PIEZONUCLEAR FUSION AT BRIGHAM YOUNG UNIVERSITY

I. Scientific Team

A. Brigham Young University Faculty Members – Department of Physics and Astronomy

Steven E. Jones (resume attached)
E. Paul Palmer
J. Bart Czirr
Daniel L. Decker
Gary L. Jensen

B. Brigham Young University Faculty Members – Department of Chemistry

James M. Thorne

C. Brigham Young University students

Stuart F. Taylor
Rod Price
J. W. Wang
David Mince
Eugene Sheeley
Paul Dahl
Paul Banks

D. University of Arizona Faculty Members – Department of Physics

Johann Rafelski

II. History

A. Scientific paper published March 1986 (submitted June 1985)

The roots of our work regarding piezonuclear fusion are described in a scientific paper published in the Journal of Physics G: Nuclear physics, 12: 213-221 (attached). This paper was received by the journal on 12 June 1985 and published in March 1986.

1. Theory proposed which explains Brigham Young University experiments

   The detailed mathematical framework given in the paper was worked out primarily by Clinton Van Siclen, the first author on the paper. The paper discusses fusion at room temperature and how this might be enhanced by increasing the density of hydrogen isotopes. The paper discusses the transition of hydrogen to the metallic state under high pressures and other technical points. One very significant concept explored in this paper is that a hypothetical particle "with mass twice that of the electron" could lead to room temperature fusion at a rate of approximately one fusion per minute per kilogram of deuterium. This is close to the actual rates observed in later experiments at Brigham Young University by Jones and colleagues, and the theoretical framework given in this early publication continues three years later to be a useful explanation for the piezonuclear effect. Indeed, this paper is often referred
to in our paper on the Brigham Young University experiments since it provides a theoretical, mathematical foundation for piezonuclear fusion.

2. "Piezonuclear fusion" coined by Steven Jones of Brigham Young University to describe cold fusion

In addition to initiating the study, Steven Jones (one of the authors) coined the term "piezonuclear fusion" in analogy to the term "thermonuclear fusion," to indicate that the proposed approach is to induce fusion by "squeezing" the hydrogen nuclei together at near room temperatures rather than by heating them to very high temperatures. (The prefix "piezo-" comes from a Greek word meaning to squeeze or compress.)

B. Brigham Young University Physics Colloquium 12 March 1986

1. E. Paul Palmer suggested geophysical applications

The paper was published in March 1986, and on March 12, 1986 some of the concepts in the paper were described by Dr. Jones at a Colloquium of the BYU Physics Department. Paul Palmer was present and associated these ideas with geological data on heat and helium-3 which are correlated in volcanoes and other thermal regions of the earth. Both heat and helium-3 are released in fusion reactions (proton-deuteron and deuteron-deuteron reactions). Dr. Palmer suggested that rock or ions in the earth might help to catalyze the fusion reaction. This creative leap is recorded in Dr. Palmer's logbook, dated March 13, 1986 in some detail (attached).

C. Report to DOE 13 May 1986

Our work on cold piezonuclear fusion was reported to the Department of Energy in the 1985-86 Annual Performance Report, dated 13 May 1986, along with three related documents: the Van Siclen/Jones paper on piezonuclear fusion, a note entitled "Experiments in cold fusion" dated 28 March 1986 by Paul Palmer; and "Comments on Catalyzed Fusion," a note by Steven Jones dated 1 April 1986. It was at this time that Prof. Jones received permission from the DOE funding agent R. Gajewski to pursue research on this aspect of cold nuclear fusion under an already existing DOE grant to Brigham Young University for muon-catalyzed fusion research.

D. Brigham Young University's experimental program

1. Planning began in March 1986

As a result of discussions generated by the Physics Department colloquium on March 12, 1986, an experimental program was worked out to test these new ideas. An important discussion meeting was held at BYU on April 7, 1986, involving Profs. Czirr, Jones, Palmer of BYU and Johann Rafelski of the University of Arizona along with student researchers. Plans for the research were extensively developed at the meeting. Prof. Rafelski had been very active in theoretical work on piezonuclear fusion since late 1985 and strongly urged the active pursuit of this experimental effort at BYU.

2. Use of Pd, Li, Al, Cu, Ni, Pt under non-equilibrium conditions, was outlined 7 April 1986 - notarized lab notebook page
Prof. Jones's brief notes from the April 7, 1986, meeting are attached showing that the metals aluminum, copper, nickel, platinum, palladium (because it "dissolve(s) much hydrogen"), and lithium were discussed as prime candidates for the process. The importance of non-equilibrium conditions was discussed; in particular, "shocked hydrides" and "electric discharge" were considered. These notes were notarized that day by Lee R. Phillips, a notary and BYU attorney, showing the importance attached to these ideas by the physicists present.

3. Geophysical evidence for piezonuclear fusion was sought in the scientific literature as early as April 1986

On April 13, 1986, Prof. Palmer noted in his logbook a number of fusion reactions to be studied, including the deuteron + lithium-6 reaction. (On March 18, he had noted the high amounts of sodium and lithium in magmas; these later became ingredients in our electrolyte solution.) On April 16, he records the finding of a paper by the Russian physicists B. A. Mamyrin, L.V. Khabarin, and V. S. Yudench [Dokl. Adad. Nauk. SSSR, 237:1054 (1978)] in which they report excess helium-3 found in various metals. This paper was encouraging to us, but we were surprised that no follow-up work was recorded in the literature.

4. Electrochemical cell built and measurements taken beginning May 1986

On May 22, 1986, our first electrochemical cell for "electrolytic infusion of hydrogen into metals" was built (see attachment from Prof. Palmer's logbook) and on May 23, D$_2$O (heavy water) was added. We looked first for gamma rays from proton-deuteron fusion using a sodium-iodide detector and found on May 27 that the foreground rate when the cell was operating was slightly higher than the background rate when the cell was not operating, but the result was not statistically significant. In June, we developed another means of loading hydrogen isotopes into metals, using pressurized gases, and added a crude neutron counter.

5. Work on a highly sensitive, energy resolving neutron detector was begun in 1986, since neutrons of the correct energy are a sure indicator of nuclear fusion

Throughout the summer, the neutron detector was worked on while different electrolytes were tried, including the addition of NaOH or H$_2$SO$_4$ to D$_2$O and the addition of "impurity salts" of various metals. We also tried loading the cathode with deuterium before beginning the electrolysis (see, e.g., 10 September 1986 entry). By September 3, 1986, we saw a foreground minus background rate of about 5x10^{-3} in the neutron counter, but the result was neither statistically significant nor consistently repeatable. However, this rate proved to be consistent with the rate obtained in later work when the neutron counter system had been dramatically improved. As the school term began anew, we concluded that in order to make progress in our work we had first to improve the neutron detector. Bart Czirr and Gary Jensen continued this work. Some of the effort went into trying to find a suitable hydrogen-rich, inorganic scintillator. While this work continued, Dr. Jones pursued muon-catalyzed fusion research.

6. Student papers presented on piezonuclear fusion experiments in March and April 1988
In January, 1988, Prof. Jones organized a student research class along with Prof. Palmer and Prof. Larry Rees. Piezonuclear fusion was one of the principal research topics, pursued by students Paul Dahl and Paul Banks. Both wrote term papers on the topic. Paul Dahl presented an oral paper at the Spring Research Conference of the BYU College of Physical Sciences and Mathematics 1988 on 12 March 1988; his paper was entitled "An Experimental Investigation of Piezo-nuclear Fusion." On March 25, 1988, we prepared some deuterated metal samples which were sent to Harmon Craig of the University of California at San Diego for helium and tritium analysis. These samples were later sent to Al Nier of the University of Minnesota, but analysis has not been completed as of 30 March 1989.

7. Further experiments planned, research program set out and pursued vigorously from August 1988

In August, 1988, the decision was taken by the scientific team to vigorously pursue experimental piezonuclear fusion research. In particular, Prof. Jones logbook records that a fusion group meeting took place on August 24, 1988, and that this matter was aired. We decided that both gamma and neutron detectors would be used. Since energy applications for muon-catalyzed fusion then appeared remote, we decided to place particular emphasis on our piezonuclear fusion research program. This decision followed work in early August by Prof. Jones in which he outlined a paper on the subject. His intent was to include a discussion of piezonuclear fusion research in his paper for publication in the proceedings of a muon-catalyzed fusion workshop previously held in Florida in May 1988, as recorded in his logbook entry dated 9 August 1988. (Prof. Jones is an editor of this Proceedings, which is publication no. 181 of the American Institute of Physics.) However, during a visit to Provo on August 15-16, 1988, Prof. Rafelski dissuaded Jones from including this work in his paper but rather encouraged more experimental studies. Dr. Jones also discussed the BYU work on this fusion process and his intentions to emphasize piezonuclear fusion studies with Dr. Alan Anderson and Dane Chapman in August and early September, 1988. Our group has vigorously pursued its experimental piezonuclear fusion research at BYU since August 24, 1988.

8. Publishable results obtained in 1988-1989

We started with gamma-ray studies since the sodium-iodide detector is easiest to set up. As before, we saw only non-significant hints of gamma production in our 3 inch sodium iodide counter, so we decided to concentrate on using the neutron spectrometer, which was fully conditioned for use in late 1988. Our first studies with this spectrometer were done using titanium, palladium, tantalum, nickel, aluminum, iron, and lanthanum. We also used several methods of loading deuterium into metals, including the original electrochemical method. Thus, we performed anew the experiment which we had started with in May 1986, namely electrolytic infusion of deuterium into metals, but with a much-improved neutron detector. Of these experiments, Paul Palmer records: "Steve and Bart have set up experiments exactly as we did a year or so ago and looked for fusion-generated neutrons in Bart's liquid-scintillator, low-resolution spectrometer.....As in the previous work, the results were tantalizingly positive." Within a few weeks, the results had reached a statistical significance of over five standard deviations. We also found correlations between tritium detected in Hawaii and volcanic eruptions there, in agreement with expectations that piezonuclear fusion occurs in the earth. We decided in early February to publish our results.
E. Discussions with scientists at other institutions (1986-1988)

Our work in this field has been communicated to a number of scientists outside of BYU in the 1986-1988 period; to name a few: Harmon Craig (University of California, San Diego geophysicist), Al Nier (University of Minnesota), Alan Anderson (Idaho Research Software), Gus Caffrey (Idaho National Engineering Laboratory), James Cohen, Mel Leon, Jim Bradbury, Richard Maltrud, Mike Paciotti (all Los Alamos National Laboratory), Russell Kulsrud (Princeton Plasma Physics Laboratory), Archie Harms (McMaster University), and Mike Danos (National Bureau of Standards).

III. Scientific Contacts with University of Utah Researchers

A. Steven Jones reviewed proposal by Pons and Fleishmann at request of DOE, 20 September 1988

According to Prof. Jones' logbook, he reviewed a proposal by Profs. Stanley Pons and Martin Fleischmann on September 20, 1988, entitled "The Behavior of Electrochemically Compressed Hydrogen and Deuterium." It is not possible from the title to determine that the proposal relates to nuclear fusion. The proposal was sent to Prof. Jones by Ryszard Gajewski, director of the Division of Advanced Energy Projects of the Department of Energy. Dr. Gajewski has funded Jones' work on cold nuclear fusion since 1982, and his specific work on electro-fusion since May 1986. For his part, Dr. Jones has reviewed about eight or ten proposals relating to cold nuclear fusion, his primary field of research. The cover letter with the proposal says nothing about declining to review the proposal if the reviewer was doing related work. Indeed, most of the proposals which Dr. Jones is asked by the DOE to review relate closely to his active research on cold nuclear fusion, including muon-catalyzed fusion. The cover letter did specify that the reviewer agrees to "use the information contained in the proposal for evaluation purposes only." This Jones accepted and acknowledges that he has abided by this agreement. The development of the project at BYU outlined above, including the use of electrochemical cells since May 1986, shows that Brigham Young University was conducting research in electrolytic piezonuclear fusion long before the review of the University of Utah's proposal. When that proposal becomes available to the public it will become obvious, if it is not already clear, that Brigham Young University researchers have not used information from that document. We adhered to our program in a straightforward way.

B. Jones offered to cooperate with Pons and Fleishmann

Prof. Jones recommended that the University of Utah's proposal be approved, despite his unresolved reservations about the theoretical underpinnings. He also suggested to R. Gajewski that he inform Pons and Fleischmann that Jones had been doing related work on piezonuclear fusion since at least 1986 and that perhaps a cooperative effort between the nearby universities (BYU and University of Utah) would be desirable. Jones pointed out that the techniques of the two efforts (e.g. neutron detection at BYU) were complementary and that the research effort could be benefitted by cooperation.

1. Pons responded with telephone call December 1988

Dr. Gajewski did inform Pons of the proposed cooperation, who in turn called Jones in (about) December 1988 to discuss the matter.
2. Jones offered use of neutron detector

In ensuing contacts, Pons requested written information regarding the neutron spectrometer which had been developed at BYU. Jones mailed him this information and offered to allow Pons to use the operating neutron spectrometer at BYU. Pons seemed pleased with the offer.

3. Pons and Fleishmann visited Brigham Young University laboratory, 23 February 1989

Finally, on February 23, Pons and Fleishmann came to BYU to visit Jones and his colleagues in the BYU Underground Laboratory. Pons and Fleishmann were shown the neutron spectrometer and the energy spectra which it produced, including calibration and actual data distributions. In particular, we openly pointed out the fusion neutron signal observed in our data. We also discussed some of our geological evidence for piezonic fusion. In the exchange of information, Fleishmann showed us one of their electrochemical cells, although we understood that this particular one was one that did not work. We invited them to bring their (working) apparatus to BYU to verify its operation with our neutron spectrometer. They agreed, and a date of February 26 was set for the test.

4. Researchers agreed to work toward simultaneous publication

Over lunch at BYU that day (February 23, 1989), Jones told Pons and Fleishmann that the BYU group was preparing to publish their data and offered to let them publish simultaneously. Dr. Jones reports that when he made the offer to allow the University of Utah researchers to publish simultaneously with the BYU report, he was attempting to establish an open and cooperative relationship.

The University of Utah researchers did not come back to the BYU laboratory to test their equipment on February 26 as agreed. Rather, they explained that morning that a graduate student had had to travel to a funeral and suggested that they would plan to come at the end of the week. But they did not come then either. Subsequently, a meeting was proposed by University of Utah President Chase Peterson for March 6, 1989 to be held at BYU with the chief scientists and Presidents of the two universities present.

IV. Contacts Between University Administrators

A. Telephone discussion between President of University of Utah and Provost of Brigham Young University, 3 March 1989

On Friday, March 3rd, the President of the University of Utah called the Provost of Brigham Young University. He made some observations about the significance of cold fusion research going on at both universities, and some of the complexities surrounding the projects. He then asked for a meeting with top university administrators and chief scientists involved in the projects as soon as possible. A meeting was scheduled for the following Monday.

B. Meeting between Brigham Young University and University of Utah administrators and principal scientists at Brigham Young University, 6 March 1989
On Monday, March 6th, University of Utah president (Chase Peterson), his vice-president for academic affairs (Joseph Taylor), and the two principal scientists (Drs. Pons and Fleischmann) involved in the cold nuclear fusion experiment at the University of Utah arrived at 9:00 A.M. to begin the scheduled meeting. Brigham Young University participants in the meeting were the president of the university (Jeffrey R. Holland), the provost and academic vice-president (Jae R. Ballif), the associate academic vice-president responsible for research (LaMond Tullis), and the principal scientist who directs Brigham Young University's piezonuclear fusion experiments (Dr. Jones).

Before the meeting the president of the University of Utah met separately with the president of BYU and his provost to discuss the agenda. It was agreed that the University of Utah's president could pursue his agenda so long as it included a brief historical summary of research done at BYU.

President Chase Peterson of the University of Utah first explained how wonderful an invention practical cold nuclear fusion would be. He also said that the large monetary proceeds from said invention could be extremely valuable to the University of Utah. Dr. Jones then held up a small flashlight and stated a strong cautionary note that he would be extremely surprised if enough power could be generated by the process to power even a flashlight, and that he could not see how in any case the proceeds from the invention could be vouchsafed for Utah. Jones then reviewed the history of the BYU research on cold nuclear fusion (at the request of Academic Vice President Ballif). He described much of the history given above. In particular, Jones showed a notarized page from his own logbook dated (and notarized) April 7, 1986, demonstrating that the metals palladium, platinum, nickel, lithium, copper, and aluminum were particularly enumerated for the BYU research on piezonuclear fusion on that date. He also showed copies of pages from Paul Palmer's notebook that demonstrate that experimental research using electrolytic infusion of hydrogen into various metals began at BYU on May 26, 1986, 2 1/2 years before we learned of the University of Utah work in this area, with first positive hints of cold nuclear fusion by this process on May 27, 1986. None of these dates were questioned or challenged by Pons or Fleischmann or anyone present, nor did anyone raise any questions about the proposal-review process. They did not allege that the BYU work had pirated any ideas from their own work. After Dr. Jones' review of detailed documents showing the BYU research over the years, Utah President Peterson turned to BYU President Holland and commented on the remarkable coincidence that such similar research had sprung up independently at the two universities.

Then the meeting shifted to a discussion of releasing the information to the public. The University of Utah researchers stated that they would prefer to have up to eighteen months to quietly pursue their research before announcing it. Dr. Jones stated that he had been funded on the research in question since May 1986, that he had positive results, that he felt obliged under the DOE grant to rapidly publish his results, and that the DOE funding agent had encouraged him to go ahead with a publication on the experimental work. In particular, Jones displayed his abstract for an Invited Paper to the Spring Meeting of the American Physical Society to be held in Baltimore, Maryland May 1-4, 1989. Jones' abstract, sent to the American Physical Society on February 2, 1989, states in part: "We have shown that nuclear fusion between hydrogen isotopes can be induced by binding the nuclei closely together for a sufficiently long time, without the need for high-temperature plasmas...We have also accumulated considerable evidence for a new form of cold nuclear fusion which occurs when hydrogen isotopes are loaded into various materials, notably crystalline solids (without muons). Implications of these findings on geophysics and fusion research will be considered."
The University of Utah contingent expressed great concern about Jones' speaking at the May meeting in Baltimore. In particular, University of Utah President Peterson suggested strongly that it would be desirable for Jones not to give the talk. Dr. Jones replied that he was shocked that Pres. Peterson would suggest that he give up an invited talk on the BYU work, and Pres. Peterson said that he would not ask Jones to cancel his talk. Instead, it was agreed that the two groups would submit papers SIMULTANEOUSLY and quickly (after about three or four weeks) in order to have the papers accepted and hopefully published before Jones' scheduled talk on May 4, 1989. It was also agreed by all that no public disclosure of the research would be made by either group prior to the simultaneous submission of the papers. In keeping with this understanding, Jones said that he would cancel a previously scheduled physics department colloquium at BYU, set for May 8 (two days later), and he did so. Jones also cancelled a talk by a graduate student (Stuart Taylor) on the BYU piezonuclear fusion research scheduled at the BYU Spring Research Meeting on March 11, 1989, in order to strictly adhere to this agreement.

In subsequent discussions between Jones, Pons, and Fleischmann, it was agreed that the precise day for the joint submission would be on March 24, 1989. On March 21, Dr. Pons called Dr. Jones and the joint submission date of March 24 was re-confirmed. Dr. Pons indicated that the University of Utah paper was already ready but assured Jones that it would not be submitted earlier than March 24. No mention whatsoever was made of the University of Utah press conference held on March 23, 1989, one day prior to the agreed date for releasing the information jointly, or of the University of Utah paper on cold fusion submitted March 11, 1989, to the Journal of Electroanalytical Chemistry by Pons and Fleischmann.

C. Agreements from 6 March 1989 meeting

After an extended discussion of what might be done to accommodate the interests of the University of Utah delegation, it was agreed by all present that:

1. Simultaneous publication

Scientists at Brigham Young University and the University of Utah would prepare and submit simultaneous publications to the same journal.

2. Publication prior to APS meeting (May 1989) where Jones was scheduled to speak

Every effort would be made to get publication prior to the American Physical Society Meeting, even though this would be difficult in the short time available. It was agreed that publication in the most prestigious physics journals would be pursued first, but if that could not be accomplished in time, the papers would be submitted to another journal. It was agreed that if necessary journals outside the field of physics would be considered including simultaneous publication in a chemistry journal.

3. Exchange of preprints

The scientists would exchange papers after they were completed.

4. No further public comments on results of research until papers submitted

No further public announcements of the results of either teams' research would be made until after the papers were submitted for publication. Brigham Young
V. University of Utah Press Conference and Subsequent Events

On March 22, 1989, we had calls from people at the Department of Energy about a press release that they had announcing a March 23 University of Utah press conference. It stated that net energy-producing cold nuclear fusion had been achieved at the University of Utah, and that a reviewer of the proposal had confirmed the result! We were shocked and disappointed by the announcement and communicated these feelings to Chase Peterson and James Brophy at the University of Utah. For example, on March 22, BYU Professor Grant Mason, Dean of the College of Physical and Mathematical Sciences, spoke to Dr. James Brophy, University of Utah Vice President for Research, and expressed to him that if the press conference were held, we at BYU would interpret this as a violation of the agreements between the two universities.

The University of Utah press conference was held there on March 23, 1989. No mention was made of the electro-fusion research at BYU. In fact, a question was asked at the University of Utah press conference: "Is this going on anywhere else, or is this the kind of process that is currently being developed by anyone else?", to which University of Utah Vice President of Research James Brophy replied: "Let's see, I'll answer it and then perhaps you can. We're not aware of any such experiments going on." It was also intimated that the University of Utah paper on their work had already been submitted, although they would not say to which journal. This was also a great shock to us at BYU. After learning of this, we could not see why we should wait until the next day to send our paper to Nature, so, after the press conference, we immediately sent our paper to Nature. We have received considerable criticism from University of Utah persons for sending our paper in on this day instead of waiting until March 24. However, it was verified to us on March 24 that Pons and Fleischmann had indeed submitted their paper prior to March 23; their preprint entitled "Electrochemically Induced Nuclear Fusion of Deuterium" contains this statement on the title page: "Submitted to Journal of Electroanalytical Chemistry March 11, 1989." Thus, the University of Utah paper was submitted prior to March 24, although we are given to understand that a paper on the subject also was submitted on March 24 to Nature.

There remains one final area to be recorded in this effort to lay out the facts. Numerous allegations and insinuations have been made and continue to be made to the effect that Dr. Jones pirated the idea of fusion in an electrochemical cell or some unspecified ideas from the University of Utah work based on his review of their proposal. For example, such insinuations appeared in a front page article of the Deseret News on April 21, 1989. Similar insinuations have been made to officials at the Department of Energy and to scientists at Los Alamos National Laboratory in New Mexico.

On or about February 14, University of Utah attorney Norm Brown spoke to the BYU attorney Lee Phillips and suggested that Jones had pirated ideas from his review of the University of Utah proposal. This was reported to Jones who reported the allegation to the DOE funding agent Dr. Ryszard Gajewski. Dr. Gajewski then questioned Dr. Pons about this. Dr. Pons apologized for the insinuations of the lawyer both to Dr. Gajewski and directly to Dr. Jones on or about February 21, 1989. We know of no further accusations or insinuations of wrongdoing against Dr. Jones until March 10, 1989, when Dr. James Brophy made allegations of this kind to Prof. John Lamb of BYU. During the week of March 14, 1989, Dr. Pons made general accusations against Jones to Dr. Gajewski.
We have provided evidence from logbooks and other sources that demonstrates that these unsubstantiated allegations are false. When the University of Utah proposal becomes available through the Freedom of Information Act, it will be completely clear, if it is not already, that our work was conducted independently of theirs. (According to agreement with the DOE, BYU retained no copy of the proposal following the evaluation.) Meanwhile, we have opened our logbooks and other documents to public view.
Piezonuclear fusion in isotopic hydrogen molecules

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Abstract. A rough estimate is made of the rate of fusion of the nuclei in a deuterium molecule at room temperature and atmospheric pressure. As the density of the collection of molecules is increased, the fusion rate will likewise increase, in a manner given by an easily derived semi-classical equation. With sufficient experimental data, perhaps obtained through use of a diamond-anvil high-pressure cell, density-dependent expressions for the internuclear potentials of isotopic molecular hydrogen can be constructed and values for the corresponding fusion rates calculated.

1. Introduction

Fusion of isotopic hydrogen nuclei is a very efficient means of producing energy in the high-temperature, high-pressure interiors of stars. In terrestrial conditions, of course, the nuclei acquire an electron and approach one another no closer than is permitted by the molecular Coulomb barrier. The probability of fusion of the nuclei in molecular hydrogen is then proportional to the probability of quantum-mechanical tunnelling through that barrier, or equivalently, the probability of finding the two nuclei at zero separation.

By replacing the electron in a hydrogen molecular ion with a negatively charged muon, the fusion rate is greatly increased. The reaction is facilitated by the reduced equilibrium internuclear separation due to the much larger muon mass. A similar, though not nearly so extreme, distortion of the molecular potential can be achieved by subjecting the molecules to very high pressures. With the possibility of creating pressures of several million atmospheres presented by the diamond-anvil cell (Jayaraman 1984) it is interesting to consider rates for fusion of the nuclei in isotopic molecular hydrogen as a function of pressure.

Separation of the centre-of-mass motion of the nuclei from their relative motion gives Schrödinger’s equation for a particle of mass equal to the reduced mass of the two nuclei, and bound in a potential well constructed from the total electronic energy and the nuclear repulsion. A classical particle moving in the molecular potential $V(r)$ depicted in figure 1 will oscillate harmonically about the point $r_0$ (corresponding to the equilibrium internuclear separation) and between the turning points $r_a$ and $r_b$. Its quantum-mechanical counterpart, described by the probability amplitude $\psi$, is not similarly constrained to lie entirely within the classical turning points, but extends into the classically inaccessible regions.

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0305-4616/86/030213 + 09 $02.50 \text{© } 1986 \text{ The Institute of Physics}
March 13, 1886  Source of volcanic heat.

Collographic yesterday by dinner trace of 84U physics set me thinking. He talked of more catalyzed self fusion—among other things such as quark search and electron-catalyzed fusion of HD molecules. He talked of spontaneous fusion under pressure (low) and catalyzed fusion (high).

\[ H + D \rightarrow ^3\text{He} + \approx 6\text{ MeV (rholm.)} \]

Well, when earth's sedimentary material at a continental margin gets pulled down in a subduction zone at a plate boundary, fusion could take place as the pressure increased. What is the deuterium concentration (D) looked on chart of isotopes. D is \(0.015\%\). If all fused, and the energy was 5 MeV that would be, per gram of water (with normal concentration of D),

\[
\frac{m [\text{g}]}{M [\text{g/mol}]} A (\text{mole of}) \frac{F (\text{ratio of})}{2} \frac{2 (\text{ratio of}) \text{H}}{\text{H}_2\text{O}} C (\text{concentration factor}) \times 5\text{MeV (mol)}^{-1} \times \frac{1\text{MeV}}{1\text{MeV}}
\]

\[
= \frac{1.9 \times 10^{-13}}{18\text{g}} \times 0.00015 \times 2 \times 1 \times 5 \text{MeV} \times 1.6 \times 10^{-19} \text{cal} \times 1.9 \times 10^6 \text{MeV}^{-1} \text{cal}^{-1} \text{mol}^{-1} \times 1.9 \times 10^4 \text{D} \times 0.015\%
\]

That is interesting in 2 million cal/g.

Suppose we take rock up to 3000°C. That takes \(1000\) cal. This gram of water could take 2 kg of rock up to high temperature. So many rocks there is certainly of the order of 1 g of water (in water of crystallization as well as interstitial water) per 2 kg of other stuff.

To measure all this, measure the \(^3\text{He}\) that outgases from the lava! Simple. Thus date must be available. The ratio of \text{H} / \text{\(^3\text{He}\)} that outgases would allow computation of the fraction of spontaneous fusions per average water molecule.

Perhaps the rock catalyzes the reaction! Separating separated \text{H} and \text{D} from water and even oxygen might catalyze.

(CONT)
Mar 13 1986 (cont.) Heat from DH fusion.

Do not sure exactly how to look at the digestion of heat in a convection cell. Suppose in a cell from a midline ridge to a trench, the entire energy was dissipated in friction. Assume energy from A to B around bottom of deep fault, then drive long and all from B to A is free to be converted to frictional energy at A.

The energy is much too small and it is 3000 $\frac{\text{W}}{\text{o}0}$ $E = 10^4 \text{ joules}$

\[ E = (10^{33}) \left( \frac{9.8 \times 10^7}{\text{cm}^2} \right) \left( 10^7 \text{ cm} \right) = 100 \text{ joules} = 25 \text{ cal} \]

This is a fair amount of a gas. $p = \frac{1}{2} m v^2 = \frac{1}{2} m \left( \frac{v}{10^7} \right)^2 = 10^7 \text{ mmHg}$

$v = \sqrt{2gh} = \sqrt{2 \times (9.8) \times (10^7)} = 4.5 \times 10^7 \text{ cm}$

This is a bubble length enough to cause water to flow but not melt, i.e. negligibly compared with fusion energy.

That is, negligibly compared with total fusion energy but maybe not compared with the rate of fusion.

\[ \text{March 14, 1986} \]

Catalogue of DT fusion in earth.

I talked to James Bass and Myron Bredt about the possibility of fusion at plate subduction regions. They were very excited about the need for local energy sources to supply the heat. We found a paper by Harun Craig on 16O in isotopic ratios at volcanic regions. It is exciting.

Where gas comes from volcanic regions, the $\text{H}_2$ isotope is 3 to 5 times that found in atmosphere and crustal rocks. Craig et al attribute this to primordial mantle helium ratios being recorded in plumes. I attribute it to the generation of He by DT fusion catalyzed by something or other. I would guess that the something or other is ions of fluorine, chlorine, and maybe hydrogen. Maybe under pressure these select out the helium.

These isotopic ratios suggest a deep earth origin of the He but at this time do not indicate the cause of the ratios. Is primordial or fusion generated? Is there a mantle probe that is not contaminated with possible fusion-generated He? How about meteorites? How about the lunar deep wells?

What is the solar wind $\text{H}_2$/$\text{He}$ ratio? Doug Jones gave me a sample of neon to look up.

The best test might be a lab experiment - squash DH with catalyst in a diamond cell. The trouble with this if I didn't work is that it might be because conditions and catalyst are wrong.

Where to proceed? - think all leads and write a proposal? Or do the work and write a patent and proposal?
Jan 4/7/86

Al (n, N_i) \rightarrow \text{hydriding}
Pt + H \rightarrow \text{dissolve}
LiF + H \rightarrow \text{hydride}
H \rightarrow \text{interstitial}

Transverse e-
- to see E from Energy - write only for dope
- cast d-d j \rightarrow g

H_2O \rightarrow \text{what in const.}

\text{Travel} \rightarrow \text{tom/O.C.}
\rightarrow \text{Nashville}

\text{Pt + Fe} \\
\text{Pt + O}_2 \\
\rightarrow \text{H}_2\text{O gas}

Max Hill - nly
Dimensions: 200 - 1000 x 1000 X 1000x 1/4
Press: \sim 60 kilobars

Detectors: prods K* - low e\% \\
\text{lif}, scint - \sim 207\% \\

CZIR

The catalyzed fusion process outlined above was explained and formulated on or prior to 4/7/86 (April)

L. R. Hill

Athens, Texas 7/12/57
May 22, 1986

Electricity, the key to life. Its ability to flow through circuits and power our lives is a marvel of modern technology.

The diagram above illustrates the flow of electricity, with positive and negative charges moving through a conductor. This is the basis for all electrical systems, from household appliances to high-tech devices.

In nature, the flow of electricity is observed in lightning and the beating of our hearts. Understanding these natural phenomena can help us harness the power of the universe for our benefit.
May 22, 1936 (cont.) Electrolytic infusion of H.  

The results were completely inconclusive. The rate with the hydrated sample was 10% to 20% greater than background. The count on the background was too short but the equipment was needed for other purposes. As Bart said — you wouldn't get an cell fraction on the basis of these results — but neither would you get against it.

We don't know if we bad any H in the copper. We are going to weigh it and take it, but that is a doubtful validity because of oxide formation and surface contamination effect.

May 23, 1936 Electrolytic cell using D₂O.  

I rigged up the same cell as before but used 10% D₂O, sulfuric acid, and distilled water with copper anode and cathode. I ran it with 200mA current and applied 1.5V across cell. Hydrogen came off the cathode in small bubbles. Ran for ~10 hrs.

Bart suggests we use palladium on the metal because we know it has the ability to let H diffuse through it readily. It should work fine.

Run cell 4½ hrs at 200mA, then cut to 100mA at 3:00 pm.

Weighed our first sample and put in furnace to drive off hydrogen and then reweigh — (or maybe we will oxidize and gain weight — who knows.) Turned furnace on at ~3:00 pm 580°C

Sample from end outside of electrolytic \[ 6.5335 \text{ g} \] before cell

Sample from end inside of electrolytic \[ 6.5335 \text{ g} \] before cell — nickel

Sample from end in cell — nickel + plated \[ 8.4300 \text{ g} \] after plated

May 27, 1936

Red reported the copper strip gained 50 mg weight — 'slightly' oxidized and the nickel-plated strip weighed the same. The furnace took so long to get hot that it only took it to 375°C.

My sulfuric acid cell produced a strange strip of copper. It finally ate up the anode and deposited copper and black stuff (CO2 (?) on cathode). A gamma assay gave nothing. 1067 background counts in 1600 sec in 3 to 5 MeV and 1149 counts in 6800 sec with Cu strip.
May 27, 1960 (cont.)

Electrolysis of Copper in Deuterated Water

The background rate is 0.162 ± 0.005/sec and this rate is in the right direction on paper even though it is not statistically significant.

We do not know if this is hydrogen in the metal. We do not yet know whether the product is a reaction.

We do not know whether pressure and/or deformation is required to bring the H-D nuclei close together for tunneling.

We will look for reactions next.