Summary Of The Third Annual Conference on Cold Fusion

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Abstract

We review highlights of the international cold fusion conference that was held recently in Nagoya, Japan. Excess heat results in heavy water electrolysis experiments constitute the observations with the most important potential applications. Experiments in gas phase systems exhibit fast particle and gamma emission that make progress toward elucidating mechanisms. The evidence in support of a light water heat effect has improved.

Introduction

The Third International Cold Fusion Conference took place in Nagoya, Japan between October 21 and October 25, 1992. Over 300 attendees participated, listening to about 27 oral presentations and looking over roughly 80 poster papers. Many people have asked me about the conference, and rather than repeating the same things over and over again, I thought that it would be useful to put my thoughts down on paper as a more efficient method of communication.

Given the near complete absence of cold fusion sessions in more traditional physics and chemistry meetings, the international conferences represent about the only chance for people in the cold fusion field to get together and learn about what has happened lately. The international conferences, starting with the Salt Lake City conference in 1990, followed by Como, Italy in 1991, and now Nagoya, Japan in 1992, have been and continue to be the most important sources of reliable and relevant information in the field; an important meeting was also held at BYU in 1990 which focused on nuclear products. The field is advancing pretty rapidly these days, and since publications tend to lag with more than a year's delay, the conferences and conference proceedings play a key role in the field. The next international conference was originally scheduled to take place in Hawaii in November, 1993; I understand that it may be delayed until December.

The results presented at this conference were overall technically much stronger than last year's conference, and benefitted by a very strong showing from the Japanese contingent. I will

first itemize what I thought were some of the most interesting new experimental results presented at the conference.

I admit to having numerous biases. One bias is that I believe the observations of excess power are ultimately the most important, both scientifically and technologically. Another bias is that I favor results which in my view help to elucidate reaction mechanisms.

Following the discussion of significant positive results, I review abstracts and presentations of negative results. Coming from the theory end of the field, I felt that it was appropriate for me to survey the theory papers which were presented (in the following section); in this case, it was possible to include a larger fraction of the papers submitted. Having my own theory as to the origin of the effect, I warn the reader that my discussion of theory necessarily carries a bias in favor of my world view; it is my hope that this discussion will be useful in spite of this bias. Almost as interesting in some cases as what was presented, was what was not presented; a discussion of work that was absent is presented before the summary and conclusions.

For a review as long as this one, there are many issues and many details, most of which I have made a serious attempt to get right. I would hope that I will not make enemies of those whose work I did not include (which at this point will include about half of all papers submitted). This review was constructed from preprints, notes, memory, and discussions with many people in the field -- should the reader note errors or misconceptions, I would appreciate corrections.

Survey Of Positive Results

1. S. Pons[1,2] described briefly recent results obtained at the Japanese-funded IMRA laboratory in Sophia Antipolis, France. During the Como meeting (July, 1991), Pons and Fleischmann had announced that they were able to obtain very high levels of excess power production (on the order of 1000 Watts/cm^3) corresponding to a factor of 10 power gain, and that they had done so 11 times.

   Part of their research since then has focused on defining a procedure that would improve on the reproducibility of this very high power effect (at Como, they had announced that complete reproducibility had been attained on achieving consistent excess power at lower levels). At Nagoya, Pons reported that this had been accomplished; that very high levels of heat production (more than 1 kilowatt/cm^3) were now obtained reproducibly accompanied by a factor of 4 power gain.

   The key to the new results included some advances that they outlined. One such improvement involves the observation that the excess power generation increases at higher temperatures. The cathode is charged at intermediate current densities at temperatures below 50° C for several days, and then the current is stepped up. Due to the relatively low thermal loss of the cell and calorimeter, the cell temperature rises, but the loading is maintained. This rise improves the excess power generation, which in turn

drives the temperature higher; the positive feedback leads to very high excess power generation and vigorous boiling.

Pons and Fleischmann perform their calorimetry using open cell systems, which have the advantage of being cheaper and more accessible, and allows them to do more experiments at a time. The particular method of calorimetry which they have developed was motivated in part by the existence of the positive feedback described above -- Pons and Fleischmann are able to achieve good calorimetric precision with time-varying electrolyte levels, cell temperatures and cell voltages. Most others have sought in their work to maintain either constant temperature or power, or else require the presence of steady-state conditions in their system to obtain accurate results. Very few groups have so far taken advantage of such sophisticated methods to obtain excess power values from their raw data; no other groups have yet reported the ability to obtain reproducibly the high power and boiling mode reported by Pons and Fleischmann.

It was pointed out by Pons that the calorimetry could be checked during the very high excess power burst by measuring the time taken to boil away the electrolyte, and using a knowledge of the heat of vaporization to compute the total energy and hence power generation. He presented the results of this analysis for one cell, which he said was in agreement with the calorimetric results.

Pons stated that 2.5 moles (close to 50 cc) of D_2O were boiled away during a time of about 10 minutes, during which time the average *iv* input power was 37.5 watts. The numbers can be checked, as follows: The heat of vaporization of heavy water is about 41 kJ/mol at 100° C, and 2.5 moles of heavy water corresponds to 102.5 kJ; the energy lost during this time in the calorimeter (primarily radiative) is 6.7 kJ. The input electrical *iv* energy during this time is 22.5 kJ. The excess energy produced is the output energy (102.5 + 6.7 kJ) minus the input energy (22.5 kJ), or 86.7 kJ. The production of 86.7 kJ in 10 minutes corresponds to an excess power of 144.5 watts, and a power gain of 3.85.

The volume of the cathode was given to be 0.0785 cm^3, which was noted by many (this volume was in error, as will be commented on shortly). The average excess power claimed during the boiling episode was 144.5 Watts, which would correspond to 1841 W/cm^3.

The cathode geometry was given by Pons to be cylindrical, with a diameter of 2 mm and a length of 1.25 cm. I note that this geometry does not correspond to the volume quoted by Pons above -- a rod of these dimensions would have a volume of 0.03927 cm^3, which is almost precisely a factor of 2 smaller than the volume given during Pons talk. Pons has confirmed that this smaller volume is correct (the correct value appears in their conference proceeding[2]). I will continue my discussion here using the corrected power per unit volume, which is 3682 W/cm^3.

The anomalous excess energy production in this experiment is considerable, as can be calculated. In 1 minute, 8.7 kJ of excess energy is produced. At a density of 12.02 g/cm^3 and an average mass of 106.42 amu, pure Pd contains 6.8×10^{22} atoms/cm^3. The total

number of atoms in the cathode is 2.7×10^{21}, or 0.0044 moles. In 1 minute, the excess energy production is 1.96 MJ/mole, which corresponds to 20.3 eV/atom of Pd. This number is greater than can be accounted for by a chemical explanation for the effect. After 10 minutes, the cathode has produced 203 eV/atom.

In the absence of current flow, film-boiling limits the heat flow from the cathode at cathode temperatures higher than about 120° C; the maximum heat flux from the rod under these conditions is limited to I think somewhere near 125 Watts/cm^2. The surface area of the cathode, including the top and bottom, is 0.85 cm^2, which leads to an observed average heat flux of about 170 Watts/cm^2. This number is comparable to, but greater than my version of the film-boiling limit given above, and was a potential cause for concern.

Pons and Fleischmann have considered this effect, and have found experimentally that the presence of current flow delays the onset of film-boiling to higher temperatures and higher heat fluxes. In their conference proceeding, they claim[2] to have observed heat transfer rates during electrolysis in separate experiments which are between 1-10 kW/cm^2. I consider this result to be very important.

The cathode gets very hot in these experiments. Pons and Fleischmann have observed the Kel-F supports at the base of the cathodes to melt, from which the presence of temperatures in excess of 300° C are inferred. A direct measurement of the cathode temperature is currently problematic; Pons is currently interested in practical proposals as to how to do this without impacting the electrochemistry.

A common misunderstanding often occurs in the discussion of the results of Pons-Fleischmann experiments which is of interest here. It is sometimes argued that the energy production during a short event can be disregarded, since there may exist energy storage mechanisms which could have been collecting energy at a low level for a long period of time. For example, the total energy output from this experiment would not be very much larger than the total input energy if no heat excess had occurred prior to the boiling event (1 watt-day = 86.4 kJ). This type of argument seeks to make palatable the notion that since the total energy excess measured over days is small compared to the input (and hence there might exist a signal to noise problem in the measurement), the measurement can be dismissed. As discussed above, this type of argument completely misses a key implication of the experiment -- specifically, that there exists no known physical mechanism which could store the energy observed to be released during the boiling episode.

It is true of this experiment as well as of others to be described below, that no products or "ashes" of the heat have been found and verified that are commensurate with the energy production. This will be discussed further below.

2. M. McKubre[3,4] described experiments done at SRI during the past several years. They have developed closed cell flow calorimeters, which are ideally *first principle* calorimeters (which means that the heat flow out of the cell goes into the flowing water coolant, and the power generation is determined by measuring the mass flow rate and

output to input flow temperature difference, with no calibration required). They have succeeded in reducing the conduction losses (which are not first principle contributions) down to the order of one per cent, and then they calibrate the Fick's law constant associated with the conduction losses. Overall, the SRI calorimeters achieve a relative accuracy in the calorimetric measurements which is on the order of a few tenths of a per cent.

The SRI group reported the development of a procedure at Como that appeared to yield excess heat essentially every time, and this method is described in the Como conference proceedings. Highlights of these experiments were discussed. A significant advance that was pioneered by the SRI group was described: it consisted of the addition of aluminate or silicate to the electrolyte, which caused the formation of a colloidal surface layer that passed light ions (deuterium, lithium, boron,...) and shielded the surface from impurities; this procedure improves the ability of the Pd rod to maintain a high loading ratio.

Two distinct modes of excess power generation were observed; one in which the excess power occurs at relatively low levels (1%-50%) and responds to changes in current density (they have observed 38 occurrences of this mode, lasting hours to many days), and one that is characterized by much higher relative power levels (up to 350% excess) and appears to be insensitive to changes in current (this mode has been observed 3 times, lasting many hours).

For the first mode of heat generation, SRI finds that the excess power rises linearly with current above a threshold current density (which is on the order of 100-200 mA/cm^2). A graph illustrating this appears in their conference proceeding.[4] This is in apparent contrast with the Pons and Fleischmann results, which showed a possible quadratic component to the increase above threshold current. Discussion during the meeting pointed to the fact that the SRI experiments are run at constant temperature, while the temperature of the Pons and Fleischmann cells increase when excess heat is produced.

Mckubre presented a graph of excess power production as a function of fractional deuterium loading as determined from resistance ratio measurements. This dependence was found to increase roughly parabolically above a loading of 0.85 ($P_{xs} \sim (x-0.85)^2$) up to loadings near 0.95, which is as high as had been achieved during their C1 experiment.

The group has spent considerable effort chasing down and quantifying uncertainties in the SRI experiments, and are now able to assign meaningful error bars to essentially all quantities measured and inferred in their experiments. The result of this analysis yields rather high sigma numbers on the excess power measurements (in excess of 50 sigma on some of the best data analyzed so far).

Their largest power numbers correspond to on the order of 15 W/cm^3; it would take a small number of hours of running at this level to defeat a chemical storage explanation. Their highest excess total energy numbers have reached 200 MJ/mole of Pd, which corresponds roughly to 2 KeV per Pd atom; this level of excess energy production cannot be of chemical origin.

3. K. Kunimatsu[5,6] of IMRA JAPAN Co. in Sapporo presented results on their heavy water electrolysis experiments. A number of things struck me as being interesting about this talk, aside from the fact that this is one of the first presentations of anything from this group at a conference at which I have managed to be. This effort appears to have a great deal of resources and some nontrivial technical expertise. They reported excess power measurements as a function of loading (where the loading was determined through measurements of the deuterium gas pressure in the cell), and arrived at essentially the same dependence of excess power on loading fraction as SRI, but with the cut-off shifted down by a few points relative to the SRI results (from a D/Pd ratio of 0.85 inferred from a resistance ratio measurement, down closer to 0.83 determined from measurements of the D_2 gas pressure[6]). The peak excess power occurs when the current density is greater than 100 mA/cm^2, and the peak excess power which appears on the graph in Ref. 6 is about 35%.

   The IMRA experiments differed qualitatively from the SRI and Pons-Fleischmann experiments in that they were run in fuel-cell mode. Conventional Pons-Fleischmann electrolysis experiments are run such that D_2 gas is generated at the cathode and O_2 gas is generated at the anode. IMRA has developed a pressurized cell in which deuterium reactions occur at the anode. Cells operating in fuel-cell mode have been developed in the past, however, this is the first time that I am aware that a Pons-Fleischmann cell has generated excess power sufficiently reliably while operating in such a mode to produce relatively high quality excess power data of the sort presented.

   This group seems to have good people, good funding, and much expertise. They presented several poster papers on studies of excess power generation, and the absorption of hydrogen and deuterium in palladium cathodes[7-10] I think that we will be hearing much more from them in the future.

   I note that a positive correlation between loading and excess heat production in a Pd/D modified Pons-Fleischmann experiment was reported by Scaramuzzi and De Ninno in a poster paper.[11,12]

4. Ya. R. Kucherov[13-15] from the Luch Association, Podolsk, Moscow Region described experiments that I thought were very important.

   The experiment involves using a glow discharge to load a Pd (or other metal) foil (1 cm $\times$ 1 cm $\times$ 0.1 mm - 1.0 mm) in D_2 gas at 10 torr, with a 400 V discharge (10 - 500 mA current). Apparently this group has had considerable experience with glow discharges and is aware of several tricks that help to preserve the surface of the cathode which helps to attain very high loading (a D/Pd ratio of more than 1).

   Numerous effects are observed; excess heat production will first be considered. Temperatures were monitored using W-Re thermocouples in the cathode and anode, and also CC thermocouples in a heat collector some distance from the cathode. Calibration was done through comparing temperature histories of “live” Pd cathodes (cathodes

producing neutron, gamma and fast particle emission) in deuterium with those of "worn out" cathodes (cathodes producing no anomalous emission). Excess power production at the level of tens of watts is observed; their best result out of 78 experiments is a 33 watt excess representing a power gain of a factor of 5. Given the small total cathode volume, the resulting power generation rate is quite high; the highest values are on the order of 3000 watts/cm^3 of Pd. The highest total energy production observed to date exceeds 20 kJ.

After about 100 seconds after the start of the discharge, neutron emission is observed (a huge signal, reaching up to 10^6 neutrons/sec in some experiments). The neutron detection described in their earlier work was done using RUP-1 silver activated ZnS scintillation detectors and type SNM-18 gas discharge (^{3}He) detectors. The 10^6 neutron/sec signal appeared in the scintillation detector as 2000 counts/sec at a distance of 1 meter; the signal showed up as 10000 pulses/second at a distance of 30 cm on the SNM-18 detector. No emission was observed using a hydrogen discharge.

After a while, gamma emission is then observed (also a huge signal, up to 10^5 gammas/sec in some experiments). The gamma emission was studied using four detectors (Ge-Li, stilbene, NaI and SPS plastic); most of the recent results were obtained using a liquid nitrogen-cooled Ge-Li DGDK-50 detector with 1.6 keV resolution at 1332 keV, and an efficiency of 10^{-3} at 511 keV. An example of an anomalous gamma spectrum from Pd is shown in a recent publication[16].

Gamma lines were identified from short-lived isotopes (the gamma spectrum returns to its initial state in 3-5 days), and some of the identified lines originate in isotopes in the neighborhood of Pd (lines originating from isotopes with a nuclear charge of Z-3 to Z+8, where Z=46 for Pd, were observed).

A very substantial flux (10^4 to 10^6 ions/sec) of fast ions is emitted from the cathode, and silicon surface barrier detectors were used for detection. The bulk of the emission occurs between 1-5 MeV, and in some experiments lasts for a few minutes after the discharge is switched off which allows for an accurate determination of the spectrum. Correlated fast ion emission was registered on calibrated CR-39 plates installed inside the discharge chamber.

A small fraction of the fast ions are observed at high energy; peaks were observed at 6 MeV, 12 MeV and 16 MeV. The mass of the particles at 12 MeV and higher was determined to be greater than or equal to 4, as determined through measurements with different barrier thicknesses.

5. There was a Chinese team[17-20] that presented results from a somewhat similar system to that described by Kucherov. A glow discharge was created by applying high voltage (7-11 KV, 50 Hz) between two electrodes inside of a glass bulb containing deuterium at low pressure (4-13 torr). A thin (1 micron) metallic layer of the electrode material (for example, Pd) was deposited on the interior of the glass bulb. The glow discharge current was less than 100 mA; an anomalous current was observed with an average value of 1 A,

and excursions up to 10 A. A D/Pd ratio of 0.5-0.8 was claimed to have been obtained.

Substantial neutron emission (13-330 neutrons/sec) was observed, and the energy spectrum was resolved with a recoil proton fast neutron scintillation spectrometer. The resulting neutron spectrum contained both 2.0-2.5 MeV neutrons, and broad emission between 2.5-7.0 MeV; most of the emission occurred above 2.5 MeV.

Neutron emission was also recorded from metals chosen at random, and the signal strength varied with metal according to the order Pt, Nb, W, Pd, Ag, Cu, Mo and Fe. The fluence observed from the D/Pt system was 1.2×10^4 neutrons/sec.

The energy spectrum of the neutron emission for these metals was also observed. In the case of the D/Pt emission shows broad emission up to about 8 MeV, decreasing generally with increasing neutron energy, and with a number of possible peaks appearing.

Intense gamma spectra were also observed with a NaI scintillation counter during the experiments; the gamma ray yield was about ten times that of the neutron yield. The gamma spectrum of D/Nb showed lines at tens of KeV, 3.4 MeV and 5.8 MeV, and some unresolved emission below 7 MeV.

These experiments seem to me to be similar to the experiment described in 1989 by Wada.[21] Another experiment of this sort was reported by Tazima, Isii and Ikegami, and also by Jin, Zhang, Yao and Wu, at the Como conference.

6. E. Yamaguchi of NTT presented a paper[22,23] on ^{4}He production from a PdD foil that is sandwiched by gold and MnO_x. I think of the NTT research labs as being the ATT Bell Labs of Japan, which has an excellent technical reputation. This paper attracted considerable interest in the Japanese media, and there were reports that the price of the NTT stock climbed as a result. The NTT stock climbed a bit more than 10%; Morrison pointed out in his review that the stock went back down to its pre-announcement value within a few days.

   In the experiments that he reported, a current of 0.5-0.8 A/cm^2 is applied perpendicularly to the sandwich. The foil produces heat at a level of 0.5-5 Watts for about 1000 seconds (this is the case whether the foil is PdD or PdH), and then explosively outgasses. At the peak of the outgassing, the samples undergo substantial plastic deformation which lasts for about 10 seconds. During his presentation, it was not obvious whether the temperature rise observed was being claimed as anomalous or not. If the foil is deuterated, these phenomena are accompanied by ^{4}He emission.

   Yamaguchi previously reported at the BYU conference very high levels of neutron emission from this system at a 10^6 neutrons/second. The experiments described at Nagoya included only helium, heat, and fast charged particle detection.

   The ^{4}He emission is monitored using an expensive high resolution mass spectrometer that is capable of distinguishing between ^{4}He and D_2 signals, as was demonstrated. A minor

peak in the data appears near the expected HT mass position, and Yamaguchi claimed that this signal indicated the presence of HT (Claytor notes in his trip report that the HT signal, if real, would imply a "radiological hazard (> 10 Ci)."). The H_2D trimer is more massive than D_2 and does not interfere with the ^{4}He measurement.

Yamaguchi sees ^{4}He in his mass spectrometer when he uses PdD, and he sees no ^{4}He when he uses PdH. Yamaguchi stated that the amount of ^{4}He was "consistent with the heat," but if he gave figures for the amount of ^{4}He produced, I missed them. Given that heat occurs for PdH runs as well as for PdD runs, it is not clear what the statement means. Hopefully this issue will be clarified at a later date.

When asked whether the ^{4}He is due to contamination, Yamaguchi argued that it is not in the D_2 gas used, it is not in the metal, and the vacuum system being used is a high quality system of the type used in semiconductor research that will hold a 10^{-6} torr vacuum for a month without pumping.

Yamaguchi also sees 3 MeV protons and fast alphas at 4.5-6 MeV using two identical systems based on silicon detectors (Canberra Si-SSD:PD-450-19-700-AM; active area=4.5 cm^2, active thickness = 700 mu). Protons were observed at 3 MeV, and were attributed to the *p*+*t* branch of the *dd*-fusion process. Significant emission was observed between 4.5-6.0 MeV; by comparing signals with and without an intervening 7 mu foil, these signals were identified as being due to either alphas or ^{3}He nuclei. The total number of fast particles detected was a few hundred per experiment.

The experiment which Yamaguchi and Nishioka have constructed looks very impressive; I got the impression that the helium measurement capability was relatively new. I think that the ^{4}He signals are real, but I am less convinced yet that it has been made through an anomalous effect. The strongest argument in support of it being genuine is the rather strong time-correlation of the ^{4}He signal with the temperature excursion of the foil.

The NTT group has been active for years, and by now I think that the basic anomalies which they observe are likely to be right. The new result presented at Nagoya is the helium measurement, which I will be more comfortable with after Yamaguchi and Nishioka have had more experience exploring. I look forward to more results from this group.

I note that the first significant claim for substantial ^{4}He production in Pons-Fleischmann electrolysis experiments were made at Como by Miles and coworkers at China Lake. Previous negative results had been obtained in searches for helium in the cathode; Miles and coworkers claimed the observation of ^{4}He in the gas stream. Miles presented a paper at Nagoya[24] which gave an update of the group's recent efforts, which have been hindered by an inability to obtain significant excess heat.

Bockris reported at Nagoya[25] observations of ^{4}He above background (by factors of 2-100) that accompanied tritium production (described below); the helium was analyzed by thermal expulsion and mass spectroscopy.

7. S. Isagawa et al from the Japanese National Laboratory for High Energy Physics (the KEK collaboration) reported their results on experiments involving searches for heat, tritium and neutrons in Pons-Fleischmann cells.[26,27] I was impressed that the KEK was working at all in this area, and even though they have apparently had an effort at some level since 1989, it appeared to me from their presentation that they have more or less just gotten started.

   Most of their results to date are negative, and it appears that they are confident that they are going to get the expected (that is, null) results. Of the possible excess heat events that they have observed so far, they have been able to rule out all but one as being due to known (non-anomalous) causes. Neutron emission is mostly not observed, but they have one event at 3.5 sigma of excess neutron emission (23 ± 7 neutrons/sec) recorded over nine hours from one PdD cell after 20 hours of electrolysis.

   Although their results to date have little impact on the field, should they continue, their contributions could be and should be substantial in time. One thing that this group would be able to do which few other groups in the field are as well suited to do to bring on board the physics community. Positive results obtained at the KEK would stir interest in other physics laboratories as almost no other result. The physicists have written off Pons and Fleischmann, so they are free to ignore the claim of kilowatt/cm^3 reported at the conference; but if the KEK gets 10% heat power at 10 Watts/cm^3, I would bet that every physics lab on the planet will likely be pulling out their electrochemistry sets again.

8. T. P. Perng of National Tsing Hua University in Hsinchu, Taiwan described observations of excess power from molten salt electrolysis experiments as part of a paper on heavy water Pons-Fleischmann experiments.[28] Although I did not recall seeing it, and I have no notes of it, numerous friends at the conference mentioned it to me (including Liaw). I also received a preprint of this work.

   Liaw and coworkers at the U. of Hawaii described at the last two international cold fusion conferences experiments using molten salt electrolysis with Pd and Ti anodes in a LiCl-KCl eutectic saturated with LiD. At Como, the group reported the observation of excess power at a level of about 10 times the input electrochemical power in Pd (up to a 30% increase over electrochemical plus heater power), with an energy gain reported as about 1 GJ/mole Pd or 6 MJ/mole D_2. The Pd anode volume was 0.040 cm^3, so that the excess power per unit anode volume is about 250 watts/cm^3.

   This result was important because the power excess was so large relative to the electrochemical input power, and because the temperature excess was on the order of a hundred degrees centigrade, which would have the potential for efficient energy extraction.

   Little progress has so far been reported toward a reproduction of the Liaw experiment. Perng described results from an experiment performed to provide a confirmation of the Liaw experiment; the power excess claimed was on the order of 2-5 times the input

power.

The preprint[29] from C. M. Wan et al consists of an abstract and copies of 11 figures, from which I will attempt to give an account of the work. Following Liaw, the palladium electrode is used as an anode (instead of a cathode as is done in conventional Pons-Fleischmann experiments), immersed in a KCl-LiCl eutectic saturated with LiD. The anode dimensions are 6 mm diameter and 5 cm length. The molten salt sits in an aluminum container which serves as the cathode. The temperature is sensed using a thermocouple embedded in a quartz tube which is placed in the molten salt in the general vicinity of the Pd anode. A Ni-Cr alloy resistive wire heater (encased in quartz) is wrapped around the cell, within a ceramic fiber insulator. Nearby is a ^{3}He neutron detector with a 0.01% detection efficiency.

From the figures and the abstract, it is clear that a time-dependent excess neutron signal appears following 200 hours of electrolysis, at twice background (background is 5.51 $\pm$ 0.44 cpm) corresponding to 800 neutrons/sec. This neutron signal is rather clearly correlated with the excess power production which is time-dependent and rises to about 10 watts. Given the large anode volume, this level of excess power corresponds to about 7 watts/cm^3. The associated temperature excursions are about 25° C, with one excursion up to 50° C.

The abstract quotes power gains of 5 to 108 for the 6 mm Pd rod which is 5 cm long, and power gains of 8 to 560 from a 4.5 mm diameter rod. These numbers are very high and represent excesses in comparison with electrochemical power rather than total input power; I think that the highest numbers correspond to modest excess powers observed at low input current level.

There was an abstract from the National Tsing Hua University by Yuan et al that described a molten salt experiment;[30] I do not know whether this paper was presented. I suspect that this paper may not have been presented, and that Perng was reviewing results obtained by his colleagues.

9. There were several papers on attempts at replicating the Takahashi experiment that captured the attention of the Japanese press earlier this year. A. Takahashi described earlier this year obtaining tremendous excess heat in a heavy water electrolysis experiment that ran at an average of 1.7 output power over input power for about two months.[31,32] The total excess enthalpy generated was claimed to be about 2250 MJ/mole Pd (more than 20 KeV per Pd atom), which is one of the highest claims to date from this type of experiment. Following Takahashi's announcement, many laboratories attempted a replication.

Takahashi's experiment is similar in many ways to the classical Pons-Fleischmann experiments with some variations. A Pd foil from Tanaka Kikinziku Kyogo (Tanaka Precious Metals Co.) with dimensions 2.5 cm $\times$ 2.5 cm $\times$ 1 mm is used for a cathode instead of a rod. The electrolyte volume is very large (700 cc of D_2O with 0.3 M LiOD). An innovation of Takahashi is the use of a time-varying current which alternates between

a high mode (4-5 amps) and a low mode (0.2-0.4 amps) every six hours.

Takahasi's calorimeter is an open cell flow calorimeter, where water from a chiller is flowed through a coil inside the cell, and the power is determined from a knowledge of the mass flow rate and the input-output temperature difference. An advantage of this type of calorimeter design is that it is able to function at near constant temperature when high power is applied to the electrochemistry. The temperature was monitored using teflon coated thermocouples at the inlet, outlet, and cell interior. The cell was calibrated before and after the run in the initial experiment, and the calibration lines were approximately reproduced.

The total input energy for the initial experiment was 250 MJ, the total output energy measured was 410 MJ, leading to an excess of 160 MJ. Takahashi's excess power level claimed was 32 watts averaged over two months, with excursions to 100-130 watts. The cathode volume is 0.625 cm^3 (0.0706 moles), so that the average power density is 51 watts/cm^3 and peak excursions are 160-208 watts/cm^3. As discussed above, a chemical explanation of the effect (barring other systematic errors) is defeated in less than half an hour at the high excess power levels.

Attempts at replication had varying degrees of success, but no one has been able to reproduce the very high power levels claimed by Takahashi. In the reproduction which Takahashi reported at Nagoya, the excess average power was 8 watts, with excursions to 15 watts.

Takahashi's method comes with the recommendation that it is a potentially technically easier experiment than other experiments which have been reported. It is less exacting in the rigorous electrochemical purities required as compared to the SRI experiments; the cathodes from Tanaka metals are readily available to workers in the field, in contrast to the Johnson-Matthey cathodes employed by Pons and Fleischmann. The system is in principle relatively cheap to set up; Mallove[33] described a version of the Takahashi experiment which was built up using about $10K of hardware funds.

Storms at LANL is claiming excess heat from a Tanaka batch 1 foil, and no heat from a batch 2 foil.[34] The anomalous power from the first foil was more than 20%. The batch 2 foil suffered an increase in internal volume on loading, which Storms suggested might be used as an indicator of whether a cathode was suitable for heat experiments. No one has reported a success with a batch 2 foil that I am aware -- batch 3 appears to be free of this problem.

Celani[35,36] described efforts to reproduce the Takahashi experiment at his laboratory in Frascati. Experiments were run in an open cell flow calorimeter using two Tanaka metals batch 1 cathodes, one Tanaka batch 2 cathode, and one IMRA batch 1 cathode. Positive results were obtained with the Tanaka batch 1 rods (at 8% and 25% peak power excess) and with the IMRA rod (12% peak excess). No excess power was observed with the Tanaka batch 2 cathode. Blank experiments were performed where a gold cathode sheet was substituted for the Pd cathodes, and no excess was observed. A correlation between

high loading and excess heat was noted; small amounts of excess tritium were reported for the runs which gave excess heat.

Oyama[37] reported a 2.4% excess energy, which is small, but was measured with much smaller error bars; a light water blank showed no excess.

10. Tritium production was discussed by several groups. The existence of such an effect is interesting because it constitutes an additional signature of the presence of a nuclear phenomenon; tritium cannot be made chemically. An additional feature of many tritium experiments is that the tritium is not accompanied by neutron emission (neutron/tritium ratios of 10^{-7} - 10^{-9} have been reported). The *dd*-fusion reactions would produce neutrons and tritons in roughly equal amounts, so that the observations imply either a new mechanism or else a very significant modification of the fusion reactions. Possibly more significant is that 14 MeV neutrons from *d-t* fusion reactions would be expected from secondary reactions if the tritium nuclei were created with MeV-level kinetic energy. The very low neutron to tritium ratios claimed imply a very low triton energy (below 10-15 keV), sufficiently low to be inconsistent with all but the most exotic reaction mechanisms.

At Como, strong presentations of tritium production were made by Will, Claytor, Lanza, Szpak. Will has not been active in the field during the past year, and did not attend the Nagoya conference; Lanza has continued, but was not able to attend this year; Szpak has continued, but was also not present at Nagoya.

Claytor[38] (whose work impresses me) described further experiments on tritium production in which a stack of alternating layers of palladium and silicon is placed in deuterium gas at over 10 atmospheres, and a pulsed current is passed through the stack. Reproducible tritium production is claimed at levels of 0.02-0.2 nCi/hr (1.1×10^6 - 1.1×10^7 tritium atoms/second). Advances which the LANL group has made during the past year includes: reduction of background tritium, improvement in detection sensitivity, improvement in reproducibility at higher tritium generation rates, and the innovation of working with stacks using Pd built up from powder. Upper limits on neutron emission can be placed from their work as reported at Como of 4×10^{-9} neutrons/second; the ^{3}He neutron detector which they have used (which I saw during a recent visit there) has a roughly similar efficiency for 14 MeV neutrons as for 2.45 MeV neutrons (the detector is more sensitive at 2.45 by a factor of 1.5 according to Menlove).

Bockris[25] described two tritium experiments at Nagoya. In one experiment, a reproduction of the Szpak-Boss experiment described at Como was attempted. Szpak codeposited Pd on either a Cu or Ni substrate from $PdCl_2$ in D_2O containing 0.3 N LiCl; continued electrolysis resulted in tritium production. The Pd was observed to plate out during the first 6-8 hours, and excess tritium would be detected about 10 hours later. The experiment Bockris reported involved codeposition of Pd on gold, and tritium production was observed to start as soon as 10 hours after the Pd deposition, and production up to 3 times background was observed. This is the first successful confirmation of the Szpak experiment of which I am aware. Bockris did not obtain the high degree of

reproducibility claimed by Szpak.

I note that Miles also described[24] attempts to reproduce the Szpak experiment, and reported the observation of a modest tritium increase, but "not clearly beyond levels expected for electrolytic enrichment due to isotopic separation factors."

Much more spectacular are the results reported by Bockris of Chien's experiments on tritium production in a Pd electrolysis experiment where massive amounts of tritium (more than 10^{15} atoms) were observed. This report is very significant because it represents a new claim for very high levels of excess tritium.

Early on, reports of very high levels of tritium were reported to have been observed at Texas A&M, corresponding to production rates on the order of 10^{10} tritium atoms/cm^3 or higher. These experiments were clouded by charges of fraud (this charge was apparently investigated by a panel hired by Texas A&M, and not substantiated), and by the observation of high levels of tritium contamination in Pd claimed by Wolf (found by dissolving Pd rods in acid, and then performing scintillation counting on a neutralized version of the resulting solution). Similar experiments reported by Cedzynska (differing in that the distillate of the solution was analyzed by scintillation counting) failed to show contamination at the high levels reported by Wolf, and in addition found that false positives could occur when the solution was not first distilled.

Subsequently, much more stringent controls were done to attempt to defeat the insidious tritium contamination claimed by Wolf, and new post-Wolf experiments were reported in which anomalous tritium production has been claimed. Except for experiments reported by BARC, most new claims have involved tritium production rates many orders of magnitude below those of the initial claims (the new claims generally ranged from 10^4 - 10^6 tritium atoms/second). The significance of the Chien experiments is that the tritium production rate claimed by Chien (in a post-Wolf experiment) are some of the first to approach the very high early (pre-Wolf) experimental claims. Much care was taken to avoid possible contamination: samples from the same rod were dissolved in aqua-regia, and the resulting solution distilled, and then analyzed using a scintillation cocktail following the method described by Cedzynska at Como.

Chien's earlier experiment was carried out at the Institute of Nuclear Energy Research in Lung-Tan, Taiwan. The palladium cathodes used were 1.0 cm in diameter, and 1-2 cm long; Pt wire wrapped around at a distance of 4 mm was used for the anode. Electrolysis was carried out in heavy water with 0.1 M LiOD; tritium assay was done with a scintillation cocktail. Solutions exhibiting high tritium activity were sampled at the time of the experiment (10/89), and then resampled 10 months later (8/90) in order to observe the tritium decay from the sample. Tritium generation rates of 10^6-10^9 atoms/second were determined, lasting for a total of 20-30 days. The numbers claimed by Bockris at Nagoya[25] for the Texas A&M version of the experiment correspond to about 10^7 atoms/sec/cm^2 of surface area, in experiments with a 3-6 cm^2 surface area.

11. V. A. Romodanov gave an oral presentation in the theory panel at Nagoya.[39]

Romodanov's command of the English language was imperfect; he read from his paper for more than 20 minutes in a thick Russian accent. Essentially no one with whom I talked understood the point of what he said, and his abstract did not particularly add to the information content. Given that his talk occurred in the theory section, and given that his theory appeared to be largely classical fusion modified somewhat by lattice effects, no one was expecting that a major experimental result was buried in his presentation. Two things about his talk raised flags for me, indicating that I should try to follow up if possible. One was that he was from Luch, which is the same place Kucherov is from...and I was very impressed by Kucherov's results. The second thing that I recall was that there was a table giving some very high tritium numbers; at the time I thought they were theoretical estimates because they were so large.

Romodanov handed me a preprint[40] which explained in rather clearer English what was the content of his talk. I will focus on what I consider to be the single most important part of his presentation, which if true, is of fundamental importance. Romodanov described the results of glow discharge experiments which appear to have been done on a system very similar to that discussed by Kucherov (see above in this review). Romodanov and his colleagues focused on the detection of tritium produced in glow discharge experiments in Pd and in other metals.

The glow discharge was run in deuterium gas at 100-200 torr, with an applied voltage in the range of 40-125 V, and a current of 3-4 A (a wide range of operating conditions are described in the paper, and the numbers I have chosen appear on one table -- I am not completely certain from the paper that the tritium generation was done with these parameters). Various cathode metals were used, including Y, Mo, Nb, Er, Ta, and W; as disks with a diameter of 13 cm and a thickness between 500 mu and 1 mm, or rods of 0.5-2 cm diameter. The cathode temperatures were measured to be between 970° K and 1670° K, with only minor (15% or less due to anomalous self-heating effects).

Tritium generation rates between 10^5 atoms/second and 10^9 atoms/second were measured in the different metals under various conditions. The largest rate (1.7×10^9) was obtained in Nb at 1170° K, corresponding to an increase in tritium activity in the deuterium gas of 2.3×10^4. The neutron emission was measured in these experiments with a "radiac instrument RUP-1," which appears to be a scintillator with silver activated ZnS dispersed in transparent plastic (sounds similar to the detector used by Kucherov), and a neutron to tritium ratio of 1.8×10^{-7} was obtained.

12. R. Notoya from Hokkaido University brought a light water demo that was set up and operated in the hallway of the conference[41]. The demo consisted of two cells: in one cell was a resistive heater, and in the other cell was a nickel cathode immersed in a light water K_2CO_3 electrolyte, similar to the method of R. Mills and colleagues.[42] Notoya's method differs from Mill's method in that (1) the Notoya cathode is made of porous nickel, and the Mills cathode is plain nickel; and (2) Mills uses an intermittent current, while Notoya uses a constant current.

The resistive heater was driven at 2.1 Watts electrical *iv* input; the electrolysis cell was

driven so that the joule heating in the cell was also 2.1 Watts. The *iv* input into the electrolysis cell is actually higher by about 30%, but since electrolysis is occurring with a Faradaic efficiency near unity, the power ending up inside the cell is matched as long as no recombination occurs in the space above the electrolyte. The live cell ran higher by about 15° centigrade than the blank, as could be inspected visually by observing alcohol thermometers immersed in both cells. Notoya claims that the light water cell temperature implies a factor of about 3 more net power input, or roughly 6 watts of heating present.

This was interesting for a number of reasons. This was the first live demonstration of excess heat production at a cold fusion conference that I am aware of. I have always thought that live demos would start to show up at conferences and at presentations, but I had figured that the first ones would be heavy water demos. I thought that it was significant that Notoya's system works well enough for her to be willing to bring it as a demo at a major international conference.

Many of the "established" workers in the field who have put in substantial effort on heavy water Pons-Fleischmann cells and have observed heat simply do not believe that a heat effect can be observed in light water. Among other arguments that can be heard is that if the effect is either nuclear or is fusion, it must involve deuterium. Others in the field argue that the light water claims are simply due to sloppy experimental work. Independent of the correctness of the various assertions, it is almost humorous to find senior members of the cold fusion community sounding very much like their critics and tormentors of 1989.

The first reports of heat from light water experiments were actually from Pons and Fleischmann early on in 1989; when I last spoke with Fleischmann about light water experiments about a year ago, he was firm in his conviction that it was not possible to get excess heat from a light water cell.

R. Mills, who is the originator of this particular Ni/K_2CO_3 experiment, has no previous reputation or standing as an electrochemist, calorimetrist or physicist. He rejects the notion of cold fusion as due to nuclear effects completely (in fact, he does not wish to associate himself with the cold fusion community, and does not consider his effect to be related in any way to cold fusion), and has developed his own theory as to why his experiment works; his theory is based on the proposed existence of orbitals of hydrogen that lie below the 1*s* level. An explicit assumption in the Mills and Farrell theory is that the electronic charge distribution in hydrogenic states consist of charged shells of infinitesimal thickness. In order for this proposal to be correct, quantum mechanics must be incorrect (which Mills believes -- he offers his theory as a replacement for quantum mechanics). There have been no observations of such states, and the existence of such states would likely not be consistent with the observed stability of atoms as atoms.

Reproductions of the Mills experiment have been reported previously. Noninski[43] published positive results from his experiments; Noninski views his experiment as a verification of the work of Fleischmann and Pons, who state explicitly (in a 1989 patent application) the possibility of excess heat in a light water cell with Ni as a cathode

material. Mills was apparently unaware of Fleischmann and Pons patent application and its relevance.

Confirmations of the Mills light water experiment have also been reported by Srinivasan,[44,45] and Bush and Eagleton.[46] Notoya and her laboratory come with good reputations; her confirmation of the Mills experiment (complete with demo) is probably the most significant endorsement of the light water excess heat results standing.

Notoya's demo is an open cell system. It operates at a sufficiently high excess power that recombination or other effects that would make an open cell system perform differently from a closed cell would not change the essential result even if the recombination and other secondary effects were taken into account incorrectly or ignored. You can put your finger on the tubes Notoya's demo to convince yourself that a very significant temperature difference occurs. This was also claimed for the Mills experiment, as well as for other experiments reproducing the Mills result.

During the conference and afterwards, a virtual firestorm of controversy arose concerning the difference in wires that were attached to the live cell and to the blank. A student of Steve Jones suggested that since smaller diameter wires were used on the blank, that the reduced voltage drop across the resister could account for the difference. After the conference, Notoya replaced the offending wires, and reported essentially no difference in the resulting blank temperature.

Notoya's demo was brought to the US and set up at MIT during the first week of December. During the day and a half before her presentation at MIT, the live cell and resistive blank ran at very nearly identical temperatures, consistent with no excess power production. Notoya attributed this to contamination of the nickel cathodes. After her visit, she returned to Japan and set up her demo in a laboratory in Tokyo where excess heat was observed. A few days later, she was back at MIT, attempting for a second time to demonstrate excess power production. During this visit, a temperature excess was seen during electrolysis of the second cathode tried. According to Notoya, the initial temperature differential in this case corresponded to a 100% power excess. Subsequently, a persistent excess of about 4° was observed, which she said corresponded to a 30% power excess (the reduction in fractional excess was attributed by Notoya to contamination).

The persistent excess power which Notoya obtained at MIT was about 0.75 Watts, and the cathode volume was about 0.05 cm^3, leading to a volume averaged excess of 15 Watts/cm^3. At Nagoya, the cell ran at a 4 Watt excess, corresponding to 80 Watts/cm^3. She claims that she has observed a maximum of 200 Watts/cm^3 excess. A few hours of operation at 15 Watts/cm^3 is sufficient to defeat a chemical explanation, which was done at MIT. The power excess demonstrated at Nagoya would defeat a chemical explanation in tens of minutes, and the cell ran for many hours.

Little is known about loading ratios (H/Ni) while heat is produced; no information is available about potassium loading in the Ni; there is apparently an alkaline intermetallic

layer formed which is at least several hundred Angstroms thick which may play a role. Nothing is known about the temperature sensitivity of the effect; Notoya observes the excess power to be essentially linear in applied current down to her lowest values (50-100 mA, and about 1 cm^2 geometric area at MIT; the high current levels approach 1 amp).

Notoya obtains her best results with cathodes which have an extremely high area ratio (real area to geometric area), and she uses cathodes with an area ratio of several thousand (and a reduced density of about 6 gm/cm^3). The effect is apparently extremely sensitive to contamination, especially to oils. She observes an increase of 20% in calcium concentration (near 20 ppm) in the electrolyte, which she believes may be anomalous.

I do not think that there is yet any particular contradiction between the light water experiments of the Mills type and the light water blanks in Pons-Fleischmann experiments. The light water blanks in Pd/H experiments run in a H_2O/LiOH electrolyte give zero excess power in most everyone's blank experiments these days; the Mills experiment uses a Ni cathode with a H_2O/K_2CO_3 electrolyte. These are really very different systems. In any event, in time any connections between the two systems will be clarified.

Based on Notoya's work, the evidence in support of a light water effect has improved significantly. The effect which she observes is so great that there appears to be no simple explanation for it.

So is there a light water heat effect? At this point, I am not yet sure one way or another. On the plus side: (1) the effect is large, (2) looks to be nuclear given the excess heat numbers, and (3) can be reproduced. On the minus side (from my point of view): (1) the effect has been studied by a relatively small number of groups for a relatively short time, (2) the effect appears to be somewhat insensitive to choice of electrolyte (claims[41] of heat production have been made for experiments which have used other alkali-carbonates such as Li_2CO_3, Na_2CO_3 and Rb_2CO_3) and to some degree the choice of cathode (positive results were reported[47] for Ni, Ag, Au and Sn electrodes). The reason which I am uncomfortable with the insensitivity of the effect to cathode and electrolyte comes from potential difficulties associated with finding a reaction mechanism that would show such an insensitivity.

The experimentalists have grown used to the idea that deuterium gives anomalies and hydrogen does not; the theorists who believe in fusion mechanisms are comfortable with positive effects in deuterium and negative effects in hydrogen. A light water heat effect causes consternation in both camps; it would be exceedingly difficult to reconcile with a fusion mechanism.

The neutron transfer model which I have been looking at (described briefly below) needs a neutron donor (usually deuterium) and an acceptor nucleus, and therefore has somewhat fewer constraints; nevertheless, I do not relish the prospect of attempting to explain an apparently general light water heat effect where the nuclei present are widely different from one cell to another. An experimental determination (and confirmation) of the ashes

in any of these experiments would of course greatly improve the situation.

As a result, I am not yet sure that there is a light water effect. I will be surer one way or another when more confirmations (or non-confirmations) are reported. I will be surer when Notoya, who has worked on her experiments only since last August, has had more time to think about her experiments and to improve them. I will also be surer in time after the cold fusion community has had more time to study and to evaluate the experiment.

13. The successful production of significant excess energy must give rise to ashes of one sort or another. It is not currently known what reactions are occurring; consequently, it is not obvious what ashes are to be expected. Energy excesses in the range of 1 MJ to 10 MJ have been reported in several experiments; we will consider briefly the implications of excess power generation, both per joule and for a 10 MJ total excess.

Conventional *dd*-fusion reactions producing 10 MJ would yield more than 10^{19} neutrons, and a roughly equal number of tritium atoms. Pons and Fleischmann's recent measurements[48] of the neutrons produced from their cells yield 5-50 neutrons per joule, low by more than 10 orders of magnitude from what would be predicted for conventional *dd*-fusion. Tritium is not produced in their experiments, with a limit which is probably on the order of 10^4 tritium atoms per joule; low by at least 8 orders of magnitude.

It has been suggested that the ^{4}He branch of the *dd*-fusion reaction is somehow favored, and several searches for ^{4}He have been made. The conventional ^{4}He branch yields a 24 MeV gamma, which is not observed when heat is produced. The reaction energy would have to go elsewhere to be qualitatively consistent, and many in the field believe that energy transfer to the lattice occurs. Many measurements have been performed seeking ^{4}He in the cathode after the experiment; my impression is that it is simply not there quantitatively by many orders of magnitude.

There have been some efforts seeking ^{4}He in the gas stream produced during electrolysis; Miles focused the attention of the community on this issue last year at Como when he claimed the observation of ^{4}He which at its highest levels might account for roughly 10% of the excess energy. Scaramuzzi and De Ninno[11,12] described a new cell, calorimeter plus helium detector with which they plan to attempt a confirmation; other groups are acquiring mass detection capability for similar studies. The measurements of Yamaguchi[22,23] described above has also raised interest in helium detection.

I would think that by next year's conference, that there will be a consensus by many groups established on whether substantial helium is produced or not.

If the pragmatic point of view is adopted that whatever reaction is occurring is not constrained by theoretical preconceptions, then the search for the ashes is generalized considerably to include possible isotopic shifts or anomalies, and the possible production of elements or isotopes not initially present.

A large number of studies have been reported at the international conferences in which

the cathode surface has been analyzed for the presence of trace elements. Due to the nature of electrochemical deposition in real systems, quite a long list of surface contaminants are found at significant levels, hopelessly complicating any straightforward *ab initio* experimental search.

The number of nuclei which is sought is on the order of 10^{12} per joule (or 10^{19} per MJ), which either helps or hurts depending on the point of view. Present in large quantities are D, O, Pd, Li, and H. Determining a relative isotope shift between deuterium and hydrogen is generally deemed not to be feasible, given the presence of hydrogen as a ubiquitous universal contaminant. Isotope shifts in oxygen are not currently predicted by anyone in the field, and have never been studied in Pons-Fleischmann experiments to my knowledge.

The first serious claim of possible isotope shifts in heavy elements with which I am familiar was made by Rollison at the NSF/EPRI workshop in 1989 (Rollison subsequently had to back down from her claim -- see the proceedings of the Salt Lake City conference). The glow discharge observations described by Kucherov[13-16] and the Chinese group[17-20] imply isotope shifts in Pd and other metals.

The production of 10 MJ of energy in a Pd cathode (containing 0.1 mole of Pd) would give rise to modifications of the Pd isotope distributions (assuming Pd were fuel) at the 0.0002 level, assuming arbitrarily 5 MeV per reaction. The prospect of proving this experimentally if it is in the bulk is judged to be impractical. If the reactions occur near the surface, then the numbers improve; the "noise" associated with natural isotopic separation also increases. Searches for such surface isotope shifts have been reported, and continue to be performed; such searches for now remain in the background of the field.

Lithium appears to be required for heat production in Pons-Fleischmann experiments, although it is unknown whether it plays any nuclear role.

Thompson, formerly of Johnson-Matthey, reported[49] that the lithium on the surface of a Pons-Fleischmann cathode that had been involved in heat-producing experiments showed a depletion of ^{6}Li relative to the natural abundance (down to about 4%). Pons and Fleischmann had reported (in the Salt Lake proceedings) that the lithium which they had used was initially enriched in ^{6}Li (11%); Thompson noted this in his talk, quoting an initial concentration of 9-11%, but said the Johnson-Matthey group did not have a before to compare with their after.

During the questions following Thompson's talk, McKubre noted that the Johnson-Matthey analysis only looked at the surface, and that any lithium present in the bulk might provide an internal reference. Thompson said that he thought that this suggestion could be tested. The conference proceedings from Johnson-Matthey states that no lithium is detected in the bulk,[50] which would imply that it will not be possible to establish an internal reference retrospectively.

The amount of lithium present in a cathode is an interesting question. Gozzi reported last

year the results of studies to determine Li loading in Pd during electrolysis, and found the very high number of 5% by monitoring the Li lost from the electrolyte. I questioned him at the conference (he presented[50] some nice positive results from his torus of cells where he monitors for heat, neutrons, tritium, helium, and I think gammas; unfortunately, I am lacking sufficient documentation of his results to present more details in this review), since several papers presented at each of the international conferences showed per cent level surface concentrations which fall rapidly on the micron scale into the bulk of the cathode (an exception to this was the measurements presented by Nakada[51] et al showing lithium profiles with significant lithium in 20-30 microns). Lithium concentrations were measured by Myamoto et al,[52,53] who obtain Li/Pd ratios between 3×10^{-4} and 3×10^{-3}. I suspect that the Li/Pd ratio is probably sensitive to cathode properties, to the electrochemistry and loading time; one possible explanation of the long loading time required for Pons-Fleischmann cells, and remarked on explicitly by McKubre,[2] was that extra time beyond the deuterium loading time required to see the heat effect might be due to a necessity to achieve significant loading of another species, such as lithium or other light interstitials.

I note here that energy production at the level of 100 MJ/mol would yield an observable (2%) isotope shift in lithium if the lithium concentration were at the 1% level, and if the bulk lithium did not substitute with lithium in the electrolyte. The numbers are worse if the electrolyte lithium is included, but not so bad to prohibit a measurement.

Unfortunately, very few groups are currently pursuing the lithium isotope shift problem; I consider it to be an important question, especially in light of the initial Johnson-Matthey positive measurement.

14. B. Stella[54] presented a poster that I passed by twice; the title talks about the "stimulated emission of neutrons," that is of course impossible -- neutrons are fermions, and can of course not participate in stimulated emission. The third time by, Stella grabbed me and walked me through his poster (for which I am thankful, otherwise I would have missed it).

In essence what the experiment consists of is taking a Pons-Fleischmann cell, putting it inside a 40 % efficient neutron detector underground in the Gran Sasso INFN laboratory, and directing an incident neutron beam (of about 30 neutrons/sec) with a substantial thermal component at the cell. Fast (2.45 MeV) neutrons are measured originating from the cell, and a gain of about 2 fast neutrons for every incident neutron is claimed.

After talking to him, I was given to understand that for 30 neutrons/sec input that 60 or more neutrons/sec were measured (taking into account the neutron detector efficiency). I asked if the ratio held up at a higher input flux, and he said that they had done experiments up at 500/sec incident, with the same basic neutron gain (but that their neutron detector suffered from saturation problems at such a high flux).

I asked whether the effect was reproducible. Stella said that they had done two runs (each run takes about a week to do) so far, and that they hoped to be able to do some more in

the near future.

I note that a neutron gain of 2 would be a very important result, if true, with rather important implications. I note also that this report of two observations (with a modest signal relative to noise) is the first in which such an effect has been claimed, and that no reproductions from either this group or any other group has been made.

Negative Results

1. A famous hydrogen-in-metals physicist, Y. Fukai, gave a presentation of the basic problems facing theorists attempting to provide an explanation of the anomalies.[55,56] I thought that this talk was excellent, and Fukai is really very knowledgeable; it is clear that the basic physics issues (the Coulomb barrier, screening, and solid state issues) that he discussed must be addressed theoretically, especially in the case of theories based on fusion reaction mechanisms.

   Fukai also presented a negative result[57] involving a search for neutrons that might be induced as a result of the generation of fractures in metal deuterides. The highest result observed was for fractured TiD, 1.8 ± 0.1 cpm, versus a background of 1.27 ± 0.05 cpm.

   I was impressed that he attended, since I am convinced that mainline scientists of his caliber will play an increasingly important role in the field. After talking to him, it became clear that some scientists present who were not in the field had received very strong encouragement to participate.

   His talk was not well received by a number of those in the field, and he was criticized during and after his presentation.

2. There was a negative result on a measurement of neutrons from a Pons-Fleischmann cell originating out of the physics department at Osaka University,[58] the poster of which looked very interesting. The group had a high-resolution Ge detector looking at gammas produced inelastically by neutrons impinging on an Fe plate placed between the detector and a Pd heavy water electrolysis cell. With this system, the group was able to place an upper limit of 1.6×10^{-24} fusions/dd-pair.

   There was another negative result on the fusion rate as determined by the relative absence of 3 MeV protons reported by a group from Tokyo Metropolitan University.[59] I did not see this poster. According to the abstract, the upper limit on the fusion rate from this measurement was 1.3×10^{-24} fusions/dd-pair.

3. D. Morrison of CERN presented a paper[60] that criticized the experiments in the field, and used data from an analysis of the literature published in the field to show that interest in the field is declining, symptomatic of "pathological science".

Morrison made a number of arguments, most of which are restated and amplified in his recent review[61] of the Nagoya conference (his Cold Fusion Update No. 7 on the computer network; my copy is dated 12-17-92). I will attempt here to summarize briefly what I think are key points (drawing from his conclusions listed in Update No. 7), and to provide some commentary on the points.

The major point of Morrison's presentation involves the inconsistency between the claimed excess heat production, which would correspond to on the order of 10^{12} reactions per second, and the low tritium and neutron signals which are lower by many orders of magnitude. For example, Morrison uses the upper limit on neutron emission in the Kamiokande experiments (10^{-4} neutrons/sec) to place an upper limit on heat production which is lower by 16 orders of magnitude. Morrison also noted that the mean distance between deuterons in the lattice is larger than for D_2, which implies that fusion reaction rates in a lattice would be expected to be smaller than the very low numbers which are well-known for D_2.

A weakness of the limiting argument as stated by Morrison is the presupposition that conventional *dd*-fusion is the operative reaction mechanism; it has long been recognized by many (but not all) in the field that the excess heat production can not be due to conventional *dd*-fusion. I personally would accept Morrison's limit on the heat production for the Kamiokande cells, specifically for the amount of excess heat due to the conventional *dd*-fusion channel in those experiments. Possibly more relevant for a limit on the *dd*-fusion channel would be a neutron measurement in a heat-producing cell (there is no report of any calorimetric excess heat production at Kamiokande), in which case the limit claimed in other experiments is higher, but the basic argument is unchanged. This issue has been discussed above.

Morrison advocates that due to the wide range of phenomena claimed (some inconsistent with others), and since “poorly designed and artifact-prone” experiments have been reported in the field, that only “good fully-instrumented and fully-calibrated experiments that need few and unimportant corrections” should be done; loading should always be measured.

The claims of positive heat results in light water experiments appears to be inconsistent with previous claims of heat production (in which the effect was present in heavy water, and absent in light water). This issue was commented on above.

Morrison points out that many negative results have been obtained which contradict the claims of Pons and Fleischmann, and those of Jones. Morrison points to the work of the GE group (R. H. Wilson et al, *J. Electroanal. Chem.* **332**, 1 (1992)), as well as a large number of other experiments, in which no excess heat was observed in contrast to the claims of Pons and Fleischmann. He points to the Kamiokande experiment as the strongest refutation of the original claims of neutron emission by Jones and coworkers.

This argument was presented by Morrison in a spirited fashion at the conference. He made use of the statistics of papers published for and against, seemingly as a route to help

to decide whether an effect exists or not. For example, of 727 refereed published papers in the compilation of D. Britz, in each category of effect, there are more negative results than positive results. Of experimental papers in this set, there appear 86 positive, 136 null, and 36 indecisive or contradictory.

Of the 8 experimental papers published in 1992 included in the Britz compilation, 1 was positive, 6 nulls, 1 indecisive; of papers on proton measurements, 1 positive, and 11 nulls; of papers on ^{3}He, 1 positive and 8 nulls; of papers on x-ray emission, 0 positives and 7 nulls. The subset which Britz rates as being expert yields the results as: 1 positive, 19 null, 2 unclear and 6 technical; of those looking for artifacts: 1 positive, 14 null, 2 unclear, and 1 technical.

At issue in Morrison's discussion is whether there occur, or do not occur anomalies (heat, particles, etc.) in deuterated metal systems. Taking a vote by counting the number of published papers pro or con is certainly one way of deciding the issue; most others at the conference who argued for or against presented the results of an experiment or else the results of a theoretical model.

Morrison went further at the conference; he used the results to support his contention that interest in the field is dying out (experimental papers: 72 in 1989, 128 in 1990, 48 in 1991 and 8 in 1992), which he said was symptomatic of "pathological science". Although Morrison has written about the field as an example of pathological science *Special Symposium Proceedings on Cold Fusion* of the World Hydrogen Energy Conference, July 1990, p. 233), and he discussed pathological science at Nagoya, I did not see an elaboration of his arguments in his review.

Whether the excess heat effect is real or not is a matter that either has been, or else will be, settled by experiment; not by counting papers or by discussing pathological science. Nevertheless, there is an issue buried in Morrison's arguments which is of interest. The issue of reproducibility is central in the field, especially given the early history and associated problems. The dark clouds which currently hang over the field today would likely not be present had the experiments been easier to reproduce in 1989. While the degree of reproducibility of the heat effect among groups working in the field has improved considerably since 1989, it is true that not very many examples exist where an outside effort has come back recently armed with the latest results and has attempted a replication. This situation needs to be addressed in the coming months and years.

4. J. Huizenga of the University of Rochester submitted a post deadline abstract[62] that pointed out that there are two types of claims, one for heat and one for low levels of neutrons. Huizenga maintains that there is no evidence to support any relation between the two claims. The claims of fusion products at a level down by twelve orders of magnitude from the heat production do not support the notion that the heat is of nuclear origin.

   Although Huizenga was present, this paper was not presented.

Theory Papers

All but four of the theory papers were presented as posters during two sessions where I had posters to attend; consequently I was unable to spend much time looking over the theory papers of others. I will nevertheless attempt a summary of some of the approaches of the work based both on the abstracts and on what I have seen of the approaches previously. Once again, I warn the reader that my review of the theory papers in the field are biased by my own point of view as to what physical mechanisms are responsible for the effects being observed.

The theories may initially be divided up into two general categories; those involving (modified) fusion mechanisms, and those not involving fusion mechanisms. Papers considering fusion mechanisms face the two basic problems of (1) arranging to get nuclei close enough together to fuse, and (2) possibly modifying the fusion reaction profiles. We first consider papers describing theories based on fusion mechanisms.

1. G. Preparata[63] has been working on theory for coherent *dd*-fusion reactions; a major goal of the theory is to account for the heat production by a modified *dd*-fusion reaction where the ^{4}He branch dominates, and the gamma emission is replaced by energy transfer with the lattice. He argues that a proper quantization of the low energy electromagnetic field coupled to the metal electrons leads to enhanced screening between deuterons. He then proposes that the ^{4}He branch is favored by coherence factors that come about when the reaction energy (24 MeV) is transferred to the lattice.

   In some sense, this is a version of the "classical" cold fusion model, which would be essentially forced somehow to be true if Fleischmann's initial conjecture that the effect was due to fusion were accepted. I consider this general type of model to be essentially the only game in town if it is assumed (following Fleischmann's initial conjecture) that the reaction mechanism must be fusion. I spent 6 months working on it myself in 1989.

   However, none of this makes the fundamental problems associated with screening and modification of reaction pathways any easier to solve.

2. There have been a number of speculative theories that have been based on the notion that deuterons in a metal are well-described using Bloch-type wavefunctions. In such case, the principal interaction of the deuteron is assumed to be with the lattice, and deuteron-deuteron correlation effects would be brought in at higher order. A computation of the *dd*-fusion rate using uncorrelated orbitals yields anomalously high fusion rates, as expected since it operationally leaves out the Gamow factor.

   It has been suggested that the inclusion of the deuteron-deuteron correlation terms might not lead to Gamow factors as low as in the well-known case of molecular D_2. S. Chubb and T. Chubb[64,65] have recently turned to the problem of electron correlation in ground state helium as an example where orbital and correlation effects compete, and argue that the Hylleraas solutions show an unexpected degree of overlap between the two electrons.

Multi-body fusion theories have been proposed,[66,67] that would ultimately require deuteron-deuteron correlation to be essentially absent altogether to operate. It is not clear how this could come about.

3. The possibility that anomalously large electron screening might occur is the subject of a number of works presented at the conference.[68-71] The basic idea is that if the coulomb repulsion between deuterons held in neighboring sites was reduced, then the degree of overlap of the nuclei would be increased, leading to a possibly measurable fusion rate. The difficulty here is to arrange for an enormous enhancement (of some unexpected sort) of screening in the metal beyond what screening occurs in D_2.

4. Fast (multi-KeV) deuterons are able to overcome the coulomb barrier sufficiently to fuse with a low but observable probability. There have been suggestions that conventional mechanisms exist that could accelerate enough deuterons fast enough to account for low levels of neutron emission that have been reported. This explanation follows from the known phenomenon in insulators that hundred eV ions are emitted from insulators that undergo intense fractures; the corresponding effect is much weaker in metals by several orders of magnitude.

   Theories which propose anomalous ion acceleration in metal hydrides were described in a number of abstracts.[72-75] A variant on this general approach is discussed by Fukushima,[76] wherein recent observations of conditions in sono-luminescence experiments are proposed to result in an enhancement of the fusion rate.

5. Kim and coworkers examine screening effects and modifications of the deuteron velocity distribution function that may occur at high density.[77-79] This approach is applicable both to cold fusion and to hot fusion problems; the authors believe that it may provide a solution to the solar neutrino problem. The fusion rate may be higher or lower than the conventionally calculated rate, depending on the condensed matter environment. Kim believes that these effects may also help to account for the anomalous branching ratio in cold fusion.

6. A low energy resonance in the D+D system would enhance the fusion rate at low energy (no such resonance is known theoretically or experimentally). An abstract was submitted describing a proposed novel “combined resonance tunneling” effect,[80] that was not explained in the abstract.

7. The catalysis of fusion by a heavy negatively charged particle, extending the essence of muon-catalyzed fusion, was made popular in 1989 by Rafelski and others. An abstract on catalysis by an anti-diquark with -4/3 charge was submitted.[81] From my perspective, this general approach suffers from the absence of abundant known massive negatively charged nuclear particles, and a reason why they should be appearing specifically in Pons-Fleischmann type experiments.

8. V. A. Tsarev of the Lebedev Institute in Moscow described some calculations suggesting

that an increase in the tunneling probability between deuterons would be expected due to lattice motion (my translation of "violation of stationarity in lattice").[82] Rather than the conventional kinetic or screening arguments often described, Tsarev proposed that the lattice would provide a time-dependent potential that would affect the deuteron wavefunction itself.

I cannot see how there would be any but the weakest of effects from such terms; in time more documentation of this approach will hopefully be available, and the essence of the proposal will become clearer.

Tsarev presented an interesting review of cold fusion research in Russia and in neighboring countries formerly of the Soviet Union. I do not have sufficient documentation (unfortunately) of his presentation to include a section in this review. I note that the Russian work was reviewed last year in an article by Tsarev and Worledge.[83]

A number of theorists, including myself, have gone away from fusion reaction mechanisms. The motivation for this is to avoid the coulomb barrier (if possible) and to find reactions with signatures that hopefully more closely match the experimental observations. Each new non-fusion approach carries with it specific problems and issues that are associated with the specific reaction mechanism. Aside from this, any new approach must also arrange itself to be consistent with physical law, observations in this and other fields, and must presumably be functioning in a manner not previously expected (lest it would have been found earlier). We describe such contributions below.

1. Electron capture on a deuteron would lead to two virtual neutrons; if it could be arranged for the virtual neutrons to be in proximity with neighboring nuclei, then further reactions could occur. This approach was described in two abstracts by J. Yang of the Dept. of Physics, Hunan Normal University of China.[84,85] Yang proposes that the two neutrons form a stable dineutron that reacts with deuterium to make tritium and a free neutron, and with ^{105}Pd to make ^{106}Pd and a free neutron.

   I consider this general approach to be one of the basic non-fusion approaches that actually begins to try to address the coulomb barrier problem. Once the electron capture occurs, the coulomb barrier is gone, potentially leading to the possibility of something happening near room temperature. One difficulty involved in this approach are that the electron capture is mediated by the weak interaction, which really is very weak, making it hard to obtain significant reaction rates. A second difficulty is that virtual neutrons do not generally wander more than fermis away from their point of origin, making it difficult for a virtual neutron to reach another nucleus to interact.

2. Direct lattice-induced neutron ionization was described by Tani and Kobayashi,[86] motivated by the broad neutron emission that has been observed by several laboratories at energies higher than 2.45 MeV.

   The possibility that sufficient energy may be transferred from the lattice to a deuteron to

disintegrate it is yet another significant conceptual step away from working with fusion reaction mechanisms. If a mechanism existed to do what Tani and Kobayashi proposes, the resulting spectra would likely follow the photodisintegration cross section generally qualitatively in shape, which would not be such a bad match to Takahashi's data.

Once, I suspected that a single-step lattice-induced disintegration, something like what is described in this abstract, might be possible; I followed it up with a moderately sophisticated calculation (based on a harmonic lattice, without including some of the effects described in the abstract) that has been accepted for publication. The results of my computation were that although it is possible in principle to transfer sufficient energy to do the job, the energy transfer is sensitive to sign; in the end, I concluded that single-step lattice-induced disintegration could not be done (within the limits of my model), without having individual nuclei with MeV-level kinetic energy in the lattice initially to do the ionizing.

3. I submitted two abstracts on neutron transfer reaction mechanisms that I have been exploring recently.[87-89] The basic reaction in this theory is a two-step transfer reaction of a neutron from a donor nucleus (typically a deuteron) to an acceptor nucleus located Angstroms to microns away. As originally proposed, the lattice would contribute the energy to promote the neutron from the donor, and take up energy at the acceptor; calculations showed that this was not viable, and so a modified version of the model in under development.

   The revised model works similarly, except that the intermediate state is virtual, as required since the lattice is unable to contribute energy to ionize the neutron. When the neutron reaches the acceptor nucleus, then a number of incoherent processes could occur, including gamma capture, and capture to states that decay by alpha emission. There might be a correlation between these decay products and the reaction products observed by Kucherov. Alpha particles in this model would range up to 4.1 MeV (originating from neutron capture on ^{105}Pd).

   Heat production might be accounted for if a long-lived metastable state existed that was nearly resonant with the virtual neutron, and which alpha decayed.

   If the capture at the acceptor is preceded by energy transfer to the lattice during the donor transfer (which has now been shown explicitly to be allowed at least mathematically), or during scattering of the intermediate state virtual neutron, then the coherent neutron capture proceeds into long-lived ground state nuclei, which are born essentially at rest. This mechanism could account for heat production (accepting onto light interstitials such as ^{6}Li or ^{10}B) and anomalous slow tritium production (accepting onto deuterium).

   The primary difficulty with any reaction mechanism that involves a virtual free particle is that such intermediate particles do not go very far (typically fermis) from where they are born. I presented the results of computations of the virtual neutron Green's function including lattice effects,[88] and found that under conditions that phase coherence among neighboring hydrogen isotopes is maintained in a periodic lattice, that a usefully large

and long-range contribution to the Green's function may occur that would lead to observably large net reaction rates. Quantum diffusion is conjectured to be able to set up the required coherence.

One weakness of the approach which has become apparent following the conference is that the diffusion of hydrogen in metals generally proceeds by a hopping mechanism, which would likely not establish phase coherence of the sort required by the theory. In a loaded PdD lattice, some population of the tetrahedral sites would be expected; this is of interest since the tetrahedral to tetrahedral site barrier is expected to be considerably lower than the octahedral to octahedral site barrier, which might help the situation. The issue of coherence for such a diffusion mechanism is under study.

What Was Not Presented

In spite of the relatively numerous set of papers that were presented at ICCF3, there were several key players in the field who were not present or did not give papers. I felt that the conference suffered from the absence of K. Wolf, H. Menlove, E. Storms, E. Cecil, F. Will, S. Szpak, F. Lanza, and several other key players in the field. Additionally, a paper from G. Chambers (of NRL) that I had hoped to see was withdrawn by order of the associate director of NRL.

Possibly controversial was the absence of a presentation by Ishida of experiments at Kamiokande. During the past year and a half, a very large number of measurements seeking neutrons from various cold fusion experiments were carried out. Kamiokande is famous as one of the world's premier neutrino detection facilities, and received considerable attention following the observation of neutrinos from the 1987 supernova. A positive result of observation of anomalous neutron emission at Kamiokande would be a very big event, since Kamiokande is well-respected in the physics community.

Ishida's master thesis summarizes the results of over 100 cold fusion experiments that were done at Kamiokande. Although it is a fact that neutrons were observed at low levels, there are questions about what is the origin of the neutrons. In the thesis, Ishida proposes that the neutrons are due to naturally occurring radioactive contaminants.

In the end, I think that the results from Kamiokande make either a weak case in support of the existence of anomalous neutron emission, or else a possibly disputable case in support of the non-existence of an effect. This requires further explanation. The emission of neutrons from Ti shavings in deuterium gas was reported early on by Scaramuzzi from Frascati. Attempts to replicate the experiment met with success at LANL, where both random and large bursts of neutrons have been observed with high efficiency ^{3}He neutron detectors. Low level random emission of neutrons is claimed, and bursts of up to several hundred neutrons in a 100 mu sec period were observed. The reproducibility of these experiments is not great, and in spite of the progress made at LANL in improving the reproducibility of the effect, the success rate reported

in the Como proceedings was about 10%.

Menlove worked with the Japanese team to attempt a confirmation of the LANL results. Due to the constraints imposed by the nature of the facility, the number of runs which were attempted on Menlove's samples were 6. According to Menlove, one of these samples might have shown something. If a case were then to be made that the Kamiokande results disprove the Menlove's observations at LANL, this argument is at best weak, since the probability of obtaining a null result is on the order of $(0.9)^6 = 0.53$ for these experiments.

Some have made the case that since no very large bursts (~ 100 neutrons) were observed in any of the more than 100 experiments (which would improve the statistics), that this refutes Menlove's positive observations of large burst obtained at LANL and reported at Como. Lacking from this argument is an estimate of expected frequency of bursts from the various experiments that were done. If the expected rate of large neutron bursts were negligible in the Portland cement experiments, for example, then doing many of them should not alter a conclusion regarding a Menlove experiment.

The poor reproducibility of the effect, in addition to the difficulty of determining in a post-analysis what is the difference between a cell that gave a signal and one that did not, prompted Scaramuzzi to recommend at Como that this line of investigation should make way for other approaches which are less frustrating. Research on Frascati cells has largely ceased in the field.

The Kamiokande experiments were discussed in the talk by S. Jones,[90] who was a collaborator in the experiments at Kamiokande. Jones argued that the conjectures made by Ishida about radioactive contaminants had been subsequently tested by introducing the proposed contaminants and measuring the resulting signals elsewhere; the resulting neutron emissions did not agree with the Kamiokande results. Jones therefore described the results as supporting the presence of anomalous neutron emission.

I do not think that we have heard the last of this discussion. I would hope that in the future Kamiokande would try again, perhaps with experiments which have larger signals and higher success rates. For example, the experiment described by Kucherov would yield signals up to eight or nine orders of magnitude above background at Kamiokande if there were any way to field it there.

Also absent from the conference were prominent US skeptics who have in one way or another have made technical contributions to the field in the past. The absence of such individuals indicates the lack of any significant respect that the cold fusion field currently has among the scientific community. I would have been interested in the response of such skeptics following many of the papers presented at the conference; but alas, it was not to be. At the conference, little in the way of substantive technical criticism of the best heavy water calorimetry results was offered by any of the participants. If there exist skeptics who are familiar with the Pons and Fleischmann calorimetry or the SRI work and believe that they know what might be technically in error, your technical input would be greatly appreciated.

Conclusions And Discussion

Was the conference sufficiently strong technically to turn the tide, to settle the seemingly endless controversy as to whether there is or is not any new anomalous effect? I thought that it was. I regard the technical issue of whether there is a reproducible anomalous excess power effect in heavy water Pons-Fleischmann experiments to have been settled at this point; I think that there is clearly an effect.

For such a significant conference, it has been largely ignored by the scientific community. Wrongly so, I think. The majority of scientists are currently ill-informed of the experiments, the implications, the arguments, or the goals of ongoing research in the field. At some point this needs to change, but I confess that I do not see how it might happen in the foreseeable future.

The name “cold fusion” has been adopted by the field to some degree by default. This name implies a generic physical reaction mechanism (fusion), and because the experiments involve deuterium, the name further presupposes specific reactions (*dd*-fusion reactions). But *dd*-fusion is expected to produce neutrons and tritons, neither of which are quantitatively present with the excess heat. Scientists who are not in the field are discouraged because the expected fusion products are not present in quantities commensurate with the observed energy production, and scientists working in the field have not come up with an explanation in three and a half years as to why deuterons should fuse that is acceptable to the scientific community.

There have been proposals to change the name of the field: “solid state nuclear physics” has been suggested; “nuclear effects in metals” has also been put forth. I would strongly endorse a name change.

A reviewer of this manuscript has pointed out that even these names presuppose a nuclear component to the effect, which in the reviewer's eyes remains to be demonstrated, and has recommended “hydrogen energy” or “hydrogen in metals”, with the understanding that “hydrogen” is to include the isotopes.

The field continues to receive considerable bad press, which at this point is not warranted. For example, I have recently obtained a copy of a review of the Nagoya conference by D. O. Morrison, which has received very wide distribution; it is unfortunate that the only updates about the field received by most of the physics community is through such a biased channel. This simply must change. I am open to suggestions as to how this situation could best be changed.

There are precious few sources of potential funding in this area, especially in the US. I am convinced that DOE should be funding a significant effort in the US, the goal of which should be to find out what is going on, so that an informed and rational judgement can be made

about any potential of the effect to meet US and world energy requirements. One basic claim that has been made is that excess energy at a level which must be nuclear (but is certainly not conventional *dd*-fusion) is observed in the Pons-Fleischmann experiment and variants; this is something that the DOE should be interested in.

So what is it that should be done? The list is very long, but I will attempt to enumerate some of what I think are a few relevant goals:

1. Verification of a heat effect: I am convinced that the Pons-Fleischmann cells can produce excess energy of a nuclear origin based on the amount of energy per atom evolved. The scientific community does not accept this. This issue really needs to be put to rest, and the associated controversy ended.

   Pons and Fleischmann have been publishing further details of their own work in refereed journals and in readily available conference proceedings, and more papers are currently in the pipeline. Details of the work of many other groups is also readily available.

   Considerably more is known about the Pons-Fleischmann cells than in 1989, and the reproducibility of the effect has been improved considerably. SRI has produced documentation of criterion which, if met, carries the guarantee that similar experiments at SRI have produced heat reproducibly with a very high success rate. Palladium cathodes from sources other than Matthey-Johnson have now shown the effect.

   Significant deficiencies have been identified in the principal negative experiments which were done in 1989; the main criticisms of these experiments was that a high loading was not achieved and held. For example, the method developed at SRI requires very high loading (D/Pd ratio near 0.90) to be maintained for about a week. Since positive results have been obtained at lower loadings, this constraint is likely not to be absolute; nevertheless, many in the field believe that quite high loadings do improve the reproducibility of the effect.

   I do not know how this controversy is to be ended, but I know that it does need to be ended in a satisfactory manner. The basic experiments have been done, they have been repeated in many different ways by numerous groups, and the effect is observed with considerably better signal to noise ratio than in 1989.

   Scientists in the field have gone to extremes in attempts to satisfy skeptics. Cells were stirred, blanks were done, extremely elaborate closed cell calorimeters have been developed (in which the effect has been demonstrated), the signal to noise ratio has been improved so that positive results can now be claimed at the 50 sigma level, the reproducibility issue has been laid to rest; but still it is not enough. I have heard some skeptics saying that a commercial product is the next hurdle to be jumped through before any significant funding can be justified. This is simply not right.

2. Basic reaction mechanisms for heat production: To date the claims of the observation of heat anomalies in metal deuterides have not been accompanied by any clear positive

evidence for reaction mechanisms. Anomalous heat generation would have to have a fuel, and would have to have ashes; the confirmed identification of either fuel or ashes would help tremendously towards a determination of a reaction mechanism.

I think that progress in this field is hindered by the absence of even a rudimentary understanding of the basic reaction mechanisms involved (there are of course theories, but to date there is no positive experimental confirmation of any proposed theory). At some point, the principal experimentalists in the field simply must take this issue seriously. Having an understanding of what the reaction mechanisms are would provide numerous benefits: (1) guidance as to what experimental parameters are expected to be important for optimizing reaction rates; (2) improvement of the general quality of the science being done in the field, especially as perceived by those not in the field; (3) allows those working in the field to focus more clearly on the issues that are most important. From the point of view of funders or potential funders, a knowledge of how the effect works allows the possibility of assessing more accurately potential future applications.

The determination of fuel and ashes requires high sustained volume-averaged heat production. In the case of ^{4}He production, an assay of the gas stream is required; in the case of assays for other elements and isotopes, careful mass spectroscopy (and the presence of a small electrolyte volume) will likely prove to be most important.

3. Verification and reaction mechanisms for other anomalies: Quite a few anomalies have by now been associated with deuterium in metals experiments, including observations of neutrons, gammas, fast ions, tritium, and helium production.

   None of these effects are currently accepted by the scientific community; as in the case of the heat effect, some way is needed to arrange for a consensus as to which of the effects are real. It would seem to me that the most dramatic claims come from the glow discharge experiments; most significant would be if these experiments could be further reproduced and verified.

   I think that experiments which produce energetic (MeV-level) nuclear products provide essential information relevant to the issue of reaction mechanisms. For example, a confirmation of significant isotope shifts and strong gamma emission from heavy elements would place very strong constraints on proposed reaction mechanisms. A detailed study of precisely which gamma lines are produced would likely shed light on how the gamma lines are excited, which provides further input on reaction mechanisms.

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