Cold Fusion - Alchemist's Dream

E. D. Clayton

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Richland, Washington 99352

SUMMARY

The status of cold fusion as reported in a press conference by Dr. S. Pons at the University of Utah on March 23, 1989 is briefly reviewed. Widespread skepticism has continued to prevail since the press conference and researchers at several major laboratories such as the California Institute of Technology and the Atomic Energy Establishment at Harwell, England, have now given up efforts to duplicate the cold-fusion experiments due to negative results of their experiments. So far, other major laboratories including the Lawrence Livermore and Los Alamos National Laboratories have been unable to confirm that their electrolytic cells produce excess heat as a consequence of deuteron cold fusion. The sudden and unpredictable generation of excess power, that has been reported to occasionally occur during the operation of the electrolytic cells, is mystifying and remains unexplained.

The idea that palladium might play the role of a catalyst to convert hydrogen into helium is far from new. A paper prepared in 1926, some 63 years ago, by two Germans discusses this very subject; the paper was prepared prior to the discovery of either the neutron or fusion. Indicators of fusion in the palladium metal are excess quantities of ^3He and T (the products of deuteron fusion) and anomalously high ratios of $^3\text{He}/^4\text{He}$. In 1978, however, a paper by the Russians reported finding excess quantities of ^3He and T in a number of metals and semiconductors. Because of this paper, it might be concluded that finding excess concentrations of He or variants in the $^3\text{He}/^4\text{He}$ ratio in palladium may not give assurance, after all, that fusion has recently taken place via the electrochemical cell.

Attempts have been made by various scientists to explain the extremely low level of power, as may have been generated by fusion, from experiments performed recently by Dr. Steven Jones et al. at BYU. (But not the power level reported by Messrs. Pons and Fleischman in experiments at the University of Utah.) These are discussed; one involves a purely chemical reaction; and the other, muon catalysed cold fusion in the palladium metal. Neither methods have been confirmed.

It is concluded that the very low levels of fusion power that can be inferred from the neutrons and tritium detected are, at this stage, principally of scientific interest only.

Further research on cold fusion may, however, lead to future power and other unforeseen applications. The Utah experiments have stimulated world wide interest in, and a re-examination of, cold fusion concepts.

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INTRODUCTION

On Thursday of March 23, 1989, Dr. Stanley Pons of the Department of Chemistry at the University of Utah announced in a press conference that he and a colleague, Dr. Martin Fleischman, Department of Chemistry, University of South Hampton, had achieved fusion at room temperature in a small electrochemical cell. Further, for each watt of power input four watts of power output had been achieved (300% gain). This was reported to have been achieved by electrolysis of heavy water which released deuterium at a palladium cathode. The deuterium was then absorbed and concentrated in the palladium metal where fusion was presumed to have taken place by some process as yet unknown (1).

The announcement had tremendous impact on the scientific community. Since then, hundreds of physicists and chemists throughout the world have performed large numbers of experiments in attempts to confirm the University of Utah claims, mostly with little or no success. A Brigham Young physicist, Dr. Steven Earl Jones, who has done extensive research on Muon-Catalysed Cold fusion in the past, prepared a paper, dated March 23, 1989, describing work that he and a number of colleagues had recently done at Brigham Young similar to experiments of Pons and Fleischman (2). In the Brigham Young experiments a very low level of neutrons was observed, and no excess heat; Steven Jones has asserted that Messrs. Pons and Fleischman are wrong in claiming the heat produced in their experiments is coming from deuteron fusion. Scientists from the Los Alamos National Laboratory, Texas A&M University and the National Institute of Nuclear Physics in Italy also have reported neutrons coming from the palladium or titanium rods. The low levels of neutrons observed, however, would imply fusion at power levels from a billionth to a trillionth of that reported by Pons and Fleischman.

Reports from various sources are made almost daily, but at present it may be concluded that if heat is observed, there are no indications that fusion is the cause. If heat is observed, no neutrons are simultaneously detected, and if tritium is detected then no neutrons, or heat, etc. There appears to be no correlation that any of these take place simultaneously.

It is obvious that heat, neutrons, and tritium should simultaneously be produced as a result of the well known deuteron fusion channels that are known to occur with near equal probability:

The idea that palladium might play the role of a catalyst to convert hydrogen into He is far from new. In fact it is at least 63 years old. A paper entitled, "On the Conversion of Hydrogen to Helium" was published in 1926 by two Germans, Fritz Paneth and Kurt Peters, of the Institute of Chemistry at the University of Berlin (3). This paper was recently translated by Klaus Lackner and Paul Allison of the Los Alamos National Laboratory, 16 April 1989. Note the German paper was prepared six years before the discovery of fusion in 1932 by Sir Rutherford and the discovery of the neutron by Sir Chadwick that same year. An English summary of the German paper was published in Nature (1926), pp. 526-527. There are some extremely interesting statements in the paper. Quoting from the translation:

"That helium which originated in the mutual action of hydrogen and palladium has its origin-in the sense of our original idea-in a catalytic favoring the intrinsic tendency of the hydrogen to transform itself into helium cannot directly be concluded from the experiment. If however, one decides to accept the idea of a new generation of helium, then this remains by far the most sensible explanation. To the palladium one will ascribe the role of a catalyst even more so, since the platinum, albeit more weakly, acts qualitatively in the same way.

One more important question which we have to leave unanswered for now is the whereabouts of the large amounts of energy which are most likely freed in the formation of helium. It has been said frequently that any generation of helium would immediately reveal itself through an enormous generation of heat. These expected amounts of energy are relatively enormously large, i.e. compared with those released in chemical reactions, but in the absolute amount for the helium produced in our experiments, presumably too little to be even measurable. In the formation of 10^{-8} ccm of helium one expects an enthalpy of 0.28 cal. However, if one loads the palladium with hydrogen there are much more significant heat effects which are partially interpreted as heat of absorption, partially as enthalpy of a chemical binding and to a part have their origin in the combustion of

oxygen residuals. Beside these effects, an enthalpy as a consequence of the generation of such little amounts of helium will be very difficult to detect."

It is amazing that by changing a few words, and re-arranging, the above statements might be used equally well to summarize the current status of the "Utah Experiments." It, also, has recently been reported that in the Texas A&M experiments that when Li was replaced with Na in the electrolyte that the heat generation went down. This would suggest heat generation from some type of chemical reaction at the cathode.

Many scientists continue to question the validity of the "Utah Cold Fusion" claims, largely because, 1) The method of making the announcement via a press conference prior to publishing a paper in a scientific journal was not in conformity with past professional ethics, and 2) There was no clear cut signatures that nuclear fusion of deuterons had taken place. The neutron flux, if any, was down by factors of trillions from that required to generate the power claimed to have been achieved by Messrs. Pons and Fleischman.

In the ancient past, Medieval chemists searched in vain for the elusive "philosophers stone." This was the mythical stone that would transmute base metals into gold, and transmit the philosopher who found it into a prince wealthy beyond imagination. In effect, the stone would generate something for nothing. In the case of the "Utah Experiments" of Pons and Fleischman the "magical stone" is the simple electrochemical device involving a palladium electrode in heavy water that catalyses fusion of deuterium from sea water. If the device worked it would provide a cheap unlimited power source for mankind for all time to come. (Pons, Fleischman, and the University of Utah, would in turn be the princes of energy, and become fabulously rich on the patents therefrom). Because the stakes were so high good scientific procedures, and policy, were bypassed in the interest of expediency (someone else might learn of the process and patent it first).

The findings to date are largely of scientific interest, and currently appear to have no practical power producing possibilities. If nothing

else, however, the "Utah Experiments" have served to stimulate a new, diversified, look at "cold fusion" that could in the end lead to startling innovations and new developments with unexpected benefits in the energy field.

SUNDRY FACTS - THEORY

There is nothing new about the idea, or concept of cold fusion. There are two types of known cold fusion: 1) Muon Catalysed Cold Fusion (4-6), and 2) Piezonuclear Fusion (7). In the first type, hydrogen atoms making up a molecule can be bound so closely together by a heavy negative muon that fusion occurs in less than a millionth of a second once the muonic molecule (DuD) has formed. The second type involves low level fusion as a consequence of tunneling by deuterons through the potential coulomb barrier between deuterons in a normal molecule. These are briefly reviewed below, since both have been suggested as possible explanations for results of recent experiments.

MUON CATALYSED COLD FUSION(a)

Muon catalysed cold fusion was first observed in 1956 by the late Dr. Louis Alvarez. During his Nobel prize acceptance lecture, Alvarez included the following statement:

"We had a short but exhilarating experience when we thought we had solved all of the fuel problems of mankind for the rest of time. A few hasty calculations indicated that in liquid HD a single negative muon would catalyse enough fusion reactions before it decayed (mean life of muon is $\sim 2.1 \mu \text{ sec.}$) to supply the energy to operate an accelerator to produce more muons, with energy left over after making the liquid HD from sea water...."

Later it was found that very few fusions could be catalysed by a single muon-one or less. Muon catalysed cold fusion reactions of PD were quite well understood and quite useless as a energy source.

This work was continued in Russia, however, and it was later found that on the basis of resonant muonic molecule formation theory a number of

-
- (a) The process is referred to as cold fusion because it will occur at temperatures as low as that of liquid D-T, and will cut off entirely if the temperature reaches $\sim 5000^\circ\text{K}$ (approximate temperature for dissociation of D_2 molecules).

fusions in a D-T mixture could be obtained (8-10). This has since lead to renewed interest in the process. It is now stated that muon catalysed cold fusion is, "an idea whose time has come again." D-T muon catalysed cold fusion now offers good potential for more than "energy break even." Dr. Steven Earl Jones of Brigham Young University has performed many experiments and is perhaps the leading proponent in the U.S. (6). To date, some 180 fusions of D + T have been caused to occur by a single negative muon releasing about 3 GeV of energy. Data provide evidence that under the proper conditions of pressure and density many more fusions are likely per muon. It should be emphasized, however, that a muon catalysed fusion chain will only work with a DT-mixture. It will not work with deuterium alone; it will not work with a deuterium - hydrogen mixture; it will not work with a He-D mixture. Note that in the experiments of Messrs. Pons and Fleischman it is the fusion of deuterons inside the palladium metal lattice that is reported to be taking place.

PIEZONUCLEAR FUSION

The possibility of tunneling leading to fusion in isotopic hydrogen molecules at room temperature and atmospheric pressure was described in a paper by Messrs. Van Siclen and Jones in 1985 some four years before the press conference at the University of Utah (7). Their calculations show that as the density of the collection of molecules is increased, the fusion rate likewise increases. The results are calculated by an easily derived semi-classical equation for the fusion reaction rate Λ , wherein $\Lambda = A|\psi(0)|^2$. A is the nuclear reaction constant obtained from measured cross sections for low energy collisions between hydrogen nuclei and the probability $|\psi(0)|^2$ is the square modulus of the molecular wavefunction at the interaction distance. The nuclear fusion rate for molecular deuterium at room temperature and atmospheric pressure is shown to be vanishingly small. The result also applies to other isotopic hydrogen molecules. For a D_2 molecule at room temperature the fusion rate is about $10^{-74} \text{ sec}^{-1}$. For a D_2^+ molecule it is about $10^{-75} \text{ sec}^{-1}$. In the case of a muonic ($D\mu D$) bound molecule the calculated rate is a staggering 10^{11} sec^{-1} . The latter rate

is consistent with observed data on muon catalysed cold fusion. Since the negative muon has a mass that is 207 x that of the electron the separation between deuterons in a $D\mu D$ bound molecule is about 358 fm or 358×10^{-13} cm. It should be emphasized that the equilibrium separation between deuterons of a D_2 molecule decreases very little even at substantial pressures. At a pressure of 200 kbar, the equilibrium separation is ~ 0.69 Å; at atmospheric pressure the separation is 0.74 Å. Furthermore, D_2 molecules are not absorbed as such into palladium or platinum, etc., but as deuterium atoms. These are collected at interstitial sites, or lattice imperfections, in the metal. The separation between deuterons in the metal at interstitial sites is actually greater than it is between the deuterons of a D_2 gas molecule. In addition, liquid deuterium would contain about 8.9 x as many deuterons/cc as a fully deuterated palladium lattice.

To explain the low level of neutrons observed by Jones et al., "Observation of Cold Nuclear Fusion in Condensed Matter," the fusion rate would need to be $\sim 10^{-23} \text{ sec}^{-1}$, and possibly as large as $\sim 10^{-20}$ fusions per D-D pair per second (2).

SUNDRY EXPLANATIONS PERTAINING TO COLD FUSION

Several methods have been proposed for explaining the low level of neutrons observed by Steven Jones, et al., but not the heat level reported by Messrs. Pons and Fleischman. These include: 1) Muon catalysed cold fusion induced by cosmic ray muons, 2) Piezonuclear fusion on tunneling between deuterons within the deuterated palladium lattice, 3) vibrational mechanisms in highly excited states of D₂ molecules outside the palladium metal, 4) the branching ratio for deuteron fusion, if it occurs in the palladium, favors the tritium channel, etc. Comments are provided below concerning these and other suggestions.

COSMIC RAY MUON CATALYSED COLD FUSION

M.W. Guinan, F.F. Chapline, and R.W. Moir of the Lawrence Livermore National Laboratory have prepared a paper entitled, "Catalysis of Deuterium Fusion in Metal Hydrides by Cosmic Ray Muons," preprint April 7, 1989 (11). It is suggested that the Jones results (not that of Pons and Fleischman) might be explained on the basis of muon catalysed fusion of deuterons in the titanium lattice-if each cosmic ray muon were to catalyse a chain of some 700 D+D fusions. The process is illustrated in Figure 1. We shall refer to it as the "double catalyst" process (our terminology). There are, however, some highly questionable assertions. The first is that a negative muon would be preferentially captured in a K shell of the deuterons as opposed to being captured in a K shell of the titanium or palladium nuclei. Calculations have since been reported to have been made which suggest just the opposite: the capture time for a muon in the K shell of a palladium nucleus is $\sim 0.1 \mu\text{sec}$, whereas for the deuteron it is $\sim 2.2 \mu\text{sec}$ in the deuterated lattice. Bare in mind that the mean life for a negative muon is only $\sim 2.1 \mu\text{sec}$. J.A. Wheeler proposed in 1949 that a muon entering a K orbit of a heavy nucleus would be absorbed into that nucleus before it had time to decay (12,13). It is suggested on the basis of the above

(1) Metal facilitates rapid formation of muonic molecule following capture of negative muon by deuteron; muon then rapidly catalyses fusion of deuterium in molecule.

(2) External Muons: This specifically refers to negative muons, 1) as might come from pions produced by an accelerator, 2) muons as might come from the subsequent annihilation of antiprotons generated in an accelerator, or 3) muons from cosmic rays.

Figure 1 "Double Catalyst" Process in Hydrogenated Metals
(See Note 1 Above)

calculated capture times that essentially any neutron production would be the result of muon capture neutron reactions within the palladium or titanium nuclei.

The separation energy of a neutron from a palladium nucleus ranges between 7.0 to 10 MeV depending on the evenness or oddness of the neutron number of the isotope in question (14). The rest mass energy of a muon is ~106 MeV which is at least 10 times greater than the separation energy. Muon capture in the nucleus proceeds according to the nuclear reaction, $\mu^- + p \rightarrow n + \nu$ (15). Most of the rest mass energy of the muon is carried off in neutrino energy, but the neutron energy, about 15 MeV, is substantially higher than the 2.45 MeV neutron released in the D-D fusion channel that yields ${}^3\text{He} + n$. The process is similar for the case of muon capture in titanium and other metals, etc. Further, the observed counting rate for neutrons has not been consistent with short bursts of neutrons, as would be generated by occasional chains of 700 fusions each, if the suggestions by Messrs. Guinan, Chapline, and Moir were correct. At present, therefore, little merit is given to their postulation.

VIBRATIONAL MECHANISMS IN EXCITED STATES OF D₂ MOLECULES

An interesting method has been proposed by Messrs. D.R. Jennison and E.B. Stechel of the Sandia National Laboratories to explain the low neutron emission rates observed by Jones, et al. at Brigham-Young University (16). It is stated that surface science arguments support the creation of D₂⁻ molecules at the cathode surface. If the D₂ molecule left the surface adiabatically, it would be vibrationally cold. If, however, interactions with the electrolyte caused it to leave the surface as D₂⁻ (such as coordination with positive ions), conversion to neutral D₂ would take place non-adiabatically resulting in highly vibrationally excited D₂ (~ v=13) molecule. The classical turning point for this vibration results in a compression of the D-D distance of about 50%, a distance sufficient to explain the low neutron flux claimed by Jones on the basis of piezonuclear fusion - if a dense population of excited molecules were created as a result of the electrolytic reaction. Since the deuterons spend most of their

time at the classical turning points, where the kinetic energy and velocity are zero, and come closer together in an excited state, the deuterons are more likely to tunnel through the potential barrier and fuse. The rate of piezonuclear fusion would be enhanced. The process is illustrated graphically in Figure 2. Calculations recently made by Dr. Lee Burger of PNL are in conformity with the statements of Jennison and Stechel regarding the excited vibrational state of the D₂ molecule. If there is merit to the above, it is extremely interesting because neither palladium, titanium, platinum, or any other hydrogen absorbing metal would be required for the cathode. To generate fusion, only a metal supporting the creation of D₂⁻ molecules at the cathode need be used. Research should be conducted with other materials to determine if the proposed mechanism is valid.

BARRIER PENETRATION PROBABILITIES WITHIN THE HYDROGENATED METAL LATTICE/ PIEZONUCLEAR FUSION

A paper by A.J. Leggett and G. Baym of the Department of Physics, University of Illinois, entitled, "A Rigorous Upper Bound on Barrier Penetration Probabilities in Many-Body Systems: Application to Cold Fusion," was submitted to Physical Review Letters, May 3, 1989 (17). In the preprint the following statement appears:

"We conclude that if the 'cold fusion' claimed to have been observed in D trapped in Pd or Ti is a real phenomenon, it is unlikely in the extreme to be due to solid-state enhancement of equilibrium probability."

It has previously been mentioned that the inter-nucleon separation between deuterons at interstitial sites in a metal lattice is actually greater than the separation between deuterons in a gas molecule outside the metal.

A paper by S.E. Koonin and M. Nauenberg of the Institute of Technical Physics, University of California, Santa Barbara, entitled "Cold Fusion in Isotopic Hydrogen Molecules," was submitted to Nature, April 7, 1989 (18). In this paper, tunneling rates of diatomic molecules of deuterons are calculated considering the shielding effects of the negative electrons and

⁽¹⁾ Note: Deuterons spend most of time at classical turning points, and come closer together in excited state. Hence, deuterons are more likely to tunnel through potential barrier and fuse.

Figure 2 Fusion Enhanced by Excited Vibrational State of Deuterium Molecule⁽¹⁾

found to be orders of magnitude larger than the results of Van Siclen and Jones in their paper on Piezonuclear Fusion in Isotopic Hydrogen Molecules. The rate of P+D fusion also was calculated to be larger than for D+D due to the enhanced tunneling in the lighter system. (Note: the latter result is totally inconsistent with experimental data on muon catalysed cold fusion which show that the time required for fusion of a PuD molecule is some 300 times longer than the fusion time of a $\text{D}\mu\text{D}$ molecule.)(6)

It also is stated in the paper by Koonin and Nauenberg that enhancements of the electron mass by factors of 5-10 would be required to bring the cold fusion rates into the range of recently claimed observations, but there is no plausible mechanism for achieving such enhancements. Further, it is concluded that if any nuclear processes are being seen at all, it is more likely the neutron-free P+D reaction that is occurring rather than D+D, which again is contrary to the data on muon catalysed cold fusion.

BRANCHING RATIOS OF D_2 FUSION AT LOW ENERGIES

Because of the paucity of neutrons in the Utah experiments, it has been suggested that in the hydrogenated lattice the $\text{D}+\text{D} \rightarrow \text{T}+\text{P}$ channel for fusion may predominate. To suggest the latter is to ignore the very considerable experimental data from muon catalysed cold fusion. The branching ratio for the two channels has been measured and is near unity. This was determined by the Soviets as early as 1964 (19). The statement by V.P. Dzhelepov, et al. is: "Here it was established that the ratio of probabilities of channels (3) and (4) is close to unity" (19). The numbers 3 and 4 refer to the T and ^3He fusion channels. The ratio applies to liquid deuterium (very cold indeed). Data also are provided in a recent paper by Ronald E. Brown and Nelson Jarmie of the Los Alamos National Laboratory (20). The paper is entitled: "Differential Cross Sections at Low Energies for $^2\text{H}(\text{d},\text{p})^3\text{H}$ and $^2\text{H}(\text{d},\text{n})^3\text{He}$," preprint LA-UR-89-953. The paper contains data that can be used to estimate the branching ratio for deuteron fusion at low energies. The data imply the split to be ~51% on the T side and ~49% on the ^3He side, or a branching ratio of near unity.

FUSION OF DEUTERONS INTO ^4He

It also has been suggested that in the palladium lattice the deuterons might somehow fuse to form ^4He . This is extremely unlikely, because some 24.4 MeV of gamma rays (the difference in nuclear binding energies between the two fusing deuterons prior to fusion and the ^4He nucleus after fusion) must be released if the ^4He nucleus is to remain intact. There appears to be no reasonable mechanism for the release of this large quantity of energy. The branching ratio for fusion into ^4He and a gamma ray is about one million times less than it is for fusion into the

which occur with near equal probability.

It has even been suggested that within the hydrogen metal lattice ^4He might be produced with the emission of heat but no gamma rays. The latter suggestion comes under the nature of compounding "miracle upon miracle" and needs no further comment.(a)

SECONDARY D+T FUSION WITHIN THE HYDROGENATED METAL LATTICE

If fusion of D+D takes place within the metal lattice, then about 50% of the time the result will be a 1 MeV triton and a 3 MeV proton. The 1 MeV tritons may in turn interact with the deuterons in the lattice by means of the well known fusion reaction, $\text{D} + \text{T} \rightarrow \text{}^4\text{He} + \text{n}$ (17.6 MeV). For every 4000 DD-fusions it is roughly estimated that about one DT-fusion might follow. The intensity of 14 MeV neutrons would be far too small to detect in the case of the experiments by Jones et al. at Brigham Young. If, however, any significant power were being generated, such as 4 watts from fusion of deuterons (University of Utah experiments) the presence of 14 MeV neutrons would be clearly detectable as another possible signature of fusion.

(a) An MIT theorist recently withdrew a paper suggesting how these miracles might occur.

^3He to ^4He RATIO WITHIN THE METAL LATTICE

The atomic composition of helium in the cosmos is, $^3\text{He}/^4\text{He} = \sim 140 \times 10^{-6}$ (21). About one atom of ^3He is found per 7143 atoms of ^4He . Helium exists in the earth's atmosphere to the extent of 5.24 Vppm. In the earth's atmosphere the $^3\text{He}/^4\text{He}$ ratio is very substantially different from that of the cosmos. It is, $^3\text{He}/^4\text{He} \approx 1.4 \times 10^{-6}$.

This ratio differs by a factor of 100 from that of the cosmos. The ^3He in the atmosphere has been substantially diluted by α -decay of actinide elements over the years, releasing ^4He . The release of ^3He from piezonuclear cold fusion of D+P within the earth may also have effected this ratio (7).

The diffusion rate of He within a metal is presumably small. A simple method of producing high purity hydrogen has been to filter the hydrogen gas through heated palladium which freely passes the hydrogen but restricts the flow of other impurities in the gas. Therefore, He that is produced as a result of possible deuteron fusion is expected to remain in the metal. A mass spectrographic measurement of the He in the metal, or more specifically the ratio of $^3\text{He}/^4\text{He}$ in the metal (before and after the experiment) provides an indication of fusion. Since, however, the Soviets (1978) have previously detected (see below) anomalously high ratios of $^3\text{He}/^4\text{He}$ in metals, the interpretation of the results must be made with extreme caution.

SOVIETS - OTHER

SHOCK INDUCED FUSION

In 1986 a paper was published by the Soviets entitled, "Possibility of Nuclear Reactions During the Fracture of Solids." This was authored by B.V. Deryagin et al. of the Institute of Physical Chemistry, Academy of Science, Moscow (22). On the basis of an analysis of data on the characteristics of radiation accompanying the fracture of solids, such as LiD_2 , it was suggested that DD reactions may take place during the fracture of deuterated dielectrics. It was shown that weak neutron streams may be seen during the shock-destruction of deuterium-containing solids indicating that DD-fusion reactions could occur in the destruction zone. One theory suggests that the strong electric transient field created at the rapidly spreading cracks in the crystals accelerated the deuterium atoms so much that some of them fused on collision.

A speculative comment as to why such a "wild" experiment may have been done in the first place is perhaps in order. LiD_2 is a component of nuclear weapons used to breed tritium - for fusion with deuterium - to produce neutrons - to cause further fissions in the U or Pu (the term is "D-T" boosted device).

If the US proposes shooting down Russian missiles with rail guns or high velocity projectiles, then the effect of these high velocity projectiles on an incoming missile, would of course be of some interest.

The possibility of shock induced fusion is an interesting concept deserving further study.

ANOMALOUSLY HIGH ISOTOPIC RATIOS OF $^3\text{He}/^4\text{He}$

Another interesting Soviet paper appeared in 1978 (23). The paper was authored by B.A. Mamyrin et al. of the A.F. Joffe Physicotechnical Institute, Academy of Sciences of the USSR, Leningrad. The paper is entitled, "Anomalously High Isotopic Ratio of $^3\text{He}/^4\text{He}$ in Technical-Grade Metals and Semiconductors." It is shown in this paper that the $^3\text{He}/^4\text{He}$

ratio in a number of commercially-pure metals and semiconducting materials is orders of magnitude higher than expected on the basis of the ratio in the earth's atmosphere, etc. Ni, Ti, Pd, Pt and a number of other metals were included in this study.

The point is that the helium isotope ratio in any object carries information about processes leading to their formation. This is why the ^3He content of the palladium cathodes in the Utah experiment is considered an indicator of fusion. The Russian paper, however, implies that finding excess quantities of He or variants in the $^3\text{He}/^4\text{He}$ ratio may not be a good indicator, after all, that fusion has recently taken place via the electrochemical cell.

The following is quoted from the 1978 paper:

"4. It can be assumed that ^3He in metals is formed as the result of processes which occur right in the metal and these processes lead to the formation of either ^3He , T, or T and ^3He together. An analog of processes of this type is the nuclear reaction which takes place with the interaction of deuterium mesic atoms with ordinary deuterium atoms at thermal energies $\mu^-d + d \rightarrow ^3\text{He} + n + \mu^-$.

It should be borne in mind that the absolute concentration of ^3He and T in metals are low and the build-up time for ^3He and T may be quite long. Accordingly, even processes with a very low yield can, in principle, supply the observed quantities of ^3He in metals."

Further investigation seems warranted.

CONCLUSIONS

As of June 5, 1989, it appears quite certain that the excess heat reported by Messrs. Pons and Fleischman is not being generated by fusion of deuterons in the palladium cathode. In the absence of significant fusion indicators it is highly likely that the excess heat is coming from unknown chemical reactions. One may even select a few words from a paper prepared by two Germans, Messrs. Paneth and Peters, some 63 years ago (1926) to help describe the current situation. Quoting from this early paper:

"However, if one loads the palladium with hydrogen there are much more significant heat effects which are partially interpreted as heat of absorption, partially as enthalpy of chemical binding and to a part have their origin in the combustion of oxygen residuals. Besides these effects, an enthalpy as a consequence of the generation of such little amounts of helium will be very difficult to detect."

So far in the "Utah experiments" no ^3He has been reported and the low levels of neutrons and tritium, as indicators of fusion, correspond to heat generated from fusion that is far too small to detect.

In 1978 the Soviets reported finding anomalously high isotope ratios of $^3\text{He}/^4\text{He}$ in a number of metals. Finding ^3He in the palladium or titanium cathode may not, by itself, be an indicator that fusion of deuterons has taken place, nor even the detection of tritium. Further experiments are needed in the case of the Soviet results.

The fact that the power level decreased when sodium was used to replace lithium in the electrolyte in the Texas A&M experiments is very discouraging and suggests a chemical reaction is involved.

There appears to be no good correlation in time for the observations of neutrons, tritium, and heat. When there are neutrons, no tritium or heat are observed. When tritium is detected then no neutrons and when heat is observed neither neutrons nor tritium are detected at the same time.

Piezonuclear fusion or tunneling of deuterons through the potential coulomb barrier has not yet provided a satisfactory exploration for even the very low level of neutrons observed in the Jones experiments at Brigham Young.

Jones attempts to explain the low level of neutrons in his experiments on the basis that isotopic hydrogen is known to accumulate at lattice imperfections in metal lattices and that a local high level of concentrations might be conducive to piezonuclear fusion. There appears to be no evidence, however, that in such accumulations the separation distance between deuterons would be substantially less than it is in a gas molecule.

A vibrational mechanism has been proposed by Messrs. Jennison and Stechel of the Sandia National Laboratory to explain the Brigham Young results. It is suggested that if a D_2^- molecule left the cathode surface it could end up in a highly vibrationally excited state with a compression of the D-D distance by about 50% at the classical turning point. Piezonuclear fusion (tunneling) in a dense population of excited molecules could explain the low level neutron flux observed by Jones. Note that absorption of deuterons into the palladium or titanium would play no part in the reaction! This concept deserves further study.

The probability ratio for fusion of deuterons by the two channels, $D+D \rightarrow {}^3He + n$, and $D+D \rightarrow T + P$ is near unity. To explain the lack of neutrons on the assumption that in the palladium or titanium lattice the tritium channel is highly favored, is to ignore the considerable amount of data that exists on muon catalysed cold fusion.

The claims of Messrs. Pons and Fleischman have resulted in extensive reviews of data on cold fusion or fusion by any mechanism other than "hot plasma fusion" or "inertia confinement fusion." It was reported by the Soviets in 1986 that weak neutron streams may have been seen during shock-destruction of deuterium-containing solids indicating that nuclear D-D fusion reactions could occur at the moment of fracture. This concept of shock induced fusion deserves further study.

The low levels of fusion power that can be inferred from the neutrons and tritium detected are principally of scientific interest only.

Further research on cold fusion may, however, lead to future power and other unforeseen applications; this includes, muon catalysed D-T cold fusion, shock induced fusion, enhanced piezonuclear fusion from highly vibrationally excited states of D₂ molecules, and the process by which neutrons are generated during temperature cycling of metal samples heavily loaded with deuterium from high pressure gas, etc. It appears extremely unlikely, however, that the "Utah experiments" of Messrs. Pons and Fleischman will, as such, prove to be anything more than of scientific interest in the end.

Some years ago the Soviets reported having discovered a new form of water, "polywater." This would have a higher density, a higher boiling point, and a lower freezing point than ordinary water. Theorists throughout the world rushed to be first to publish papers proving that polywater should exist-even giving its chemical formula. Later "polywater" was found to be nothing more than ordinary water with a little salt dissolved therein-or salt water. At this point in time the claims of Messrs. Pons and Fleischman may be likened to the earlier claims regarding the discovery of "polywater." As yet, however, no theory has been proposed to explain with assurances even the small level of neutron flux observed by Jones in the Brigham Young experiments.

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APPENDIX

A FEW SUNDRY FACTS ON PHYSICAL PROPERTIES OF SELECTED METALS AND HYDROGEN

1. Palladium is known to absorb up to 900 times its own volume of hydrogen gas at room temperature.
2. Palladium has been used as a filter to produce high purity hydrogen. Heated palladium freely passes the hydrogen, but restricts the flow of other gases or impurities in the hydrogen.
3. Palladium containing hydrogen will glow red hot when exposed to air.
4. Platinum will absorb large volumes of hydrogen at room temperature giving it off at higher temperatures.
5. Platinum will cause a mixture of hydrogen and oxygen gas at room temperature to explode.
6. Platinum wire will glow red hot when exposed to methyl alcohol vapor. (Basis for "Duck Hunters" hand warmers, etc.)
7. Tantalum will absorb 740 times its own volume of hydrogen at 600°C, giving off the gas at 800°C. (Tantalum has been used as a "getter" in the past.)
8. According to a statement by Mr. Herman Garfinkel, Vice President of Englehard Metals Corp., in April 1989: "Palladium tends to alloy itself with both platinum and lithium in heat producing reactions."
9. Fully hydrogenated metals, Pd, Pt, Ta, etc., increase their volume by ~3-4% on absorption of the hydrogen; this expansion is easily observable.
10. Hydrogen enters the metal as atomic hydrogen not as a molecule. The separation distance between the "bare" deuterons located at interstitial sites in the metal is greater than the separation between deuterons in a deuteron molecule of gas under STP.

11. The equilibrium separation between deuterons of a D_2 molecule decreases very little even at substantial pressures. At a pressure of 200 kbar, the equilibrium separation is $0.69\text{\AA}$; at atmospheric pressure the separation is $0.74\text{\AA}$.
12. Hydrogen is predicted to become metallic at a pressure of two to three million atmospheres.
13. The density of liquid deuterium is about 1.4 g/cm^3 .
14. Liquid deuterium would contain about 8.9 times as many deuterons/cc as a fully "deuterated" palladium lattice.
15. As a result of the tritium content in heavy water, there will be about 100,000 dpm per liter from the T beta decay.
16. Concentration of deuterium in sea water: one atom of deuterium per 600 of hydrogen.
17. Concentration of tritium in sea water: one atom of tritium per 10^{18} of hydrogen; activity, $\sim 1/2$ pico Curie/liter.
18. Concentration of tritium in heavy water: one atom of tritium per 10^{13} or 10^{14} atoms of deuterium; activity $\sim 0.045\text{ }\mu\text{Curies/l}$.

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