points are again shown as the darkest dots. The darker gray continuous curve (with the greatest fixed baseline shift) shows the effect of A = -59 milliwatts and B=0 superimposed upon the low-pass curve.

It is important to consider that the published curves for heavy water [Type 3 or $3_B$] are too thick on their left hand side. Because the possibility of superimposed points added to a linear transformation cannot be excluded, it is unclear which values of A and B produce the best fit.

Notwithstanding the above, the transformation used to change the Type 2 (7/10/89) to Type 3 (7/13/89) $D_2O$ curve was approximately:

fixed baseline shift [A=] -9.6 milliwatts, and
time varying shift [B=] -94 nanowatts/second.

To determine the actual energy subtracted from the July 10, 1989 curves, the integrals under the power curves were measured. Then using the duration of the experiment, the average excess power subtracted was derived. For the heavy water this derived power was about 30 milliwatts (+/- 15%) in the time beyond 18 hours. The light water curve was characterized by an average power baseline subtraction of 0.3 milliwatts. In summary, this analysis demonstrated that either a relative $D_2O/H_2O$ excess power was measured, or that an asymmetric (second) treatment of the data was used after July 10, 1989, that is, the light and heavy water data were processed differently.

## 1.4 RESULTS: ARTIFACTUAL POINTS

Some of the points in the published $D_2O$ curves appear to be possible artifact, of two varieties. Points A and A' in figure 4 are one variety. These points are arbitrarily positioned along what was purported in the published paper to have been "intentionally introduced as a time calibration mark." It is not clear either how the locations of points A and A' [in Type 3 and Type 4 curves, respectively] were chosen, or why the published temperature curve [12] changed at that time.

A second variety of possible artifact concerns the apparent higher density cluster of points on the $D_2O$ curve between 18 and 25 hours [figures 2, 3, and 4]. The PFC publication [12] indicates that dots were the result of a time "average(d) over 1-h(our) blocks."

The upper set of curves in figure 4 are the Type 3 $D_2O$ and $H_2O$ curves, one above the other to facilitate examination. Gray dots were located over each data point

to facilitate a count within a forty (40) hour period for these curves. Although less than 42+/-1 dots were expected, for the $D_2O$ curve the number of dots exceeds 45. Although differences in sampling rate could account for the number, it would not account for the unusual thickness of the published heavy water curve on its left side.

These observations of artifacts were reported to the PFC in February 1992. The March 10, 1992 PFC memo presented two "new" curves to "correct" this matter [17]. One new curve (Type 4) is examined in the mid and lower sets of curves in figure 4. The mid set of curves compares one published curve (Type 3, dark dots) to the post-publication-3/10/1992 (Type 4) curve. The two curves have been normalized, put in registration, and superimposed. The only change is that in the 3/10/92 memo, the Type 4 curve had the symbol "[ ]." For easier identification, a solid gray dot has been placed between each pair of brackets.

At least three published data points [labelled "i," "ii," and "iii"] are missing from the Type 4 curve. Most notably absent is data point "iii," the expected opposite superior half of the "incidental" points: "A" and "A'."

The mid set of curves in figure 4 also compares data points A (Type 3) and A' (Type 4). Point A' appears shifted toward the baseline. This is examined further in the lower set of curves in figure 4 which have been normalized and are in registration for comparison. The times are hours and the power (vertical axis) is watts. One published curve [Type 3 (PFC/JA-89-34)] is on the right hand side and is gray colored. The data from the July 10, 1989 curve [Type 2] is also shown on the right side, located slightly higher (black continuous curve). The Type 4 curve is on the left hand side.

The magnitude of the "data" point shift [A to A'] is +24 milliwatts. This point shift is one of a number of changes used to optically identify and distinguish this Type 4 curve.

## 1.5 ANALYSIS: EVAPORATION FROM $D_2O$ SOLUTION

Why was a second baseline subtracted from the $D_2O$ curve after July 10, 1989? The PFC has said that the "shift for the heavy water data came as a result of a computer subtraction designed to compensate for water evaporation [15]." Could increased solvent loss for $D_2O$ actually impose a need for a greater "baseline shift"? Albagli [12] states that the temperatures of the two solutions were similar. However, temperature curve imaging indicates that the $H_2O$ experiment was operated at a higher (1 degree Centigrade) temperature.

Given that the $D_2O$ solution was at a lower temperature than the $H_2O$ solution, and given that for each temperature the vapor pressure of $D_2O$ is less than $H_2O$ [deuterium oxide melting point (STP) is 3.82 Centigrade, its boiling point is 101.4C] more evaporation must have occurred for the light water. Therefore, the baseline subtraction should have been less for the $D_2O$ experiment.

There is another reason why less baseline subtraction should have been used for the $D_2O$ data. The PFC data itself indicates that evaporation was a minor source of solvent loss, because the calculated thermoneutral potentials were quite close to their theoretical values [13]. Therefore most solvent loss occurred by electrolysis. Such solvent loss would be greater for the $H_2O$ solution again because such electrolysis is, in fact, used to commercially isolate heavy water due to light water's tendency to form gas by electrolysis first.

In summary, putative differential excess solvent loss for heavy water is not a reasonable explanation for the asymmetric algorithm used to shift the 7/10/1989 $D_2O$ curve.

## 1.6 ANALYSIS: EFFECT OF CURVE SHIFT

"This equation ($P_x+P_h$ = constant) allows the unknown power, $P_x$, to be determined. If $P_x$ increases, then the feedback control system of the calorimeter reduces $P_h$ to maintain $T_c$ constant [12]." Therefore any potential curve shift would hide part, or all, of any measured excess heat.

Furthermore, unless stated otherwise, readers presumed that baseline shifts were the same for the $H_2O$ and $D_2O$, and based upon careful calibration experiments. "(T)he data for the $H_2O$ curve ... was taken at the time of the 1989 experiments, and in the exact same way that the data was obtained for the $D_2O$ curve [19]."

## 1.7 SUMMARY OF SECTION 1

In summary, Table 1 lists the different curves which have been purported by the PFC to be the results of a single Phase-II heavy water experiment in 1989. Neither baseline data from before the experiment, nor calibration data concerning the putative heat loss versus solution height, nor the solution volume during the experiment, have been released or provided. Given these limitations, the following optical assignments remain presumptive.

Type 1 curves are the raw heater power data. The best experimental graph is $1_D$. A baseline shift of about 900 nanowatts/second was applied to the Type 1 curve to get the Type 2 curve. A second 94 nanowatts/second shift was applied after July 10, 1989 to generate prototype 3. The superposition of additional points, not present in

either the Type 1 or Type 2 curves, completes the transformation to the Type 3 curve (7/13/89).

The Type 4 curve (which appeared March 1992 [17, 18]) has one of the published "data" points moved 24 milliwatts. Other questioned "data" points are missing. In the "newest" curve [Type 5] all of the questioned "data" points have vanished.

## 2.0 INTRODUCTION: SENSITIVITY OF THE PHASE-II METHODOLOGY

The March 10, 1992 PFC memo states they "first subtract(ed) the baseline drift [so that] any onset of anomalous heating would appear as an excursion from zero." That memo and the subsequent Appendix [17] explored various linear curves [derived by uncertain fit over varying portions of the data] used to produce baseline shifts.

Given the widespread reports confirming cold fusion phenomena [2,3,4,5,7,8,9,10] and the recent release of the PFC Phase-II methodology [17], it is reasonable to evaluate the technique. The following analysis determined whether the Phase-II method was sensitive to excess enthalpy characterized by a ramp and step function, including the presence of a changing baseline.

## 2.1 ANALYSIS: CAN THE PHASE-II METHODOLOGY DETECT EXCESS POWER

Figure 5 shows five sets of curves. Each set has three curves derived from three hypothetical "successful" experiments wherein excess enthalpy occurred at time T. Each set of three curves is labelled a, b, and c, and is the result of "operations" used to process the first set of curves in the upper left of the figure.

These hypothetical curves (labelled with lower case nonsubscripted letters) should not be confused with the several curves of figure 1 such as Type $3_B$ curve (note upper case subscript).

In figure 5, curve 1a is the first hypothetical heater power curve, with a slow "turn-on" of excess heat at time T. An expected delay to onset is reasonable. As was noted in the March 10, 1992 PFC memo "(t)he level of excess heat .... was claimed to appear after an initial 'loading period' of some hours or days."

Curve 1a is initially stable (flat baseline) but then decreases as the hypothetical excess heat increases. The shift occurs because the heater requires less input power to maintain the same temperature.

One complexity is that the baseline of the heater power curve, prior to a "successful" experiment, may not be flat. Such baseline drift could be due to electrolysis loss (which might increase the thermal diffusion pathlength to the environment) or evaporation or other reasons. For the second hypothetical experiment (curve 1b) there was the same change in excess heat at time T, but superimposed upon an intrinsic baseline drift. Curve 1c is the hypothetical experiment wherein at time T a step function occurs rather than a ramp function.

The following analysis demonstrates why it is critically important to notice that a linear regression fit can be made to either the entire curve, or to the baseline prior to the onset of excess power. Figure 5 presents these two methods. Curve sets 2 (with 4) and 3 (with 5) show each method.

On the left of figure 5, curves 2a, 2b, and 2c show the derivations of excess heat using regression fits made to the initial portions of the heater power curves prior to the onset of excess enthalpy. Such linear fits made using only the experimental data prior to the hypothetical onset of excess heat are labeled $Y_{HT}$ (for "true" linear regression fits) to distinguish them from the curves $Y_{HF}$ (for "false" linear regression fits). $Y_{HT}$ curves are fit to the curve prior to the onset of excess enthalpy, whereas the $Y_{HF}$ curve are fit to the entire data curve (including the excess enthalpic portion). The "truth" and "falsity" of each method will be apparent now.

Curves 4a, 4b, 4c show the result of deriving the true excess power: $P_{XT} = Y_{HT} - P_H$. Curves 5a, 5b, and 5c show the "falsely derived" excess power using the linear regression fits made to the entire curve: $P_{XF} = Y_{HF} - P_H$. The fundamental value of each method can be seen by comparing each matched pair of Curves 4a with 5a, then 4b with 5b, and then 4c with 5c. The $P_{XT}$ curves do qualitatively reveal the excess enthalpies which were postulated in this gendanken "successful" experiment. On the right side of figure 4, curves 5a, 5b, and 5c, the $P_{XF}$ curves, confirm that the flawed technique masks the excess heat even in every hypothetical "successful" case.

Note also that the $P_{XT}$ curves (4a, 4b, 4c) correctly derive that the excess heats began at time T. However, in the $P_{XF}$ curves (5a, 5b, 5c) there is only a breakpoint at time T, and the presence of excess heat is not clearly revealed. In the $P_{XF}$ curves there appears a surge of "excess" heat, a narrow impulse, at the initiation of the hypothetical excess heat. It is interesting that reexamination of the PFC heavy water published curves (Types 3 and $3_B$) reveals a similar pattern, a possible "signature" of excess heat detected by the Phase-II methodology.

In summary, the algorithm used in the Phase-II experiment is flawed because it hides any constant excess heat. Efforts using such an insensitive methodology are doomed a priori. Critical data analysis requires careful

measurement of the baseline drift before the turn-on of any putative excess heat. Perhaps additional heavy water should not have been added to the solution soon after the experiment started, but should have tried to obtain the best possible initial baseline, which may be lacking in Types 3, 3b, 4 and 5 curves.

## 2.2 ANALYSIS: REEXAMINATION OF THE PHASE-II $D_2O$ CURVES

Given the asymmetry of the curve shifts for the heavy and light water experiments, it is thus important to consider if there actually was excess power in the $D_2O$ experiment. The PFC paper [12] does give hints of excess power. First,

*"(w)hen enough solvent was added to the $D_2O$ cell to compensate for that lost to electrolysis at the end of the 100 h period shown in Figure 6, Ph (the heater power) returned to within 20% of its original value."*
*[Albagli et al., J. of Fusion Energy, 9, 133 (1990)]*

That 20% discrepancy in heater power, used to heat the same volume of fluid has been suggested as corroborating evidence that the heavy water cell produced excess heat [14].

Noninski [13] examined these thermal matters and calculated that as much as 2 watts per $cm^3$ palladium of excess power was generated over 60 hours for the heavy water.

The optical analysis and integral derivations of subtracted energy from the $D_2O$ curve, as discussed above, indicate approximately 400 milliwatts per $cm^3$ generated for the 9 cm by 0.1 cm diameter cathode.

Because of the flawed Phase-II methodology and these facts, the Phase-II $D_2O$ experiment was reexamined using the initial baseline drift on the Type $1_D$ curve. A calibrated excess power curve [$P_{XT}$, the "true" corrected excess power] for the Phase-II $D_2O$ experiment was derived. It is listed as Type 6 in Table 1 and shown in the upper portion of figure 6. The average excess power derived by this method is 62 milliwatts (+/- 34 milliwatts).

This derived value of excess heat is on the order of the measured difference between the Type 2 and Type 3 curves. It is qualitatively similar to the value expected for a "successful" experiment [1,12]. The time of apparent turn-on of excess heat in the derived Type 6 curve is also close to the expected time cited [12].

## 2.3 ANALYSIS - THERMAL PATHLENGTH LOSS

The PFC Appendix [17] indicates that the Phase II system was calibrated during the experiment by adding 5 cc of $D_2O$ at t=14 hours. The $D_2O$-air interface was described as located at L+x(t) from the top of the calorimeter. The Appendix purports without supporting evidence that because a 1/(L+x(t)) term dominates the thermal losses it produces a linear baseline drift of the heater power curve.

The Appendix [17] claims that such linearization follows because x is close to 0.0, and so 1/(L+x(t)) can thus be approximated as: $1-(x/L)+(x^2/L^2)$ and so there supposedly results a linear baseline drift.

But is x/L really close to zero under open conditions wherein solvent losses include both evaporation and electrolysis [0.2 amperes]? Use of the Faraday and the dimensions provided in Albagli [12] and the Appendix [17] indicate that the approximation was inappropriate before the experiment was over.

However, just suppose that it were appropriate to remove some baseline shift. Removing the baseline based upon a linear regression from data taken after the experiment begins simply eliminates any steady state signal. As figure 4 shows, the effect is not merely a baseline correction, but is the removal of the entire time-invariant signal.

Therefore, a correct method was made to determine the calibrated excess power using appropriate mathematics for the baseline correction. A series of curves was generated using the actual postulated 1/(L+x(t)) thermal resistive pathlength, instead of the improper linearization. The model consisted of two thermal loss paths, one fixed and one varying. The ratios of the two thermal losses are shown in the lower set of parametric curves in figure 6. When the Type $1_D$ curve is plotted upon the initial corresponding baseline-shift/thermal-loss curve [with an estimated 55% (+/- 20%) of heat exiting through the variable path] the difference (gray area) would be the calibrated excess heat for $D_2O$.

This derivation is similar to the excess heat derived optically and shown in the upper curve in figure 6. The parametric-baseline subtraction method appears to have more sensitivity to excess heat.

## 2.4 CONCLUSIONS and SUMMARY

The light water curve was published by the PFC essentially intact after the first baseline shift, whereas the heavy water curve was shifted a second time. The cells were matched [12], and solvent loss would be expected to be greater for $H_2O$.

The Phase-II methodology is flawed because it masks a constant [steady-state] excess heat. Furthermore this paradigm fails to use either the true baseline drift, and

may avoid the first 15% of the $D_2O$ curve in Types 3, $3_B$, 4, and 5 curves.

What constitutes "data reduction" is sometimes but not always open to scientific debate. The application of a low pass filter to an electrical signal or the cutting in half of a hologram properly constitute "data reduction," but the asymmetric shifting of one curve of a paired set is probably not. The removal of the entire steady state signal is also not classical "data reduction."

In the May 1992 Appendix, the PFC retroactively claims its "systematic" errors now total 100 to 400 milliwatts, implying an insensitivity of >30 kilojoules.

**Much current skepticism of the cold fusion phenomenon was created by the PFC paper's reporting "failure-to-reproduce" [12] as opposed to its later claimed "too-insensitive-to-confirm" experiments [17]. Because it may be the single most widely quoted work used by critics of cold fusion to dismiss the phenomena, the paper should have clarified all "data" points and the methodology used. Apparent curve proliferation, volatile points, asymmetric curve shifts, combined with an impaired methodology have needlessly degraded the sensitivity, and believability, of the Phase-II calorimetry experiment.**

REFERENCES

1. M. Fleischmann, S. Pons, "Electrochemically Induced Nuclear Fusion of Deuterium," *J. Electroanal. Chem.*, 261, 301 (1989).

2. J.O'M. Bockris, G.H. Lin, N.J.C. Packham, "A Review of the Investigations of the Fleischmann-Pons Phenomena," *Fusion Technology*, 18, 11, (1990).

3. E.F. Mallove, "Fire from Ice: Searching for the Truth behind the Cold Fusion Furor," John Wiley, New York, (1991).

4. M. Srinivasan, "Nuclear Fusion in an Atomic Lattice: An Update," *Current Science*, 143, (1991).

5. E. Storms, C. Talcott, "Electrolytic Tritium Production," *Fusion Technology*, 17, 680, (1990).

6. S.E. Jones, E.P. Palmer, et al., "Anomalous Nuclear Reactions in Condensed Matter," *J. Fusion Energy*, 9, 199, (1990).

7. D. Gozzi, P.L. Cignini, et al., "Nuclear and Thermal Effects During Electrolytic Reduction of Deuterium at Palladium Cathode," *J. Fusion Energy*, 9, 241 (1990).

8. H.O. Menlove, M.M. Fowler, et al., "Measurement of Neutron Emission from Ti and Pd in Pressurized D2 Gas and $D_2O$ Electrolysis Cells," *J. Fusion Energy*, 9, 495, (1990).

9. B.F. Bush, J.J. Lagowski, M.H. Miles, G.S. Ostrom, "Helium Production During the Electrolysis of $D_2O$ in Cold Fusion Experiments," *J. Electro. Chem.*, 304, 271, (1991).

10. T. Matsumoto, K. Kurokawa, "Observation of Heavy Elements Produced During Explosive Cold Fusion," *Fusion Technology*, 20, 323, (1991).

11. The United States Patent Office is reported to rely upon Albagli et al., (12) to reject all patent applications involving cold fusion or even those inventions capable of working in the "cold fusion environment."

12. D. Albagli, R. Ballinger, V. Cammarata, X. Chen, R.M. Crooks, C. Fiore, M.P.J Gaudreau, I. Hwang, C.K. Li, P. Linsay, S.C. Luckhardt, R.R. Parker, R.D. Petrasso, M.O. Schloh, K.W. Wensel, M.S. Wrighton, "Measurement and Analysis of Neutron and Gamma-Ray Emission Rates, other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes," *J. of Fusion Energy*, 9, 133 (1990).

13. V.C. Noninski, C.I. Noninski, "Comments on 'Measurement and Analysis ... Cathodes," *Fusion Technology*, 19, 579-580 (1991).

14. Complaint to President C. M. Vest, Massachusetts Institute of Technology, Cambridge MA from Dr. Eugene Mallove (August 1991).

15. C. Anderson, "Cold fusion tempest at MIT," *Nature*, 353, (1991). Also, Majorie Hecht, "A Case Study of Fudging," *21st Cent. Sci & Tech.*, Fall 1991, Vol 4, No. 3. page 54.

16. D. Albagli, R. Ballinger, et al., "Measurement and Analysis ... Pd Cathodes," PFC/JA-89-34 [July 1989].

17. S.C. Luckhardt, "Technical Appendix to D. Albagli, et al., *J. Fusion Energy*, **1990**, Calorimetry Error Analysis," MIT Report PFC/RR-92-7, (May 1992).

18. It remains difficult to interpret Types 4 and 5 curves because although the regression coefficients used to shift the raw experimental data curve (?Type $1_D$) were listed to five (5) decimal places, no mention was given of the many coefficients of fit, or as to why the first about 18 hours of data may have been left off the linearization scheme.

19. Personal communication [August 30, 1991] from Prof. R. Parker to the author.

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TABLE 1 : SIX TYPES OF CURVES FOR $D_2O$ EXPERIMENT

The PFC Phase-II $D_2O$ data has been displayed several ways since July 10, 1989. This table lists known curves of that single experiment, including the five Types reported by the PFC.

| # | CODE | DUR'N [HRS] | CONT/ DOTS | INCID PTS | QUEST PT | ?ASYM SHIFT | ? PT SHIFT | BLS nW/s | DOCUMENT [DATE] | ASSIGNMENT |
|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 1 | 0-98 | CONT | n.a. | n.a. | n.a. | n.a. | n.a. | 7/10/89 | Raw data |
| 2 | $1_B$ | 0-98 | CONT | n.a. | n.a. | n.a. | n.a. | n.a. | JFE 90 | Published |
| 3 | $1_C$ | 0-80 | CONT | n.a. | n.a. | n.a. | n.a. | n.a. | 9/1991 | Memo 1991 |
| 4 | $1_D$ | 0-98 | CONT | n.a. | n.a. | n.a. | n.a. | n.a. | 3/10/92 | Best? |
| 5 | $1_E$ | 13-98 | CONT | n.a. | n.a. | n.a. | n.a. | n.a. | 5/92 | PFC/RR-92-7 |
| 6 | 2 | 0-98 | CONT | n.a. | n.a. | ? | n.a. | 900 | 7/10/89 | Memo 7/10/89 |
| 7 | 3 | 18-98 | DOTS | +++ | +++ | yes | n.a. | 994 | 7/13/89 | Memo 7/13/89 |
| 8 | $3_B$ | 18-98 | DOTS | +++ | ++++ | yes | n.a. | 994 | JFE 90 | Published |
| 9 | 4 | 18-98 | DOTS | + | gone | yes | yes | 994 | 3/10/92 | 3/10/92,5/92 |
| 10 | 5 | 18-98 | DOTS | gone | gone | yes | gone | 994 | 3/10/92 | Memo 3/10/92 |
| 11 | 6 | 18-98 | CONT | n.a. | n.a. | n.a. | n.a. | yes | Fig.6 | Calibrated |

CODE - TYPE. Type 1 has five graphs, shown in figure 1. They are nearly identical with various portions clipped or removed. Curve $1_D$ may be the best raw data curve released to date.

DUR'N - Duration of Experimental Time shown in graph. Clipping often occurs at >80 hours or 0-16 hours.

CONT - Continuity. Continuous (CONT) or binned samples (DOTS).

INCID(ental) Points - Points from experimenter-created glitch.

QUEST(ionable) Points - Points beyond the raw data curve.

?ASYM(metric) SHIFT - Is there a second shift of the curve beyond the first baseline shift.

? P(oint) SHIFT - Is there movement of point "A" to "A'."

BLS (Baseline Shift) - Is there any baseline shift?. Quantities are nanowatts per second.

n.a. Not Applicable.

ASSIGNMENTS BY TYPE

Type 1 are the raw data (heater power curves) which have been released several forms, which appear to be identical.

$1_D$ - BEST PROBABLE RAW EXPERIMENTAL DATA - continuous.

$1_C$ - PROBABLE RAW EXPERIMENTAL DATA - continuous,
Identical to $1_D$; clipped at circa 80 hours.

Types 2 and 3 are the previously released 7/10 and 7/13/89 excess power curves. Type $3_B$ was published [12], and is slightly different from Type 3, but not enough to warrant a new category.

2 - FIRST-SHIFTED BASELINE [July 10, 1989 curve]
Continuous [shift circa -900 nanowatts/second].

3 - SECOND-SHIFTED BASELINE [July 13, 1989 curve]
Sampled data [shift circa -990 nanowatts/second].

$3_B$- PUBLISHED $D_2O$ CURVE - Figure 5b in J. Fusion Energy (9, 199)

Types 4 and 5 [March 10, 1992 PFC memo] were derived using various regression fits [although given to five significant figures, the correlation coefficients were not presented].

4 - NEW SHIFTED BASELINE [Fig. 5 in 3/10/1992 Memo]
Sampled data : one incidental point missing; the other shifted 24+ milliwatts; other minor differences.

5 - NEWEST SHIFTED BASELINE [Fig. 9 in March 10, 1992 Memo]
All incidental and questioned points missing.

6 - CALIBRATED DERIVATION OF EXCESS HEAT
Correction based upon initial slope of Phase-II data. The Type 6 curve is shown in Figure 6.