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The Status of "Cold Fusion"

David J. Nagel

Condensed Matter and Radiation Sciences Division

February 17, 1998

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THE STATUS OF "COLD FUSION"

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ABSTRACT

The questions raised by reports of nuclear reactions at low energies, so called "cold fusion", are not yet answered to the satisfaction of many scientists. Further experimental investigations of these and related questions seems desirable, at least for scientific if not practical reasons. Properly conducted, such investigations would be indistinguishable from normal research. They would yield information germane to accepted areas of scientific inquiry and technological utility.

1. INTRODUCTION

The announcement on 23 March 1989 by Pons and Fleischmann that they had achieved power generation from nuclear reactions at ordinary temperature had a rapid and enormous impact. About six weeks later, the cover stories of three major popular news magazines in the U. S. were on "cold fusion". The response to the prospect of easy and inexhaustible energy, maybe with little residual radiation, was comparable to the public reaction to Roentgen's report of x-rays in 1895. Then it was thought that privacy would no longer be possible.

The strength of the "cold fusion" surprise had two bases. One was the strong knowledge, on the part of physicists, that high energy beams (or equivalently, high temperature plasmas, with their associated high particle velocities) are needed to force nuclei into contact, a prerequisite for their reaction. Physicists had worked hard for four decades, and spent billions of dollars, in only partially successful efforts to produce and contain the multi-million degree plasmas needed to get significant energy out of nuclear fusion. Despite the major progress on heating fusion plasmas, and on overcoming many instabilities which tend to destroy plasma containment, three milestones remain to make fusion energy useful. They are (a) getting out as much energy as it takes to create the plasma, so-called "scientific break-even", (b) getting out energy equal to the total it takes to run the reactors, or "energy break-even" and (c) realizing enough energy to be economically viable, that is, to overcome thermodynamic inefficiencies and other losses. The primary fusion research reactors in 1989 in Japan, the Soviet Union, the U. K. and the U. S. each cost hundreds of millions of dollars to build and operate. So, it was no wonder that a report of nuclear fusion reactions in a relatively simple and inexpensive apparatus at room temperature was a shock.

The second reason for the strong reaction was the fact that the work had been done by two chemists. The situation was compounded by their method of announcement, that is, a press conference before the work was ready for publication. Later, it became known that they felt to be in competition

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with another scientist (a physicist!) in a nearby university (Taubes, 1993). An irrational polarization between physicists and chemists has plagued the study of "cold fusion."

The situation in the middle of 1989 was very challenging solely on scientific grounds. Careful experiments designed and executed by competent scientists with sensitive and calibrated equipment, using well analyzed and controlled materials and chemicals, plus thorough associated instrumental and data analyses, were clearly needed. Such experiments are expensive and take time. However, the situation deteriorated as that year progressed due to mistakes and unnecessarily-rapid judgments by some scientists, organizations, agencies and editors.

The fundamental functions of individual scientists are quite few and clear. From a *proactive* viewpoint, by following their interests and the needs of their sponsors, they produce new knowledge and communicate what they learn. From a *reactive* viewpoint, scientists evaluate the work of others and respond to the needs of those who pay them. Throughout 1989 and the years since the initial announcement, due to haste or other reasons, many individual scientists made serious mistakes in the conduct and reporting of their work, as well as in their evaluations. And, the field attracted several people who appeared to be scientists, although they did not have sound credentials or, worse, had a history of problems.

Some major laboratories appear to have been too hasty, possibly because of extraneous pressures. Negative reports from a few of them during 1989 were thought to be authoritative, although they were subsequently called into question. The need for adequate time and money to perform and thoroughly check difficult and complex experiments, even at large and well-equipped laboratories, seemed to be forgotten during the summer of 1989.

In the U. S., a committee set up by the Department of Energy issued a negative report on "cold fusion" in November of 1989, even though the situation then was still very fluid. The committee may not have gotten adequate access to then-current information because of secrecy. Many organizations were being very cautious because of commercial or security concerns. The report served to squelch funding for "cold fusion", at least in the U. S. By the end of 1989, "cold fusion" was already largely discredited.

Journal editors, with their pivotal role in scientific communication, also contributed to the difficulties. One year after the initial press announcement, an editor of Nature magazine (Lindley, 1990) wrote a summary of the situation under the title "The Embarrassment of Cold Fusion". He ended by asking "Would a measure of unrestrained mockery, even a little unqualified vituperation, have speeded cold fusion's demise?" The impact of such an editorial is impossible to assess, but it might not have encouraged dispassionate consideration of "cold fusion". Also, that editorial predated many of the primary findings, patents and publications on the subject. Only a few journals consistently published papers on "cold fusion". They include two physics journals, Physics Letters A and the Japanese Journal of Applied Physics, and a journal which provides a bridge between physics and engineering, namely Fusion Technology. The Journal of Electroanalytical Chemistry, mainly read by chemists, has also published many articles on "cold fusion". These journals have relatively small circulations, and they are but four out of over 5000 scientific journals.

The situation was not entirely negative because numerous careful scientists and organizations pursued thoughtful and methodical programs. However, the abundance of mistakes, especially a few very public retractions of positive results, discredited the field. There resulted, and still remain, two important disconnects between the hundreds of people yet actively working on or monitoring "cold fusion" and significant other groups. First, the relative dearth of normal scientific literature broke down communications between "cold fusion" workers and both other scientists and those who fund research. Second, the eventual unavailability of reports in newspapers and magazines severed communications with the public. A plot of the column-inches of material on "cold fusion" in popular magazines and in newspapers would show a great decline in the past few years. New publications devoted to "cold fusion" and related topics, newsletters and internet web sites failed to compensate for the breakdown in normal communications. Now, most scientists and the public believe that "cold fusion" is dead as a research area; they seem to think that all of the reports of nuclear reactions at low temperatures are wrong.

The question addressed here is indeed: has "cold fusion" gone away? We begin to answer by briefly surveying the character of the international conferences on the topic, along with the available patent and scientific literatures, in the next section . Whatever the status of "cold fusion", the possible outcomes for it and other new areas of science are few and certain; they will be stated and discussed in the following section. Then two sections will describe frameworks for examining the experimental and theoretical aspects of "cold fusion". Only a very limited summary of some major reports will be provided. A plausibility argument for the lack of reproducibility in "cold fusion" experiments is offered. Before the conclusion, a few less controversial, but possibly-related current scientific puzzles will be mentioned. The paper will end by noting the connection between work on "cold fusion" and an area of research and development which is both old and widely respected, and by listing potential approaches to the subject.

The term "cold fusion" is used merely as a label for the subject in this paper. The current evidence indicates that ordinary fusion cannot explain the reported effects. Nuclear reactions, more generally, and multiple mechanisms are not yet excluded. So, the term "cold fusion" might be wrong, even if nuclear reactions can occur in condensed matter at low energies. Alternative terms include "anomalous nuclear effects", "anomalous effects in deuterated systems", "low-energy nuclear reactions", "low-energy nuclear transformations", "chemically-assisted nuclear reactions" and, in Japan, "new hydrogen energy".

The isotopes of hydrogen will be labeled in this paper as follows: H for the proton or hydrogen atom, D for the deuteron or deuterium atom and T for the triton or tritium atom. Most "cold fusion" experiments have involved D_2O, although many have employed H_2O. Tritium is more often reported as a product than used as a reactant.

2. ACTIVITIES AND DOCUMENTATION

International Conferences on Cold Fusion

ICCF	Location		Year
1	Salt Lake City	Utah	1990
2	Como	Italy	1991
3	Nagoya	Japan	1992
4	Maui	Hawaii	1993
5	Monte Carlo	Monaco	1995
6	Lake Toya	Japan	1996
7	Vancouver	Canada	1998

There have been several conferences dedicated to "cold fusion", most of them in a series of international meetings or else in the former Soviet Union. The International Conferences on Cold Fusion follow a North America-Europe-Japan cycle. The seventh conference in this series is scheduled for Vancouver Canada in April of 1998. Each of the past ICCF conferences attracted two to three hundred participants. The quality of the presentations has generally increased with time. Attendance by reporters from the broadcast media, newspapers and magazines has declined steadily, from a strong presence at the first two meetings to very little or no attendance at the last two conferences. The meetings in the former Soviet Union near Moscow (1993), in Minsk (1994) and four times in Sochi near the Black Sea (1994, 1995, 1996 and 1997) have been relatively small. The conferences in 1995 and 1996 in College Station Texas on "Low-Energy Nuclear Reactions" were also much smaller than the ICCF.

In addition to these conferences explicitly on "cold fusion" or closely-related topics, there were a few other individual conferences on the subject. Some routine scientific society meetings also had sessions on the topic. The published conference proceedings are excellent sources for scientific and technical details on the topic. A useful list of 22 "cold fusion" conference proceedings was compiled by Britz and is available on the internet (see Webliography).

Another part of the web site just cited lists 15 books, 214 patents or patent applications, and 1115 papers on "cold fusion" as of 27 June 1997. The list of books includes most of the volumes specifically on the topic. The single-author books vary widely in their quality and views. Some are very negative (Huizenga, 1992) and others are thoroughly positive (Mallove, 1991) regarding claims of nuclear reactions at low energies. Most address the historical aspects of the field, rather than specific scientific details.

The patent situation is very mixed. The U. S. Patent and Trademark Office has granted patents for only a few of the over-200 invention disclosures it reportedly has received on "cold fusion". Patent offices elsewhere appear to have ranged from similarly unresponsive to substantially more receptive. Examination of the 214 patents and applications cited by Britz shows that Japan has issued 120 patents. The worldwide patent history derived from the data of Britz is:

Year:	1989	1990	1991	1992	1993	1994	1995
Patents:	51	33	10	21	25	8	1

These data would indicate that "cold fusion" died as an area of innovation after about six years.

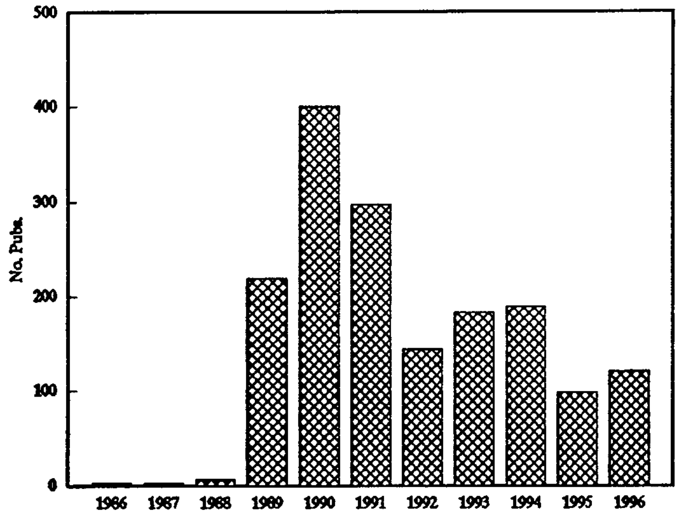

Figure 1. Annual number of "cold fusion" publications *vs* year of publication.

Publication activity is a flawed way to study the character, and even activity level of a field, but it does show when a topic emerges, and then either grows or declines. The annual history of publications on "cold fusion" can be examined for both the rate of publication and its variation with time. Figure 1 shows the number of papers reputed to be on "cold fusion" as a function of year. This presentation was obtained by searching Chemical Abstracts using the phrase "cold fusion". The total number of papers obtained from this process was 1691, with a small fraction of them predating 1989. This compares with 1115 papers in the Britz bibliography. Figure 1, like much else regarding "cold fusion", is open to alternative interpretations. It shows that the level of activity on "cold fusion" may be either declining unsteadily, or else fluctuating, but holding very roughly constant after the very active first three years. Either way, the figure indicates that publication activity in the field is still around 25% of the peak rate of 400 papers in 1990.

The decline in funding and activities regarding "cold fusion", and in journal, magazine and news coverage, has contributed to a decline in criticism of reports on the subject. Unfortunately, people actively working on the subject have tended to be less critical, probably because there was significant (and possible sufficient) early criticism; they apparently do not want to contribute further problems to the field. Most critics lost interest after a few years. Since the earliest days of the field, Morrison has remained engaged and been a critic *par excellence*. His article, which cited "three miracles" required for "cold fusion" of deuterium to be real, remains one of the most challenging critiques of the field. He focused attention on (a) the D-D separation, (b) the lack of nuclear ash commensurate with the excess heat and (c) the ratios of nuclear products, notably neutrons and tritons (Morrison, 1994a).

3. POSSIBLE OUTCOMES OF "COLD FUSION"

Scientific topics will turn out to be either right or wrong, sooner or later. This is the case regardless of how they arose and the number of mistakes made in their pursuit. If a topic or theory has not yet been accepted to be either correct or incorrect, usually within some limits, then it has an intermediate and transient status. Figure 2 indicates this situation schematically.

There are many cases of observations and ideas which turned out to be wrong. Polywater and infinite dilution memory effects are examples from the recent past. So also is the earlier case of N-rays. And, there are numerous instances of radical assertions, some contrary to then-accepted ideas, which have turned out to be correct. Continental drift and gravity waves, the effects of which have been observed in astrophysical cases but not yet on earth, are but two fairly recent examples.

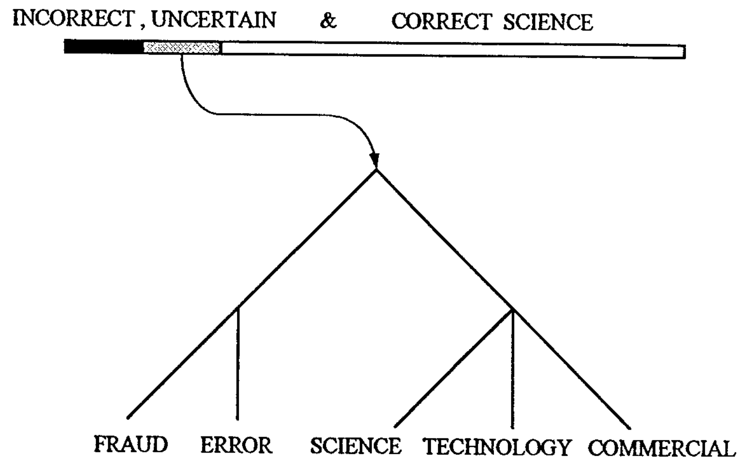

Figure 2. Possible dispositions for the reports of, and questions raised by "cold fusion".

At any time in history, there are several reports or ideas in science which are not accepted as either right or wrong. The effects on humans of low-level exposure to ionizing or electromagnetic radiation are in this category. It is difficult to obtain statistically significant observations of small radiation-induced effects in the presence of many physical and physiological variables. The reported observation of a magnetic monopole several years ago was not duplicated, even though the credentials and methods of the scientist were judged to be sound by many other scientists. At present, "cold fusion" is thought by many competent scientists to be in the indeterminate category.

The second figure also includes a branching diagram which contains the possible outcomes for the enigma of "cold fusion". Either there is nothing to the subject, or there is something new involved after all. In the first case, the reports of statistically-significant measurements by numerous groups with adequate equipment and calibrations, and diverse controls and null tests, must *all* be ascribed to either fraud or error. Many people have difficulty with the notion that most of the reported results were fabricated, especially in cases where similar results are seen by different groups. Sequential lies or a conspiracy to lie would be indicated. These possibilities seem unlikely. Similarly, many observers cannot ascribe all of the larger observations of heat, reaction products or radiation to mistakes on part of individuals, or, especially, by teams of investigators. A psychologist would point to the possibility of a group mentality, maybe due to the influence of a strong leader or to wishful thinking. However, that something like a group mentality is possible does not make it likely in all cases. The great majority of scientists and the interested public apparently believes that all of over 100 reports of anomalous effects in "cold fusion" experiments are simply wrong. Most of this is probably due to a combination of preconceptions and acceptance of comments by others, rather than personal examination of the available relevant information.

If there really is something new in the reported "cold fusion" results, it is at least of scientific significance. That is, it would broaden our knowledge of the physical world. It may not produce a technology, something that works. If it does yield a new capability, that technology may be of little use to anyone; or, it might be useful to limited groups such as the military; or, it could be widely important and, hence, commercially viable. The last prospect, especially if energy could be produced without fossil fuels or conventional nuclear power, is what drove both the initial reaction and continues to motivate some of the interest in the topic of "cold fusion".

4. EXPERIMENTAL SUMMARY

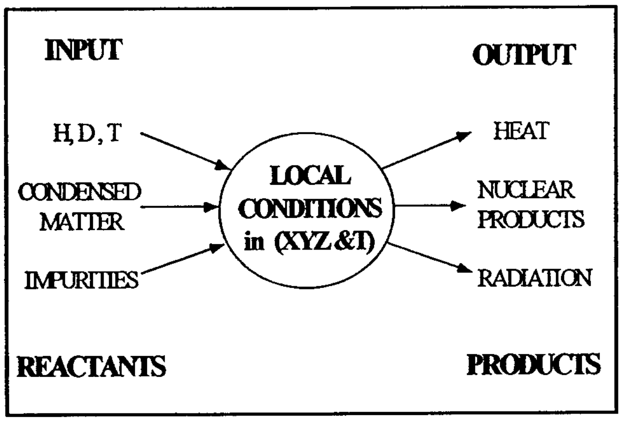

Figure 3. General diagram for physical, chemical or other reactions, annotated for "cold fusion" input reactants and output products.

The central question of "cold fusion" was and still is: can nuclear reactions be produced (controllably) at low energies? Addressing the laboratory aspects of this puzzle can be done by considering the general aspects of reactions, as indicated in Figure 3. For a reaction of any kind to occur, the reactants, or something associated with them, have to be brought together in space and time, under the appropriate conditions. In physical terms, wave function overlap is prerequisite to any reaction. Then products can result, with some associated probability. The essential challenge of "cold fusion" is to prepare conditions within a solid material (at ordinary, low temperatures) in a fashion that leads to nuclear reactions. Reported experimental work on "cold fusion" can be placed in a matrix where the "loading" methods which bring input materials together under the "right" conditions form one axis, and the types of products measured are arrayed along the other axis, as indicated schematically in Figure 4.

The juxtaposition of hydrogen or its isotopes in condensed matter has been accomplished by many procedures which fall into three categories. The first, in terms of the energetics involved, is thermodynamic loading of protons or deuterons into any of several metals in a pressurized and heated vessel. The second is the method employed by Pons and Fleischmann and most workers in the field, namely electrochemical loading. It can be done using either aqueous or fused-salt

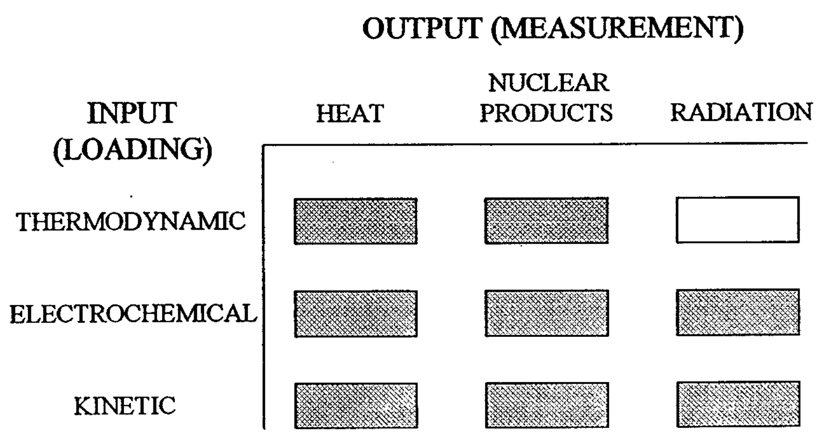

Figure 4. Matrix of loading methods and measured quantities employed in "cold fusion" experiments. Boxes for experiments discussed in the text are shaded.

electrolytes. The effective pressure of ions on the electrode surface is very high, since electrolysis is actually an aggressive process. During loading of D into a Pd cathode, over 10 deuterons can pass through each surface unit cell of the Pd material each second. The third method is kinetic loading in a low-pressure environment, where the protons or deuterons are shot at the material with sufficient energy (above about 10 eV) to penetrate the surface and remain in the material. The energy can be imparted to the particles by acceleration of a beam of ions, by use of a plasma nearby the material or by the collapse of cavitation bubbles on the surface of the material being loaded. Initial surface adsorption, followed by diffusional penetration, may also play an important role in the kinetic loading experiments. Hybrids of these three basic loading schemes can be employed. For example, electrochemical loading of a hollow Pd cathode in one experiment produced the high pressure conditions for what might be thermodynamic loading of Pd powder within the central cavity. Other methods beyond this classification, including the use of explosives and hypervelocity impact, have been considered, but they apparently have not been not studied experimentally.

It is difficult to overemphasize the roles of materials, especially surface conditions, in "cold fusion" experiments. Impurities in the electrolytes or in the nearby atmospheres, on surfaces, and within electrodes and other solid samples seem to play a key role in many experiments. Surface conditions have proved to be central to the ability or inability to load hydrogen isotopes into materials in some of the more careful electrochemical and kinetic loading experiments. Measurements of the detailed and evolving surface conditions during the course of a "cold fusion" experiment are generally difficult, and usually have not been made. If and when "cold fusion" proves to be real and understood, it may be better to organize experimental methods around the surface conditions. Then, the means of bringing together H isotopes and materials might be viewed merely as processes to create the necessary conditions, initially on the surface and then within the appropriate materials.

The products which have been claimed in "cold fusion" experiments fall into three categories. The first is power beyond what is put into the experiment (excess power), or its time integral (excess enthalpy, or heat). This is said by some to be the primary signature of "cold fusion", and has been measured in many experiments by the use of sensitive temperature sensors in calibrated calorimeters. The second is nuclear products, the so-called nuclear "ashes", which are present after, but not before, a nuclear reaction. Examples in this category include tritium, helium and heavier isotopes. Finally, radiation has been reported in many experiments in the field, either as prompt energetic particles (neutrons, tritons, alpha particles or electrons), or photons (x- or gamma-rays), or as residual radioactivity, which also yields particle or photon emission.

It is noteworthy that chemists tended to do electrochemical loading and heat measurements. Most kinetic loading and nuclear product or radiation experiments were done by physicists. Positive results have been reported in all of the nine boxes indicated in figure 4. A few of the provocative results from the shaded boxes will be mentioned in the remainder of this section.

A longer review could exhibit exemplary data. With one exception, we will provide only terse summaries of results selected because they are both extraordinary and difficult to dismiss at present. Illustrated, comprehensive reviews of "cold fusion" reports by Srinivasan (1991) and by Storms (1991 and 1996a) provide much useful information and many references. Here, we primarily cite recent papers, most of which contain references to earlier work by the various groups who have had long-term programs on "cold fusion".

The initial paper by Fleischmann and Pons attracted much criticism. However, it, their other six open literature articles, the dozen conference proceedings papers they published and their international patent application (Pons et al, 1990) merit attention. The long paper published in July of 1990 (Fleischmann et al) and the patent application, which appeared on 20 Sept 1990, include the temperature history of a cell which neared boiling. Pons and Fleischmann sequentially reported increasingly larger values of excess power and energy. At ICCF-3, they showed data from a cell which reached boiling, and reported power densities exceeding 1 kW/cm^3 (Fleischmann and Pons, 1993). At ICCF-4, they stated that in some experiments, heat production continued after a cell boiled dry, the so-called "heat after death" phenomenon (Pons and Fleischman, 1994). In one of their experiments, the 3.27 MJ of excess heat generated corresponds to 29 MJ per mole of Pd, or 300 eV per Pd atom. This value is about 100 times higher than "any conceivable chemical process". At ICCF-6, Pons and his colleagues (Roulette et al, 1996) reported data from one experiment which gave excess powers as high as 101 watts, with 294 MJ of excess heat.

McKubre and his group (1994) have measured excess heat in many experiments. In the extreme case, they obtained an excess heat corresponding to 343 eV for each Pd atom in the system. This, again, is about 100 times higher than what could be explained by chemical processes. That is, burning, or otherwise reacting chemically, the entire unreacted parts of the experiment would not release anywhere near that much energy. The ratio of total output energy to total input energy was 1.032. That value may seem small, but the excess 3.2% corresponded to 0.79 MJ, more that ten times the measurement threshold of the well-calibrated apparatus.

Arata and Zhang (1997), who did the already-mentioned hybrid loading experiment with the hollow Pd cathode filled with Pd powder, obtained excess powers in the 10 to 90 W range. Their integrated excess energy is as high as about 20,000 eV per Pd atom in the powder (about 4 gms). Further, they report that they obtained excess power in all six of their experimental runs.

Preparata (1996) reported strong results from a new "cold fusion" experiment at ICCF-6. The cathode consisted of a Pd wire 50μm in diameter and 250 cm long wound in a large helix (about 20 cm diameter) around the central Pt anode. A voltage was applied along the Pd cathode as well as between the cathode and anode. Preparata reported excess power exceeding 300 W for about one hour. Because of the small cathode volume, the corresponding power density is very large, namely 60 kW/cm^3. The excess energy per atom was calculated to be about 19,000 eV/Pd, a value close to that obtained by Arata & Zhang (1997). This near coincidence is fortuitous. It is based on conservative readings taken from a small published graph.

The excess heat experiments just mentioned were performed by reasonably-funded groups of capable scientists, who published quite detailed papers over the course of a few years. Other instances of excess heat in electrochemical and other careful experiments could be cited. The salient point is that, if heat results quoted above are correct, nuclear processes *must* be involved.

Some other factors, which have emerged from multiple electrochemical "cold fusion" experiments, deserve mention. It was found in experiments by Fleischmann et al (1990) and by McKubre's group (1994) that there is a threshold in current density near 100 mA/cm^2 for excess power, which varies linearly with current density beyond that value. The groups lead by Kunimatsu (1992) and by McKubre (1994) showed that it is necessary for the ratio (= X) of D to Pd atoms in a cathode to exceed a value near 0.83, where the partial molar enthalpy for absorption of D in Pd becomes positive (exothermic). After that, the excess power varies quadratically with the value of (X - 0.83). McKubre and his colleagues also reported that excess power correlates with the time rate of change of X, namely dX/dt. Fleischmann and Pons (1993) discussed "positive feedback", that is, how increased cell temperatures promote excess power generation. Lonchampt et al. (1996), in a reproduction of the Fleischmann and Pons experiments, showed that higher temperatures favor excess power (P_{xs}) production: 0-5% P_{xs} below 70°C, 10% P_{xs} in the range 70-90°C and up to 150% P_{xs} at boiling. Taken together, these reports indicate that certain dynamical and other conditions are prerequisite to, or favorable for the production of excess power. It must be noted that not all of these requirements for production of excess power have held up in some later experiments. However, if "cold fusion" actually turns out to produce energy, these findings may be central to achievement of reproducible and controllable behavior. They would probably also be important for the design and operation of practical engineering applications.

The results of power and heat measurements depend on the calibration of the calorimetric electrochemical cells used in the different experiments. Hence, calibration techniques and related data analyses have been argued energetically in the "cold fusion" literature. Methods employed varied widely, from relatively simple (Miles and Johnson, 1996) to rather complex (Fleischmann et al, 1990). The methods of Fleischmann and Pons have been heavily criticized (Wilson et al, 1992 and Morrison, 1994b), and were defended (Fleischmann and Pons, 1992 and 1994).

Jones et al (1995) argue that Faradaic efficiencies below 100% can account for reports of excess heat in cells with either D_2O (plus Pd and Pt electrodes) or normal water (with Ni and Pt electrodes). They list several potential causes of excess heat, including systematic errors. Then, they show experimentally that cells can produce excess power until barriers to the migration of dissolved H isotopes and oxygen are introduced. The barriers stop the recombination of the H isotopes with oxygen in solution, which represents an apparent source of energy. Whether or not that mechanism could quantitatively explain all excess power reports is unclear. Even if correct in some cases, the mechanism may not explain why certain time and sample variations are seen in most "cold fusion" experiments. Often, the same cell gives no excess power at some times, or for some samples, but shows apparent excess power at later times or for other samples. If the work and analyses of Jones et al incline a person to conservatively set aside all reports of excess heat generation in "cold fusion" experiments, then the reports of nuclear ashes remain to be considered.

Nuclear products have been measured directly in the contents of electrochemical cells, or gases which came from them, after numerous experiments. Tritium and, and especially helium, are the most commonly reported nuclei appearing after "cold fusion" runs.

Trituim production is of more than scientific interest. In the U. S. now, expensive accelerator and reactor methods to produce tritium for stock-piled nuclear weapons are in competition with each other. Tritium was observed by Will and his colleagues (1991) in electrochemical experiments. For values of D/Pd near unity, they observed tritium generation in all four D_2O experiments, and no tritium in any of their four H_2O experiments. Tritium has also been measured in kinetic loading experiments. A team led by Claytor (Tuggle et al 1994) drove D into Pd with a glow discharge in a low-pressure D_2 gas ambient, and measured tritium build up with an ionization chamber in a recirculating system with excellent signal-to-noise ratios. They independently confirmed the appearance of tritium by scintillation measurements of water produced by oxidizing the gas in the experiments. Chambers et al (1991) reported tritions emitted with 5 MeV from an experiment where a low-energy D beam was used to load a Ti foil. Reproducibility was a problem in both of these kinetic-loading experiments for unknown reasons, which probably have to do more with variations in materials than changes in procedures. The absolute levels of tritium reported in "cold fusion" experiments are very small. Whether or not tritium production by "cold fusion" can be reproducibly controlled, and scaled up to values of interest for weapons, might be determined by programs costing a small fraction of current tritium production expenditures.

Helium has been reported by several groups, and a review of the results is available (Bressani, 1996). In general, the 5 ppm of He normally present in air has made it challenging to show that any measured He is not due to diffusion or leaks. However, some of the experimenters have taken great pains to address this problem. Groups in the U. S. (Miles and Johnson, 1996), Europe (Cellucci et al, 1996) and Japan (Arata and Zhang, 1997), among others, have made strong reports of He. Arata and Zhang detected both isotopes of He in the Pd black used in their experiments by two methods, high-resolution mass spectrometry and ionization potential variation spectrometry. They obtain ratios of ^3He to ^4He in the range of 0.1 to 0.5 for five samples, a thousand times the natural value of 0.13×10^{-3} for air. These ratios are very difficult to dismiss, because they are obtained by the same instrument within a single mass scan, free of calibration uncertainties. Arata and Zhang have adequate mass spectral resolution to resolve the HD and ^3He peaks. The two peaks were measured to have the correct mass difference to within an estimated 20%, and no other atomic or molecular interference could mimic the observed peak structure. Pd black that had not been exposed to high pressure D_2 showed no He of either kind when subjected to the same degassing and mass analysis protocol.

George and Stringham (1997) have concentrated on loading metal foils by cavitation bubble collapse. They routinely observe melting of the foils, and the appearance of micro-craters near the edges of the melted region. They also report elevated levels of He, 10 to 1000 times greater than ambient air, and anomalous ratios for its isotopes. George and Stringham measure excess powers of 80 W in some runs of a system which gives energy balance in runs that do not produce excess power. Their heat and helium values tend to correlate.

A quantitative correlation between measured excess heat and the absolute number of nuclei produced is clearly desirable, and has been sought in several laboratories. Miles and Johnson (1996) have reported correlation of heat and helium levels in several experiments. They calculate that the probability of a chance occurrence of their correlations is one in 750,000. Their ratio is 10^{11} to 10^{12} He atoms per second per watt of excess power. This corresponds to between 6 and 60 MeV per helium atom produced. An energy of 24 MeV is released in conventional fusion as a gamma ray when two deuterons form ^4He. Arata and Zhang (1996) obtained a ratio of their absolute heat to He levels near 24 MeV. It is notable that, in standard D-D fusion, the 24 MeV gamma-ray emission branch has a very small probability (10^{-7}), compared to the formation probabilities for tritons (50%) or neutrons (50%). How the production of many MeV per reaction can be as probable as indicated by Miles, Arata and their colleagues, and how the energy can appear in the lattice mainly as heat without gamma-ray emission (which would be easy to measure), are two of the primary riddles of "cold fusion", and two of the greatest challenges to theoreticians.

The observed ratios of neutrons to tritons in "cold fusion" experiments are generally in the range of 10^{-5} to 10^{-7}, rather than unity, as in normal deuterium fusion. Clearly, if these observations are correct, the processes at work are *not* ordinary fusion from both reactant condition (input) and reaction product (output) viewpoints. This is another of the intellectual enticements of "cold fusion", regardless of any potential practicality, which also calls for an explanation.

Prompt and delayed energetic radiation from "cold fusion" experiments have also been reported frequently. There have been many descriptions of neutron emission, but the levels are very low. The most vexing observation of radiation is from the work of Wolfe, shown in Figure 5. The results were presented only by a member of the organization which sponsored the work, a noteworthy departure from the normal practice of a scientist communicating his own research (Passel, 1995). Wolfe reportedly measured the radioactivity in three pieces of Pd after D_2O electrolysis in separate cells. He obtained gamma-ray spectra with strong lines at energies matching *earlier tabulated* values of emission from Ag and other heavy isotopes of elements near Pd. These results indicate the occurrence of either H or D nuclear reactions with Pd or impurities, or initial D-D reactions whose products subsequently involve Pd or other elements in secondary nuclear reactions, leading to the active isotopes associated with the observed gamma-ray peaks.

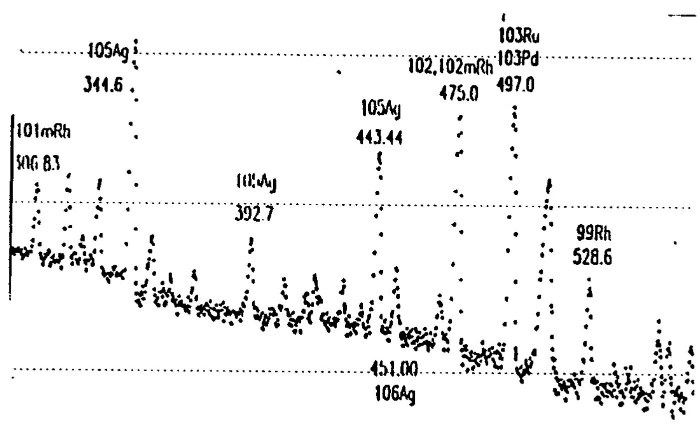

Figure 5. Gamma ray spectrum measured by Wolfe in the range from 295 to 574 keV from Pd activated in a "cold fusion" experiment. Some of the peaks are labeled with their energy and the isotopes which emit radiation at those energies. The vertical axis is the log of the total counts, with dotted lines at values of 10^2, 10^3 and 10^4.

Wolfe's gamma-ray spectra contained about 50 lines in the region from 295 keV to 1412 keV. Although not yet reproduced, they are doubly dramatic. First, the good signal-to-noise ratios and the appearance of gamma-ray lines at known energies are very difficult to dismiss. Second, the data highlights the possibility of nuclear reactions beyond fusion of light isotopes. Of course, the Coulomb barriers for reactions of protons or deuterons with heavier elements are much greater than for reactions involving only H or D, so reactions involving heavier isotopes are even more difficult to explain. The resemblance of such reactions to alchemy has not escaped attention.

A Russian group has used kinetic loading from a glow discharge plasmas of H, D or H and D. They measure all three classes of products, namely heat (Karabut et al., 1995), nuclear products (Savvatimova and Karabut, 1995a) and radiation (Karabut et al., 1992), including radioactivity (Savvatimova and Karabut 1995b). This series of measurements has produced one of the most diverse and extensive data sets of any "cold fusion" experiment. Attempts to reproduce these results have had mixed outcomes.

For most of the time since March of 1989, there have been reports of anomalous effects in systems with electrolytes of "normal" water, which consists of H_2O and D_2O in the ratio of 10^4 to 1. Many people working on the original Pons-Fleischmann systems tended to dismiss the normal water work because of the dominant "light" water component. Their reaction was not much different than the response their work with heavy water elicited from the broader scientific community. The many reports of excess heat production in H_2O-based systems will not be reviewed here. However, one series of experiments has received much attention since ICCF-5 in 1995. The work involves potential destruction of undesirable radioactive isotopes in nuclear waste.

Patterson obtained five U.S. patents related to a closed-loop, normal-water system containing a bed of metal-coated plastic beads, across which a voltage is applied. Outupt to input power ratios have varied greatly, and reproducibility in others' laboratories has not been satisfactory. Miley and Patterson (1996) performed diverse analyses of the coatings after electrolysis. They reported the appearance of Mg, Cr, Fe, Cu and Ag, each at concentrations exceeding 2 atomic per cent, and other trace elements in the Ni coatings of the beads. Most recently, Patterson and others have asserted that the recirculatory system can induce the decay of heavy radioactive materials, notably isotopes of thorium and uranium. This raises the question of whether low-energy nuclear reactions could be employed for nuclear waste remediation, in addition to the production of energy and isotopes. These reports are among the more controversial in the field now, both because of their character, and because they should be susceptible to confident confirmation by other groups, due to the size of the reported effects.

One other variant on the original Fleischmann-Pons approach deserves attention. Only a few people have employed *acidic* H_2O or D_2O electrolytes, in a fascinating contrast to most of the work on "cold fusion", which has involved *basic* solutions. Either approach has it merits. However, it seems very reasonable to use acidic solutions, with a hydrogen isotope concentration many orders of magnitude higher than in a basic solution, for electrolytic experiments designed to drive H or D into a cathode. Will et al (1991) employed acidic solutions in the tritium generation experiments discussed above. Dash and his group used acidic electrolytes, and reported the appearance of high concentrations of the elements in localized regions. They measured S, K, Ca, V, Cr, Fe, Ni and Zn on Ti cathodes (Kopecek and Dash, 1996) and Ag on Pd cathodes (Dash et al, 1994).

It must be emphasized that experiments where elements reportedly disappear (as in the thorium and uranium experiments of Patterson) or appear (e. g., in the experiments of Dash and his colleagues) are particularly worrisome. In the former, it must be proven that the material of interest did not simply get redistributed within the system, possibly to levels below instrumental thresholds or outside of instrumental acceptance solid angles. In the latter, it has to be shown that low densities of initial impurities, below minimum detectable limits before the experiment, did not get concentrated during electrolysis and brought above the thresholds of the microanalytical tools used on the cathodes after experimental runs.

There are instances in observational science where reproducibility and controllability are not possible. For instance, supernovae 1987A was neither, and is not thoroughly understood. Nor, are earthquakes and volcano eruptions, at least at present. However, these two attributes are to be expected from ordinary laboratory experiments. Their absence from most "cold fusion" experiments has been one of the most damning criticisms of the topic. However, there is a scenario which might explain the lack of reproducibility and controllability.

The effects of uncontrolled (sometimes called "hidden") variables has a long history in science and technology. Candidates for such influences on "cold fusion" experiments can be listed. For a particular element, the usually-unmeasured distribution of isotopes is the first possibility. The concentrations of low- and moderate-level impurities constitute a second class of potentially critical variables. And, the arrangements of atoms, the atomic structure, offers many possibilities for unexpected and unknown variations in the physical and chemical conditions in a material. Going beyond such static factors, there are some dynamic variables which could be germane to the outcome of experiments. They include isotopic and elemental redistributions in the matrix, for example, due to diffusion, and the fluxes of electrons and hydrogen isotope nuclei, as well as structural phase transformations. Of all of these, impurities have gotten the most attention.

Impurities could play a role in "cold fusion" experiments in a few different ways. They might create or destroy needed conditions, for example, the attainment of very high ratios of D to Pd in a lattice. Impurities could conceivably promote or retard reaction rates, without participating in the reaction, much as do catalysts or inhibitors for chemical reactions. And, impurities could actually participate in the reactions. The numbers involved in this last possibility are especially interesting. If 1 ppb of an impurity undergoes a nuclear reaction which yields 1 MeV each second, a power of about 1 W is produced. One watt is a nominal excess power in most "cold fusion" experiments which report excess heat. If 10 MeV resulted per reaction, also a normal sort of nuclear reaction energy, correspondingly less material or a lower "reaction" rate would be required, or a higher excess power would result for the same rate. The important point is that few "cold fusion" experiments have been analyzed for impurities to the ppb level in the various phases within the experiments before, during or after runs. Impurity levels in materials generally vary on the ppb level from batch to batch and, even, from sample to sample within a batch of material, or from cm to cm along a wire or rod of material. It is possible that, if there are unknown processes at work in "cold fusion" experiments which involve impurities, the reproducibility and controllability riddles will be explained on the basis of inadequate knowledge and control of impurity levels.

The above impurity argument is another version of the "dead graduate student" problem noted early and continually throughout the pursuit of "cold fusion". It was recognized that, if 1 W of power were produced by ordinary fusion, then there would be lethal doses of neutron-induced radiation damage to persons nearby. This too can be taken as evidence, if nuclear reactions do occur in "cold fusion" experiments, that normal fusion physics is not at work.

Two notable features appear repeatedly in post-mortem microscopic examinations of "cold fusion" samples. Protuberances and craters about 10μm in size are seen in the acidic electrolysis experiments by the Dash group (Kopecek and Dash, 1996), in glow discharge loading experiments (Savvatimova and Karabut 1995a), and in cavitation-bubble loading experiments (George & Stringham, unpublished). They are often the sites where unexpected elements appear in relatively high local concentrations. Whether these features are due to deposition, erosion or energetic micro-eruptions remains to be determined.

Material characteristics are central to "cold fusion." The composition and structure of the cathode and other materials, and experimental procedures, determine what is observed. Non-uniformities in chemistry and atomic arrangement exist on diverse spatial scales in most metallic materials. For example, grain boundaries (2-D), dislocations (1-D) and vacancies (0-D) attract impurities in many materials. And, the composition and micro-structure often vary with time. Storms (1996b) asserts that the character of what he calls the "nuclear active state" differs from the bulk properties. This has obvious implications for theories which attempt to explain "cold fusion."

5. THEORETICAL SUMMARY

Theory really only has two functions. The first is to explain the past, that is, to produce understanding of observed events and processes. The second is to predict the future by suggesting the qualitative and, best of all, quantitative outcome of new experiments designed to test or falsify a theoretical idea. There are many examples of both of these situations in science. It can be asked if theory has served either of these functions regarding "cold fusion". Put another way, are the available ideas adequate to explain any or all of Morrison's "three miracles"?

There are over a dozen quite distinct published theoretical ideas associated with the field. They vary widely in character and development. Some are entirely atomic while others emphasize nuclear mechanisms with little attention to the atomic size or energy scales. Others focus on solid-state effects. Several of the theories are little more than ideas, while a few of them have been elaborated. Some have not changed since first being proposed, but a few have evolved as new information and ideas appeared. Coherent physical processes are central to several of the theories.

Examination of "cold fusion" theories can, as with the experimental situation, be organized in a matrix. In this case, each of the separate theories is compared with a template of questions. The first question is what is the key or basic concept involved in the theory? Next, it is asked if the idea has been written out mathematically, so that it can be examined for completeness and correctness? Then, if an analytical theory is indeed available, has it been reduced to numbers? If quantitative calculations based on the theory have been made, have they been compared with available data? Or, have the numerical results been used to design new experiments? Importantly, it is not likely that a physical theory would apply exclusively to "cold fusion" and to no other observable aspect of the physical world. Hence, the final question in this series asks what are the implications of a particular theory in realms outside of "cold fusion", especially for spectroscopy, which is a sensitive probe of the processes and energetics associated with physical mechanisms?

A wide ranging review of "cold fusion" theories appeared a few years ago (Chechin et al, 1994). It was not organized as recommended above, but was quite comprehensive and critical. It would be useful to have a current version of that review, because some of the theories have evolved and new data has appeared. It is especially critical to determine which of the theories of "cold fusion" are simply incorrect in their basic idea or their theoretical development, or else fail comparison with sound data. It is also important to note which theories are incomplete, even if it is impossible to show them to be wrong at this time. Some of the theories focus on the excitation of nuclei in a lattice, that is, the formation of an excited intermediate state through concentration of energy onto a nucleus. However, they do not treat the deexcitation of the nucleus in a way consistent with available data, especially the low levels of neutron and gamma-ray emission.

The immense difference in energy densities on atomic and nuclear scales is at the heart of "cold fusion" and is the primary challenge to theories which hope to explain it. On the atomic (condensed matter) scale, potentials and excitation energies are on the order of 1 eV in volumes of 10^{-24} cm^3. On the nuclear scale, potentials and transition energies are typically on the order of 10^6 eV in volumes near 10^{-39} cm^3. The ratio of energy densities of 10^{21} is daunting. An energy densification of 10^{12} from acoustic fields to the atomic (optical) scale is observed in sonoluminescence. It is due to acoustically-induced flash vaporization, followed by spherically-symmetric bubble collapse, which is central to the energy density increase in sonoluminescence. This widely-observed phenomenon is not fully understood, but it is quite distinct from "cold fusion". The use of acoustically-induced cavitation bubbles to load hydrogen isotopes into materials by bubble collapse on the material surface should not lead to confusion between sonoluminescence and "cold fusion". The possibility that ordinary fusion might occur within collapsing cavitation bubbles in D_2O without any solids present, so-called "sonofusion", is yet another subject.

It must be noted that there are a few areas in physics where well-accepted nuclear effects are seen in the atomic realm, and vice versa. Hyperfine structure in atomic spectra, and subtle but well-measured isotope effects on x-ray spectra, are two examples. Of course, the best known example of the interaction of nuclear and lattice processes, and a case where a large energy density ratio is indeed realized, is the Mössbauer effect. Solid-state effects on radioactive decay processes are not as widely known, nor are they verified yet. They have been reported recently by Reifenschweiler for tritium (1996) and some nuclei of Ni, Zn and Sr (1997). Most of these various processes have been examined for their potential application to "cold fusion", but little has resulted. It seems clear that, if "cold fusion" is real, an even-handed treatment of both nuclear and condensed-matter regimes will be a hallmark of the successful explanation.

It should be recognized that some ideas potentially germane to "cold fusion" are in the process of being patented or are being held as proprietary. If experiments show that new science about low-energy nuclear reactions really has been discovered, then the history of ideas aiming to explain the observed effects should be interesting and possibly instructive.

Goodstein (1994) makes some useful general points regarding theory and "cold fusion". He cites the assertion by the philosopher Popper that a scientific idea cannot be proven to be true. No matter how many experimental reports agree with it, the idea (and the experiments) may nonetheless be wrong. Rather, the goal is to falsify theoretical ideas, that is, to show experimentally that they are wrong to some precision, and then to go on to develop other testable ideas. Of course, the lack of complete theoretical ideas about "cold fusion" makes this impossible! It has also been noted often since 1989 that it is impossible to disprove "cold fusion" experimentally. No set of negative experiments can prove that there is nothing new there, because each of them might have been done incorrectly, possibly in a region of "parameter space" which is not relevant.

It is possible, and maybe even likely, if "cold fusion" does turn out to involve new science, that theoretical understanding will only follow achievement of experimental reproducibility. Now, it is difficult to discriminate among the competing ideas, and to know which of them is worth the effort of further development; and, it is also challenging to come up with new ideas and evolve them, when the experimental situation is still so complex and fluid. However, the lack of any comprehensive "cold fusion" theory, including both excitation and deexcitation, is certainly problemmatic. The absence of complete and correct theories after over eight years of work on "cold fusion" is sobering, which tends to offset any enthusiasm from the current inability to explain away all the reported positive observations.

6. RELATED SCIENTIFIC PUZZLES

There are always many well-defined challenges in science, some old and some new. Such topics may or may not turn out to be correct, as discussed in relation to Figure 2. The core argument of this paper is that "cold fusion" is still among the undecided problems. There are other scientific riddles involving hydrogen and its isotopes which are probably distinct from "cold fusion," but they still come up in the same general context.

The phenomena of hydrogen embrittlement and stress corrosion cracking in metals and alloys are ascribed to the effects of environmental hydrogen. Protons, generally from water, dissolve in the materials and move rapidly along grain boundaries, leading to cracking and failures at stress levels well below the inherent strength of the materials in the dry state. The basic mechanism, which would not involve nuclear reactions, is not understood in detail.

While hydrogen-induced cracking is distant from the essential reactions which may be involved in "cold fusion", the entry of hydrogen into materials and the diffusion of protons within materials are central to controlling and understanding any "cold fusion" reactions. The surface physics and electrochemistry of hydrogen adsorption and absorption are old and still viable research areas with many quantitative challenges. The diffusion of hydrogen and its isotopes within materials is still not understood quantitatively, or even qualitatively on a basic, mechanistic level. At relatively modest elevated temperatures, hydrogen will diffuse over 10 m in the time that C, N or O will move about 1 μm in ordinary metals. This great contrast implies fundamentally different mechanisms may be at work. A recently-developed phonon-absorption model for the diffusion of hydrogen, deuterium and tritium fits the measured temperature dependencies of the diffusion coefficients in several simple metals (Baird and Schwartz, 1995). That model should be examined for its extensibility to predict the large absolute values for the same diffusion coefficients.

Studies of phases and their dynamics are certainly important to solving the "cold fusion" riddles. Two recent studies of Pd with H or D might be important in this arena. Fukai and Okuma (1995) reported a remarkable phase of Pd after addition and removal of H. Using both density and lattice parameter measurements, they found an unprecedented vacancy concentration of 18 +/- 3 %. This compares with vacancy concentrations produced thermally, which are normally well under 0.1 %, and those in off-stoiochiometric inter-metallic compounds, usually no more than a few per cent. The high vacancy phase may or may not be germane to "cold fusion" experiments, but it shows again that the Pd-H system remains poorly understood at high H loading.

The structure of Pd loaded with D to values of X (=D/Pd) exceeding 0.9 remains a question central to "cold fusion." A current experiment (Skelton et al, 1997) seeks to probe that phase. Fine beams of hard x-rays from a storage ring, on the order of 10 μm in cross section, were used to probe the interior of a 1 mm diameter Pd cathode during loading. Spatial resolution was obtained by choosing the off-axis beam position in 50 μm steps. Time resolution was gotten by taking sequential spectra during the loading. Room temperature diffusion rates have been derived by following the α to β phase transformation front, for which there is about a 12 % volume expansion. High loadings have not yet been achieved at the storage ring facility, and the work continues.

An ion beam experiment employing much higher energies than in "cold fusion" has revealed a remarkable anomaly. Kasagi et al. (1995) used 150 keV D beams to bombard deuterated Ti targets. Protons and alpha particles were observed at energies up to about 17 MeV and 6.5 MeV, respectively. The possibility of three-body D+D+D reactions was examined. The ratio of the three-body to the two-body rates was calculated to be 10^{-17}, while the experimental ratio is 10^{-5}. Whether or not this great discrepancy will be explained by unexpected D-D proximity in condensed matter, and even if so, whether that has any relevance to much lower-energy processes in "cold fusion," remain to be determined.

7. CONCLUSION

It is interesting, even if premature, to consider the potential engineering applications of "cold fusion". If it turns out to be "real", that is, if exothermic nuclear reactions are possible at ordinary temperatures, what are the implications? Many people have noted that the ability to produce temperatures only up to 100 °C, or a few hundred C° in pressurized aqueous electrolytes or in fused salt systems, would limit engineering applications. However, modest temperatures can be useful for desalination, which could have a major impact in some parts of the world. And, the higher temperatures would be sufficient to power electrical generators using available or somewhat modified technology.

Power densities are important to potential applications. In "cold fusion" experiments, reported excess powers are typically within the 1-10 W range, with some values both lower and higher by about a factor of ten. The Pd and other materials typically have volumes between 0.1 and a few cm^3. Hence, most power densities vary widely up to values near 100 W/cm^3, with the notable exception of the 60 kW/cm^3 result from the thin-wire cathode experiment in Italy. Higher values could be expected if "cold fusion" energy generation were to be optimized. Current and thoroughly-engineered commercial pressurized light water nuclear reactors have fuel rod power densities around 500 W/cm^3 average and 1000 W/cm^3 peak. It appears that "cold fusion" power densities, if controllable and sustainable, may indeed be viable for electrical power generation as well as for other uses.

Another essential factor regarding "cold fusion" is the energy released per atom. Both Pons and Fleischmann and McKubre's group have reported values near 300 eV per Pd atom. Arata and Zhang got a value over 60 times higher, as did Preparata. These high values argue both against chemical origins and against the oft-stated concern about a "cold fusion" cell being an energy storage device, as is a battery. Since Pd might merely be the host or environment for any reactions, it might be better to base energy density comparisons on the reactants. Miles and Johnson report 6 to 60 MeV per He atom produced, and Arata and Zhang found about 24 MeV per He atom (or half these numbers per deuteron). Ordinary D-D fusion reaction yields are near 1.5 MeV per deuteron, and U or Pu fission yields about 200 MeV per atom. The reported "cold fusion" excess energy values greatly exceed chemical reaction energies, which are a few eV per atom. They fall between fusion and fission yields on a per-atom basis. Of course, reactant densities (cm^{-3}) and reaction rates (sec^{-1}) are as central to power engineering as are the energy-per-reaction values. Discussion of fuel densities and the widely-varying rates of potential nuclear reactions are beyond the scope of this paper.

Integrated excess energies would limit some engineering uses of "cold fusion", just as the capacities of batteries determine their applications. Values near 300 MJ were noted earlier. How does such an energy relate to more familiar units and situations? To make the arithmetic easy, we note that 400 MJ is about 10^8 cal, or 10^5 kcal, the unit commonly called the "calorie" and used to measure the energy content of food. At a food intake of 2000 of these calories per day, 400 MJ would power a large person for 50 days. For another example, consider that one liter of gasoline has an available chemical energy of about 35 MJ. Hence, the highest integrated energies reported to date in "cold fusion" experiments, some of which have run for weeks, correspond to the energy from roughly 10 liters of gasoline. It seems clear that optimization and scalability will be pivotal to major utilization of any energy produced by "cold fusion", and these must be preceded by attainment of reproducibility and controllability.

The nuclear physics aspects of "cold fusion" are most central and provocative. However, the solid-state physics and material science of the subject are also important. These latter topics are areas of long and vigorous investigation. They have been and remain legitimate subjects for research and development, even if "cold fusion" were never claimed.

The level of interest in hydrogen within metals and alloys, and in hydrogen technology for energy and other reasons, is easy to assess. There are many books on these topics. Every other year there are major conferences, the World Hydrogen Energy Conferences, the eleventh of which was held in 1996. There are also bi-annual Gordon Research Conferences on Hydrogen in Metals and Alloys in the U. S. The point is that the loading, storage and egress of hydrogen (and its isotopes) from condensed matter systems are still of major scientific interest and practical importance. Some of the questions central to "cold fusion", for example, the achievement of high loading and associated phase changes, are pertinent to ordinary research on hydrogen-solid interactions. Careful investigations of these and related topics produce information which is publishable without extraordinary controversy in several journals, and can be of varying utility in the commercial and other employment of hydrogen.

Given the situation just described, a conservative approach to the questions raised by reports of "cold fusion" would be to proceed in two stages. The first would include focused attention on thorough investigations of the high and variable loading of hydrogen and deuterium into metals and alloys, plus study of the associated phases and kinetics. The work would require particular attention to the materials involved, especially their structure and impurity content before and after experiments. The employment of surface and bulk probes of the dynamics of loading and unloading, with both temporal and spatial resolution, is highly desirable. *In situ* x-ray diffraction during high loading experiments is particularly attractive. The high vacancy-concentration material reported by Fukai and Okuma should be studied both experimentally and theoretically. This first stage would involve ordinary research on hydrogen isotopes in condensed systems, regardless of its contentious motivation.

Once the materials science aspects and dynamics of high loading experiments are controlled, even if not completely understood, then a second stage could concentrate on attempts to produce and detect nuclear reactions in relevant phases at low energies. There are several untested ideas for experiments of this general type. However, it seems clear that experiments which have produced the strongest reports of such effects should be emphasized, rather than starting from scratch. If there are some "correct" sets of material and ambient conditions, which must be achieved in order to produce the desired nuclear reactions, past experiments may have attained those conditions. A systemic search over a grid of relevant parameters could be performed using modern methods of experiment design and statistical analysis. Such experiments would be time consuming and, hence, expensive. However, they would be necessary to provide both an unambiguous demonstration of any new effect(s), and to convince the broader scientific community of the reality of any discovery.

The step-wise approach recommended above would supersede the diverse uncorrelated, and often under-funded, experimental investigations of "cold fusion" to date. It could produce sound scientific and technical results, even though it cannot guarantee observation of the low-energy nuclear reactions of which some people are already convinced. Given the number of relevant topics, including materials science and engineering, surface science, electrochemistry, ion-solid interactions and others, this conservative program would require several $M annually for several years. A government, company or investor might want to attempt a more aggressive approach in order to directly reproduce any of the more significant experiments. Here, the cost might be about $1-2 M for two or three years for each type of experiment. These estimates are based on the costs of well-qualified and equipped scientists performing complex research, and on experience of several "cold fusion" laboratories to date.

For perspective, \$10 M is about 10^{-4} of what is spent annually on research and development by government and industry in the U. S.

There are two other opposing points which can be made regarding the entire inquiry into "cold fusion". The first has to do with a "higher level" analysis of the entire body of information relevant to "cold fusion". Bayesian analyses of different topics are able to produce quantitative estimates of probabilities based on incomplete and uncertain information. This approach grew out of the nuclear power industry, where estimates of the likelihood of various (mostly undesirable) occurrences are needed. But, the probabilities for relevant occurrences, such as machinery failures and potential human actions, are not known, either at all or with the desired certainty. A flippant characterization of such analyses says that something is known, even if not everything is known, and some results can be stated succinctly with appropriate rigor. The point here is that the potential application of this reasoning and methodology to the entire set of positive and negative reports of "cold fusion" might yield an estimate of the probability of there being something really new after all, even at this time of sporadic and limited reproducibility.

The view expressed in the last paragraph could raise hope in some quarters that at least an estimate of the reality of "cold fusion" could be achieved ahead of experimental reproducibility and understanding. But, a recent experience in nuclear physics provides a counterpoint (Taubes, 1997). In the mid 1980s, peaks were observed in the spectra of positrons emitted from highly energetic atom-atom collisions. The very deep and very transient potential wells, which exist when, for example, two uranium nuclei are near to each other, exceed the production threshold for electron-positron pairs. The positron emission is separately measurable. Due to the variable kinematics of such collisions, a continuous and smooth positron energy spectrum was expected. However, peaks six standard deviations above the continuum were obtained in the initial experiment. Ordinary statistics indicate that they would have only one chance in a million of being extraneous. Theories were developed to explain the peaks. Later experiments in the same series also gave peaks, but they did not appear in a new experiment or in another sequel to the original experiment. After a decade of research costing millions of dollars and using very complex detectors at a high energy accelerator, it is now felt that the peaks never existed in the first place, that is, they are "phantoms". If "cold fusion" turns out in the end to be illusory, then the case of the positron peaks will have more company in the annals of incorrect science.

The goal of this paper was to draw additional attention to results from several past and on-going "cold fusion" experiments around the world. The hope is that further examination of the evidence will lead, first, to increased research and, then, to either (a) acceptable results regarding one or more new effects, or (b) plausible explanations of why some or all of the apparently "positive" data might be wrong. The number of distinct experimental techniques for loading hydrogen isotopes into materials and measuring the effects, and the number of people involved in "cold fusion", are greater than for most past scientific misadventures. If "cold fusion" goes the way of polywater and the positron peaks, then a post-mortem might yield insights into the global operation of science in this era of rapid communications and media attention. If "cold fusion" turns out to be something new, the potential implications are even more interesting and important. Either the production of energy, or the production of new materials, by nuclear reactions at low energies, would be dramatic, especially if there is much less associated radiation than for ordinary fusion or fission. And, what if such nuclear reactions could be used to augment existing weapons, or even enable new weapons, as has been the case for many new energy sources throughout history?

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Internet Discussion Groups

Three groups deal with "cold fusion", often with a low signal-to-noise ratio: sci.skeptic, sci.physics and sci.physics.fusion