Information from the APS meeting in Baltimore, May 1-2, 1989

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Introduction and Critique

By Jed Rothwell, 2023

Cold fusion was announced by Fleischmann and Pons (F&P) on March 28, 1989. Fifty-three days later, the American Physical Society (APS) held an annual meeting in Baltimore, MD, in May 1989. It included some presentations on cold fusion. Some were during the regular session, but most were during a Special Session on cold fusion. A panel press conference was held after the special session. This document includes some newspaper articles covering the cold fusion session, the transcript of the press conference, and the abstracts submitted to the special session.

40 papers presented

Forty papers were presented at this conference. By my count:

2 experimental papers were positive, Contrell and Granada. 3 if you include Jones, who did not claim to replicate F&P.

11 experimental papers were negative. However Lewis was later shown to be positive [1]. One of the speakers discussed the experiment at Harwell which was also considered a negative at that time, but which was also reevaluated and found to be positive. [2]

5 experimental papers reported work still in the early stages with no conclusion.

19 papers were about theory. Most said that cold fusion is theoretically impossible.

2 are difficult for me to classify.

In 10 of the 11 negative experimental papers, excess heat was not measured. Only neutrons or gamma rays were searched for. Later in 1989 it became clear that without excess heat, nuclear effects such as neutrons, gamma rays or tritium are seldom found. Fleischmann said, "heat is the principal signature of the reaction." This was not clear in May 1989, so it is reasonable these researchers did not try to measure heat.

No tritium studies are reported. Several were underway at that time, and proved positive, especially at Los Alamos and Texas A&M. During the discussion, positive results by Huggins, Fleischmann and Pons and Scaramuzzi were discussed. None of these groups were present.

Most theory papers concluded that cold fusion is impossible. Some say this categorically: "cold fusion cannot occur in the condensed state under conditions employed in the reported experiments." In early 1989 this was a reasonable conclusion. By September 1990 cold fusion had been replicated in 92 laboratories, in some cases many times and at high signal to noise ratios. [3] Tritium was confirmed and in some studies the ratio of heat to helium was the same as D-D plasma fusion. That was irrefutable proof that cold fusion is real. Any theory which contradicts replicated experiments is wrong by definition. Yet not a single one of these theorists has retracted or expressed any doubt. Schwinger asked, "have we forgotten that physics are empirical?" [4] Apparently, these people have forgotten that.

This conference was too soon, and it was a rush to judgement

My principal critique of this meeting was that it was too soon. Cold fusion was announced on March 28. This meeting was held 34 days later. That was not enough time to do a careful cold fusion experiment. The technique at the time was bulk palladium electrolysis. The palladium often takes weeks to load and begin producing heat. This was known at the time.

Some researchers claimed they had done exhaustive replications. That was not possible in only 34 days. With the techniques at that time, setting up an experiment and pre-testing cathodes took months. [5, 6] One of the researchers, Moshe Gai, said: "We feel that, in fact, as far as the chemistry is concerned, we have done all possible things and basically, our results exclude without any doubt the Pons and Fleischmann results." Gai also expressed this with more caution: "[our chemists] have done everything which is possibly within the lack of information that we had . . ." That is reasonable, but why didn't they wait until more information became available? When information later became available, they did not try the experiment again.

The New York Times reported a similar claim: "Dr. Nathan Lewis, leader of the Caltech team, every possible variant of the Pons-Fleischmann experiment was tried without success." Since 1989, dozens of variants have been reported, such as electrochemical loading of thin film and gas loading. Lewis did only one of these variants. Not only that, but as noted above, when he later reported his work in detail, Fleischmann, Miles and Noninski pointed out that he made a mistake in his own calorimetry, and his data probably shows excess heat. [1] Nature refused to print letters pointing out his mistake. [7] Lewis made many unfounded critiques of F&P,

such as claiming their cell was not well stirred. Clearly, he was engaged in an ill-considered rush to judgement.

In my opinion, the entire conference was a rush to judgement. It was also a rush to condemnation. Many skeptics claimed that this conference was the death blow to cold fusion. They never changed their minds. Many in mass media and elsewhere still say this. They say that cold fusion was never replicated, even though two replications were reported at the conference, and another (Huggins) was discussed. I believe Oriani and Appleby also had positive results by this time, but the conference attendees may not have heard about them.

Everyone knew there were more experiments underway. It would have been far better to reserve judgement until they were completed and reported. In October 1989 the NSF and EPRI held a workshop on cold fusion. [8] By that time many solid results were reported, especially positive tritium results. This was a much better conference.

Why did so many researchers rush to do experiments? Why didn't they wait until more complete information became available? Perhaps they were anxious to establish scientific priority. That seems strange because most of them said they were convinced cold fusion is a mistake. Why would you rush to claim priority for a bogus claim? Perhaps this was academic politics. Perhaps they were hoping to head off the opposition. Tom Passell was the program director for cold fusion at EPRI. He said that in 1989 some prominent scientists publicly disparaged cold fusion while behind the scenes they applied to EPRI for grants to study it.

Most annoying panel member

Several of the scientists at this meeting strike me as arrogant and misinformed, none more so than Meyerhof. He made several claims in the panel discussion and in the abstract, all of them incorrect.

He began by making the same claim as Lewis, that the cell was not well stirred, so there must be thermal gradients. Quoting the abstract: "Solution of the heat equation for cylindrical calorimeters with the geometries of Ref. 1 or 2 show that in steady-state calorimetry temperature gradients exist even with weak stirring." Perhaps this would be true with weak stirring, but the cell was vigorously stirred by the bubbles from electrolysis. Meyerhof did not know this. He should have asked an electrochemist. Or, he should have conducted a test with steel electrodes in a cell of this size, at these power levels. He would have seen vigorous stirring. He could have measured the thermal gradients. Soon after this meeting F&P demonstrated that their cell was well stirred by circulating a video showing a drop of red dye falling into a cell. It was thoroughly mixed within 20 seconds. Meyerhof did not respond or retract his claims.

Meyerhof claimed that there must be thermal gradients and thermometers must have been placed incorrectly: "Hence, fictious excess power can be found, depending on the placement of the thermometer." F&P later published a detailed description of the cell showing that they used an array of 5 precision thermistors. The array could be set vertically or horizontally, measuring temperature variations in either direction. They showed that variations in the liquid were at most 0.005°C, except at the bottom of the cell, where they were 0.01°C. These steps prove that the cell was well stirred and there were no significant gradients.

Meyerhof next claimed that the heat might be from recombination. That is impossible. The excess heat exceeded the limits of recombination (that is, total input power, I*V) by a large margin.

He claimed that the heat might be caused by the difference in recombination between ordinary water and heavy water. The paper showed that F&P used the thermoneutral potential for heavy water. [9]

He claimed that Huggins et al. had the same problems. He was wrong for the same reasons. [10]

During the panel discussion, Meyerhof described another problem. He said that F&P were using a Dewar cell:

> Now the Utah group had a more refined calorimeter, namely they had a kind of thermos bottle. But unfortunately, when you look at the picture of the thermos bottle, you see that there is no more thermos bottle effect at the neck of this thermos bottle. The neck is fused together, there is no vacuum between them. And depending on how high the water is inside and outside, heat can leak out through that neck. So, they have a more non-uniform temperature in their calorimeter than you would normally have if you had a proper thermos bottle. So that's very unfortunate. And therefore, because of this temperature variation and again, because of the position of where their thermometer was, they saw something that they interpreted as excess power, which unfortunately, is not true.

Meyerhof did not know this was a half-silvered Dewar, with no silver below the electrolyte waterline. Nearly all the heat leaves the cell below the waterline, even when electrolyte levels rise and fall. So, the water level inside and outside makes no difference. F&P were well aware of the problems Meyerhof described, and they designed the cell to avoid them.

Meyerhof did not know these details because F&P did not describe the cell in detail until months later in peer-reviewed papers. It takes time to prepare and publish such papers. Perhaps it was reasonable for Meyerhof to speculate there might be such problems, but he should have presented his thoughts as speculation: “perhaps their cell is a simple Dewar, in which case it will have this problem.” Or he might have asked them about the cell. Any textbook on calorimetry says needs well defined heat loss path, which a Dewar does not have. Meyerhof should have realized that F&P would probably not make amateur mistakes in their own area of expertise. Granted, they did make mistakes trying to measure gamma rays, a technique outside of their expertise.

These issues of stirring, thermal gradients, recombination and the half-silvered cell are discussed in detail in Ref. [11].

Meyerhof was the only panel member bold enough to declare that cold fusion was definitely wrong. A reporter asked:

> Q: Doctor Rafelski indicated that he thinks there may still be just a tiny breath of life in the University of Utah experiment. Several of the panel members seem to have come perilously

close to declaring it dead. Is anyone on the panel prepared to sign the death certificate this morning?

PROF. MEYERHOF: I would.

Meyerhof let his imagination run away with him. He declared these problems are possible, and then convinced himself they must have actually happened. He was so certain of this that during the press conference he ridiculed F&P with a limerick:

Tens of millions of dollars are at
stake, dear brother,
because some scientist put a thermometer
At one place and not another.

The notion that F&P's results were caused by inadequate stirring lived on for decades in the mass media and in the minds of scientists who never bothered to read the literature. [12]

Newspaper articles

Albuquerque Journal

May 4, 1989

Chicago Tribune

May 2, 1989, Tuesday, NORTH SPORTS FINAL EDITION

SECTION: NEWS; Pg. 1; ZONE: C

LENGTH: 868 words

HEADLINE: Scientists try to put chill on fusion claims

BYLINE: By Jon Van, Chicago Tribune

DATELINE: BALTIMORE

BODY:

Scientists burned the midnight oil Monday night to pour cold water on the notion that a new way to unleash unlimited amounts of cheap, clean energy is around the corner through a process known as cold fusion.

In an unusual late-night meeting of the American Physical Society, researchers from around the country reported on efforts to duplicate claims from the University of Utah that cold fusion can be created with relatively simple materials in any lab.

"We have seen no evidence whatsoever for nuclear reactions or even for unusual chemical reactions," said Nathan Lewis, a chemist from the California Institute of Technology, whose presentation was typical of those given Monday night.

The session, which drew a large crowd to the Baltimore Convention Center, was in stark contrast to a similar meeting two years ago in New York. That time, excited physicists stayed up all night to swap stories about their successes in producing materials that became superconducting, losing resistance to electrical current, at temperatures much higher than previously believed theoretically possible.

That meeting was dubbed the "Woodstock of physics" by some, after the 1969 rock music festival. But Monday night's session lacked such joy. Scientists from such prestigi.us institutions as California Institute of Technology, Massachusetts Institute of Technology, Brookhaven National Laboratory and Yale University catalogued their skepticism about cold fusion, a subject that has been making headlines for more than a month.

Neither of the principal advocates of cold fusion, chemists B. Stanley Pons of the University of Utah and Martin Fleischmann of the University of Southampton in England, attended, although both had been invited.

On hand was Steven Jones, a researcher from Brigham Young University in Provo, Utah, who has been something of a rival to Pons and Fleischmann. Jones, working independently, has produced indications that it may be possible to produce a very low level combination of hydrogen isotopes - fusion - at room temperature.

Jones has said his results do not indicate any promising commerical applications of cold fusion.

"There is no shortcut, no royal road to fusion energy in my work," Jones told more than 1,000 physicists. Jones said that his experiments produced no heat, and that compared to claims by Pons and Fleischmann, "my reaction to theirs is like the ratio of a $1 bill to the entire national debt."

This was greeted by hoots of laughter from the physicists.

Steven Koonin, a professor of theoretical physics at Caltech, told the scientists: "Our theoretical studies indicate that the Brigham Young University results (presented by Jones) are quite improbable, but perhaps not impossible. However, we know of no way of accounting for the University of Utah results.

"If fusion were taking place, we would see radiation in one form or another . . ," Koonin said. "None of us has seen radiation above natural levels."

He added: "It's all very well to theorize about how cold fusion . . . might take place. One could also theorize about how pigs would behave if they had wings. But pigs don't have wings."

Koonin showed the physicists a graph from the publication of findings by Pons and Fleischmann that illustrated the amount of radiation their reaction produced. He said the graph does not match the known facts about radiation and speculated that it may be the result of radon decay in the laboratory.

"I don't know how much radon they have in their lab, but I do know they mine uranium in Utah," he said, again drawing laughter.

When Koonin concluded that "we are suffering from incompetence and delusions of Pons and Fleischmann," he received sustained applause.

The report by Lewis was all the more scathing because he, like Pons and Fleischmann, is a chemist.

Lewis suggested that the "excess heat" reported by the pair was misleading to the public because they gave the impression that by putting 1 volt of energy into a system, they were getting back 4 volts or more.

In fact, Lewis said, the Pons-Fleischmann claim was based on a calculation that accounted for loss of energy from gases that escape from the experiment.

If they put 10 volts of energy into the system and only got out 2 volts, they might still claim an excess if their calculations, based upon assumptions, predicted that only 1 volt or less should be produced.

At no time did Pons and Fleischmann actually get an absolute excess of energy over what they needed to run their experiment, Lewis said, although in their public statements and in testimony to Congress seeking research funds, they may have left that impression.

Lewis said he and his students attempting to reproduce the Pons-Fleischmann experiments identified several errors that could have led the pair to conclude that they had achieved fusion or some other process that creates energy.

These errors include failure to stir the solution of heavy water used in the experiment, Lewis said. Local chemical reactions do create vast amounts of heat in small areas, but if the water is stirred, there isn't enough heat to merit interest, Lewis said.

TERMS: SCIENCE; RESEARCH; GROUP; REACTION; ISSUE

Los Angeles Times

May 3, 1989, Wednesday, Home Edition

SECTION: Part 1; Page 1; Column 1; Metro Desk

LENGTH: 1766 words

HEADLINE: COLD FUSION DISPUTE BOILS; PANELISTS RIDICULE CLAIMS

BYLINE: By THOMAS H. MAUGH II, Times Science Writer

DATELINE: BALTIMORE

BODY:

The dispute over cold fusion reached the boiling point Tuesday as scientists assembled here said they were prepared "to sign the death certificate" for the fusion-in-a-flask experiment and one Nobel Laureate said the head of the University of Utah, which backed the research, "ought to be fired."

But a university official defended chemists B. Stanley Pons of the University of Utah and Martin Fleischmann of the University of Southampton in England and fired back with charges of "hand waving" and "Eastern elite" bias.

The 40 papers submitted for presentation at an American Physical Society meeting here variously ridiculed, questioned and doubted the Utah-group's conclusion that they were able to produce more energy than was consumed in their simple fusion cell and traced a litany of purported errors in their research. The errors, they said, included failure to stir the liquid in the flasks and possible radon seepage into the experiment.

Few of the assessments were delicate.

Caltech physicist Steven E. Koonin summarized the feelings of many researchers here when he concluded that their results were based on "incompetence, and perhaps delusion."

And an indignant Leon Lederman, director of the Fermi National Accelerator Laboratory in Batavia, Ill., said that University of Utah President Chase Peterson "ought to be fired" for his role in promoting Pons' and Fleischmann's claims.

James J. Brophy, the university's vice president for research and development, dismissed the charges as "a lot of waving of hands." He said that such criticisms are to be expected "because of the obvious importance of the technology" and said that the physicists, particularly the "Eastern elite," have a vested interest in protecting their own fusion research funds. He added that supportive evidence would be presented next week at a meeting of the Electrochemical Society in Los Angeles.

The consensus of most observers was that, unless Pons and Fleischmann can produce some dramatic new evidence to support their contentions, their claims are likely to fade into obscurity along with polywater, N-rays and other highly publicized scientific "breakthroughs" that were subsequently discredited.

The University of Utah also drew fire for unabashedly hyping Pons' and Fleischmann's findings, its failure to ensure that their results were scientifically sound and its effort to obtain $25 million from Congress for a cold fusion research center before the work had been replicated. Several researchers said that the university has been greatly embarrassed by its role in promoting the cold fusion fever and bypassing normal scientific channels in an effort to obtain research funds.

Visit to White House

Meanwhile, the principals in this monthlong scientific saga, perhaps oblivious to the latest torrent of criticism, prepared to meet today with Bush Administration officials at the White House.

A University of Utah spokeswoman said that Pons and Fleischmann were in Washington preparing for the meeting and that they would have no comment on the charges. They were invited to appear at the Physical Society meeting, but declined because of their speaking and research commitments.

The panel also said that it could not yet render a verdict on the claims by physicist Steven E. Jones of Brigham Young University that he had observed a much smaller level of cold fusion than Pons and Fleischmann. Jones has made no claims of excess energy production and has repeatedly argued that his results offer no immediate hope of commercial energy production.

Pons and Fleischmann startled the world March 23 when they announced that they had discovered a hitherto unknown fusion reaction that worked at room temperature and produced more energy than it consumed -- a feat that has eluded physicists and their multimillion-dollar fusion machines for decades. They said that the extra heat could be obtained by simply applying a small electric current to palladium and platinum electrodes immersed in deuterium oxide -- the so-called heavy water in which each hydrogen atom is replaced by deuterium, which has one extra neutron.

Unlimited Energy

They said deuterium ions would be forced by the electric current to enter the palladium electrode, where they would fuse to form helium, releasing heat in the process. Their results held forth the promise of unlimited, inexpensive energy that could be produced from the deuterium in seawater.

Other scientists were immediately skeptical of their claims because the simple fusion cell produced only extremely small amounts of the radiation that should have resulted from a fusion reaction. Nonetheless, scientists throughout the world rushed into their laboratories to attempt to reproduce the Utah findings, working 16-to 20-hour days seven days a week.

Several groups throughout the world have claimed to confirm the Utah results in part, although some have subsequently had to withdraw their claims. But the research presented here this week has cast doubt on some of those claims as well.

Groups from most of the major U.S. energy research laboratories, such as the Oak Ridge National Laboratory and the Brookhaven National Laboratory, reported that their studies with the most sophisticated scientific equipment available failed to show any evidence of nuclear fusion or excessive heat output. Physicist Douglas Morrison of the European Center for Nuclear Research said that even the Harwell Research Laboratory in England, which had used a fusion cell obtained from Fleischmann, had been unable to find any evidence of fusion.

Said physicist Moshe Gai of Yale University, who worked with the Brookhaven group: "Our results exclude without any doubt the Pons and Fleischmann results."

Studies at Caltech

The strongest evidence against the Utah results, however, was provided not by a physicist, and not by the Eastern elite, but by chemist Nathan Lewis of Caltech. Like other researchers reporting here, Lewis and his colleagues had undertaken exhaustive efforts to reproduce the Utah results and had obtained no evidence of fusion or excess heat production.

But Lewis went one step further and re-evaluated Pons' and Fleischmann's research strategy and calculations of energy efficiency. He says that they made several mistakes, including basing their claim of excess energy production on hypothetical numbers rather than actual measurements.

"We could find no evidence for anything other than conventional chemistry" in their results, Lewis concluded.

The critique by Lewis was so devastating that, even though his was only the fourth of 20 papers scheduled for presentation Monday evening, more than half of an audience estimated at more than 2,000 left after his presentation. By the time the litany of negative results reached its conclusion at 12:20 Tuesday morning, only a handful of observers remained in the meeting hall.

Problems Listed

Among the problems, according to Lewis and others:

* Pons and Fleischmann reported that they measured gamma-radiation emanating from their apparatus, a sign that nuclear fusion was occurring. But Koonin and others noted that the frequency of the gamma-radiation they observed was characteristic not of fusion, but of radon, a byproduct of uranium that is commonly found in basements such as those used by the Utah researchers. Said Koonin: "I don't know how much radon they have in their lab, but I do know they mine uranium in Utah."

* Pons and Fleischmann cited the presence of helium in their apparatus as proof that fusion had occurred. But Lewis noted that helium is present in fairly large amounts in the air of most chemistry laboratories because liquid helium is used to cool many instruments. The amount of

helium they observed, Lewis said, was at least 10 times higher than the amount that would have theoretically been produced by fusion, indicating it was a contaminant from the air.

* Pons and Fleischmann did not stir the heavy water in their cell, arguing that bubbles formed at the electrodes would circulate the water. But Lewis and Walter Meyerhof of Stanford University showed that if the water is not stirred the temperature measured in the cell was dependent on placement of the thermometer, indicating the presence of "hot spots" in the cell. Temperature measurements made near the electrodes would indicate heat production, Meyerhof said, while measurements at the edge of the cell would indicate heat consumption. The same problem was present in cells used by Robert Huggins of Stanford, who recently reported a confirmation of the Utah results.

* Pons and Fleischmann and other researchers, particularly at the University of Florida, have reported the presence of tritium in their cells as an indicator that fusion occurred. But Lewis noted that chemicals in the cell can interfere with the measurements and make it appear that tritium is present when it is not. Neither the Utah nor the Florida researchers took these reactions into account.

* Pons and Fleischmann did not actually obtain excess heat production in their cell, as they had implied. Rather, they had calculated, based on a faulty assumption, that they would recover 13% of the energy they put into the cell and they actually got 20%. Their calculations that they could obtain four to 10 times as much energy as they put in the cell, he added, were based on the use of a hypothetical number with no basis in reality – as Pons conceded in congressional testimony last week.

Believed It Wrong

After Lewis' talk, an unidentified researcher from the Los Alamos National Laboratory, which has been working closely with Pons, rose from the audience and noted that "We felt he had done his energy wrong."

Although several groups reported that they had also not been able to replicate Jones' studies, which involve a complex "soup" of salts in the heavy water, most noted that they had not attempted to do so because he had not claimed excess energy production. Lewis carefully noted that the Caltech group had not studied Jones' system, but were now doing so.

During Tuesday's panel session, Jones displayed a small shoot growing in a flask. "The Utah people have claimed this is a tree ... and it is going to grow up very quickly and give us enough wood to provide all our energy needs for generations," he said.

"I don't think it's a tree, and I told Congress it doesn't need a lot of fertilizer right now," he continued. "But this little sprout is still living and I do think it will grow into a pleasant flower, a new addition to the garden of physics."

SUBJECT: FUSION ENERGY; PHYSICS; RESEARCH; UNIVERSITY OF UTAH; PONS, B STANLEY; FLEISCHMANN, MARTIN; EXPERIMENTS

The New York Times

May 3, 1989, Wednesday, Late City Final Edition

SECTION: Section A; Page 1, Column 1; National Desk

LENGTH: 1847 words

HEADLINE: Physicists Debunk Claim Of a New Kind of Fusion

BYLINE: By MALCOLM W. BROWNE, Special to The New York Times

DATELINE: BALTIMORE, May 2

BODY:

Hopes that a new kind of nuclear fusion might give the world an unlimited source of cheap energy appear to have been dealt a devastating blow by scientific evidence presented here.

In two days of meetings lasting until midnight, members of the American Physical Society heard fresh experimental evidence from many researchers that nuclear fusion in a jar of water does not exist.

Physicists seemed generally persuaded as the sessions ended that assertions of "cold fusion" were based on nothing more than experimental errors by scientists in Utah.

Furor on Initial Claim

Dr. B. Stanley Pons, professor of chemistry at the University of Utah, and his colleague, Dr. Martin Fleischmann of the University of Southampton in England, touched off a furor by asserting on March 23 in Salt Lake City that they had achieved nuclear fusion in a jar of water at room temperature.

At a news conference today, nine of the leading speakers were asked if they would now rule the Utah claim as dead. Eight said yes, and one, Dr. Johann Rafelski of the University of Arizona, withheld judgment.

Top physicists directed angry attacks at Dr. Pons and Dr. Fleischmann, calling them incompetent, reciting sarcastic verses about their claims and complaining that they had refused to provide details needed for follow-up experiments. A West European expert said "essentially all" West European attempts to duplicate cold fusion had failed.

Response at Utah University

In a telephone interview, Dr. James Brophy, director of research at the University of Utcth, responded, "It is difficult to believe that after five years of experiments Dr. Pons and Dr. Fleischmann could have made some of the errors I've heard have been alleged at the American Physical Society meeting."

The criticism at the regular spring meeting of the society came just before Dr. Pons was scheduled to meet with representatives of President Bush and just after the University of Utah asked Congress to provide $25 million to pursue Dr. Pons's research. A university spokesman said Dr. Pons was in Washington and could not be reached to answer questions.

Cold fusion, Dr. Pons and Dr. Fleischmann said, can be initiated in a cell containing heavy water, in whose molecules the heavy form of hydrogen called deuterium is substituted for ordinary hydrogen. When current is passed through the heavy water from a palladium cathode, the Utah team said, the palladium absorbs deuterium atoms, which are forced to fuse, generating heat and neutrons.

Fusion, which powers the sun and hydrogen bombs, normally occurs only at extremely high temperatures. If a means could be found to harness a form of hydrogen fusion as a commercial source of power, some scientists have said, energy shortages could be forestalled.

Some of the new experiments also sought to reproduce the less contentious findings on cold fusion reported independently by Dr. Steven E. Jones and his colleagues at Brigham Young University in Utah. Dr. Jones, who used a device similar to the one in the Pons-Fleischmann experiment, did not claim that any useful energy was produced. But he did report that slightly more neutrons were detected while the cell was operating than could be expected from normal sources. The result suggests at least the possibility of fusion, he said, although it is not likely to be useful as an energy source.

Physicists who have investigated Dr. Jones's report have restrained in their criticism, acknowledging that Dr. scientist. But from the outset they have expressed profound skepticism of claims by Dr. Fleischmann and Dr. Pons.

Attempts to Repeat Experiments

Since March, scores sought to repeat the cold fusion experiments, and investigations just hours before the meeting was convened here Monday.

The most thoroughgoing of the attempts to validate the Pons-Fleis experiment was conducted at the California Institute of Technology. According to Dr. Nathan Lewis, leader of the Caltech team, every possible variant of the Pons-Fleischmann experiment was tried without success.

Using equipment far more sensitive than any available to the Utah group, Caltech failed to find any symptoms of fusion. The scientists found no emitted neutrons, gamma rays, tritium or helium, although the Utah group reported all these emissions at high levels. And all the cells consumed energy rather than produced it, the Caltech team said.

The Caltech team intentionally reproduced experimental errors leading to the same erroneous conclusions reached by the Utah group, Dr. Lewis said. By failing to install a stirring device in the test cell, temperature differences in the cell led to false estimates of its overall heat, he said. This may have suggested to the Utah group that its cell was producing fusion energy.

Presence of Helium in Test

Noting that Dr. Pons and Dr. Fleischmann had also reported the presence of helium, a fusion product, in the test cell, Dr. Lewis said his group had also found helium. But helium is a trace component of air, and the amount of helium in the cell corresponded to what normally enters from the atmosphere.

"Pons would never answer any of our questions," Dr. Lewis told an audience of 1,800 physicists, "so we asked Los Alamos National Laboratory to put our questions to him instead, since they were in touch with him."

Other scientists said they had tried every possible variation of the Utah experiments.

Dr. Edward F. Redish of the University of Maryland, chairman of the meeting, said that 10 days ago he telephoned Dr. Fleischmann to invite him to participate in the Baltimore sessions and answer criticism.

"He told me that Dr. Pons would try to come," Dr. Redish said. "But just before the meeting Dr. Pons let us know that he would be too busy discussing cold fusion with a Congressional committee to come to Baltimore."

A spokesman for the University of Utah said Dr. Pons was preparing to meet with members of Mr. Bush's staff Wednesday.

Failure to Elicit Information

Many speakers at the meeting reported failure in their efforts to elicit information or comments from Dr. Pons. Dr. J. K. Dickens of Oak Ridge National Laboratory in Tennessee said that to duplicate the cell used by the Utah group, his laboratory had been forced to estimate its size.

"One published photograph of the Utah cell showed Pons's hand, and that gave us the scale," he said. Dr. Lewis said his group had also used the photograph showing Dr. Pons's hand as a measure of the cell's size. But Oak Ridge Laboratory, like Caltech, failed to find any evidence of cold fusion after it had built and tested the cell.

Physicists asked Dr. Lewis if he could account for the burst of heat that Dr. Pons reported as having destroyed one of the Utah cells.

"My understanding," Dr. Lewis said, "is that Pons's son was there at the time, not Pons himself. I understand that someone turned the current off for a while. When that happens hydrogen naturally bubbles out of the palladium cathode, and creates a hazard of fire or explosion. It is a simple chemical reaction that has nothing to do with fusion."

Other Reports of Failures

Among other major research groups that gave details today of experiments failing to validate the Pons-Fleischmann results were representatives of Massachusetts Institute of Technology, Lawrence Berkeley Laboratory in California and the University of Rochester. Before the meeting, a joint research group of Brookhaven National Laboratory and Yale University also reported failure to find evidence of the existence of cold fusion.

Dr. Douglas R. O. Morrison, a physicist representing CERN, the European scientific consortium for nuclear research, reported that "essentially all" West European attempts to duplicate the Pons-Fleischmann experiment had failed. The entire episode, he said, was an example of "pathological science," in which an erroneous experiment initally gained some support, then prompted skepticism and finally led to denunciation.

Most of the initial support has eroded. The Georgia Institute of Technology withdrew an early report that it had partly confirmed the Pons-Fleischmann experiment.

At Stanford University, Prof. Robert A. Huggins repeated the Pons-Fleischmann experiment several weeks ago, and obtained results that seemed to suggest fusion. But Dr. Walter E. Meyerhof, professor of physics at Stanford, told scientists Monday night that he had carefully studied his colleague's apparatus and found that the experiment was flawed because of the system used to measure heat. Nevertheless, Dr. Huggins, a materials scientist, said in a telephone interview that he is "more confident than ever" in his results.

While most critics of the Utah work limited themselves to discussion of experimental results, some directed their ire at Dr. Pons and Dr. Fleischmann themselves.

'Incompetence and Delusion'

Dr. Steven E. Koonin of Caltech called the Utah report a result of "the incompetence and delusion of Pons and Fleischmann." The audience of scientists sat in stunned silence for a mement before bursting into applause.

Referring to a possible error in temperature measurements by the Utah group, Dr. Walter E. Meyerhof of Stanford University offered this contribution:

Tens of millions of dollars at stake, Dear Brother, Because some scientist put a thermometer At one place and not another.

Dr. Brophy of the University of Utah said the Utah team, like all other scientific groups, welcomed criticism by other scientists.

"Any scientist can be proved to be slightly in error or greatly in error," he said. "If Dr. Pons and Dr. Fleischmann have made errors they will acknowledge them. But so far none of their critics have published their criticisms, and they are conducting science by press conference, as we have been accused of doing."

Dr. Brophy said his group was not disturbed by the vote by eight of nine physicists calling the Utah experiment dead. "Pons and Fleischmann will be speaking themselves next Monday at a meeting of the Electrochemical Society in Los Angeles, and the vote there would be likely to be different," he said.

Dr. Jones himself spoke at the meeting, and although participants questionedhim sharply about his experiment, questioning was generally friendly.

He drew cheers and laughter when he concluded his talk by saying, "Is this a shortcut to fusion energy? Read my lips: No!" He defended his own experiment, describing his results as a "fragile flower" that would never grow into a "tree" producing useful energy, but could nevertheless "beautify" science.

Some critics, however, continued to insist that Dr. Jones's results also stem from experimental error rather than fusion.

Dr. Dickens of Oak Ridge noted that Dr. Jones had used relatively crude neutron-detecting equipment, and had measured only a very small excess of neutrons over what could be expected from natural sources without any fusion.

SUBJECT: Terms not available

Transcript of the press conference

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AMERICAN PHYSICAL SOCIETY PRESS CONFERENCE
ON COLD FUSION
PANALISTS:
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PROFESSOR JOHANN RAFELSKI, UNIVERSITY OF ARIZONA
PROFESSOR STEVEN E. JONES, BRIGHAM YOUNG UNIVERSITY
PROFESSOR STEVEN KOONIN, CALIFORNIA INSTITUTE OF TECHNOLOGY
DR. RICHARD BOYD, OHIO STATE UNIVERSITY
DR. EDWARD REDISH, UNIVERSITY OF MARYLAND
PROFESSOR WALTER E. MEYERHOF, STANFORD UNIVERSITY
DR. DOUGLAS R. MORRISON, CERN
DR. JAMES A. KRUMHANSL, AMERICAN PHYSICAL SOCIETY
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BALTIMORE CONVENTION CENTER
KEYWORD: AMERICAN PHYSICAL SOC. P.C. COLD FUSION -05/02/89

BODY:

MR. SCHEWE: Let me introduce myself. I'm Phillip Schewe of the Public Information Division of AIP. I want to welcome you to this APS conference news on cold fusion. First of all, I'd like to thank the reporters, particularly the television reporters for their forbearance last night. It's difficult to cover a story with a camera if you can't be in the room with your camera. I want to also plug, it was a very good meeting for several reasons. We have a full slate of news conferences on many different subjects such as supernova pulsar, and nuclear physics, astrophysics.

The format for today will be the following. I'll introduce Dr. Krumhansl, the President of APS who will say a few words, and then Dr. Edward Redish of the University of Maryland who was the organizer of the cold fusion session last night and the moderator, and he'll say a few words about what happened last night. And then, beginning with the far end of the table there, Dr. Meyerhof, we'll work our way down the table. If each of you speakers could summarize your paper in five minutes, and if you would please hold your questions until all of the speakers have made their five minute statements, and then we'll open the floor to questions.

I hope we can accomplish all of this in an hour or an hour and a half at most. Many of you were at the session last night. Reporters, when you ask questions, if you would please identify yourself as you ask your question so that the speakers will know who they're responding to. So, Dr. Krumhansl, would you please begin with your statement?

DR. KRUMHANSL: As President of the American Physical Society, I'm sure that you expect the standard kinds of laying on of hands and saying welcome and all those good things. But I would also like to take the opportunity to say something about the Physical society and its major focus and how that gets implemented in the kind of a forum that you've been participating in here, and I hope we will continue whenever there is something of importance scientifically in which the public has an interest.

The purpose of the American Physical Society is to advance the knowledge of physics and to diffuse the knowledge of physics. Now, that's somewhat of a stilted term. But what we really mean is educate ourselves and educate anybody in area of science where we can make a contribution. That means that we follow the philosophy of having totally open meetings, and we publish, and we have all of the information open to the public. We publish the referee journals. And when there's a service to be carried out, we hope we can do that in the way this special conference on cold fusion has been organized. But in addition, I'd like to just spend a few more minutes telling you about the nature of the physical society. There are almost perhaps 15 -- and it keeps growing -- different sub-disciplines. We have not only nuclear physicists and particle physicists and plasma physicists, but we have material scientists, we have chemists -- we have a division of chemical physics. We have people who are trying to bridge across not only our own sub-disciplines, as Joe Redish can tell you, but also, we have interfaces with many other scientific societies. And part of this is interesting philosophically and substantively. One of the interesting things that's happening these days, is that major problems have many different component questions -- some of those are nuclear, some of them are chemical, some of them are theoretical in a broad sense -- and that's why these forms are so important. In particular, for example, I would hope that some of you who were there last night recognize how constructive the interplay between the chemists at Cal Tech and the physicists and the other participants at the various individual papers were in promoting this objective which we have. Thank you.

DR. REDISH: I'll just take a minute to tell you a little bit about the organization of the session. I'm not a practitioner in this field, so I'll be brief so as to get you on to the heart of the matter. We organized an impromptu session on cold fusion. We received invited and contributed papers. We received 40. So, they ran over. We will have another session tonight. The session tonight will be mostly papers received late over the weekend, so some of them may be late-breaking results. I command that to your attention. I think we had about 1,800 people last night at the session -- that was my estimate. The only thing I should mention, that some people have asked me about the absence of a representative from the Utah group. I can only say that they were invited with the first cut immediately. Dr. Fleischmann said that he hoped that one of them would be able to attend, but after reconsideration, they decided their schedule was too busy and they would not be able to make it, unfortunately. I think that's all that I need to say.

PROF. MEYERHOF: Thank you very much. I think I should say at the beginning that --

MR. SCHEWE: Introduce yourself, please.

PROF. MEYERHOF: I'm Walter Meyerhof. I'm professor of physics at Stanford University. I want to say at the beginning that I don't think one should look at this meeting or the session that we had as any conflict between physics and chemistry. I think all of us work together to accomplish something, namely to understand nature.

Now, I analyzed, or I tried to analyze, with the help of two of my friends at the Stanford SRI International, it's called now, Don Lorents and David Huestis -- I analyzed the results of Fleischmann and Pons in terms of the physics have not done any experiments.

Now, the essential point that I found and where I think the error occurred in their experiment, and that's my conclusion, that they have made an error in the experiment, is the following: If you have -- imagine this cup is a calorimeter and in the middle there is this palladium electrode -- of course, this is a very primitive representation of what's going on -- then it turns out there are two sources of heat in this set-up. One source is that the palladium -- at the surface of the palladium, there are some electro-chemical reactions that warm up the palladium. And this warming up may be different between heavy water and ordinary water, and therefore some people find a difference in their result between heavy water and ordinary water.

The other source of heat is due to the fact that there's a current going through the electrolyte in the center that fills up this calorimeter. This is – the electrolyte means it’s the solution of water and some conducting material. Water would normally not conduct. You have to put something into it and it doesn't make very much difference what you put into it, like salt or lithium hydroxide, any -- in fact any chemical you put in the water will conduct. And that conductivity of heat, the palladium surface and the electrolyte.

Now the method by which the power is measured, is that a thermometer is placed into this calorimeter at some point. And unless you are very, very careful, and you have an extremely good calorimeter which is isolated from the outside; for example by putting a vacuum wall between -- in other words, like a thermos bottle, you make a calorimeter like a thermos bottle -- you will find there is a temperature variation between the middle, which gets warm, and the outside which is kept cool because this whole thing is placed in a bath of water; as you have no doubt seen on TV.

Okay, now. The thing is the method of calorimeter depends on the fact that the temperature is uniform in this entire water. Otherwise, you make the following finding: If you put your thermometer very close to the center electrode, you will find that the water is quite warm there; but the rest of the water is relatively cool, because outside here is this constant temperature bath. Therefore, you overestimate the power and you would think there is an excess power.

On the other hand, if you put the thermometer against the wall of the calorimeter, you would find a deficiency in power. You might call that defusion [sic.] -- in other words, you find a deficiency in the apparent power that has been generated. Now, I believe the thermometer -- I'm sorry, I believe the calorimeter of the Stanford group in the Mechanical Engineering Department under Professor Huggins, had a very simple calorimeter consisting just of a glass. And therefore, they did find what they call excess power because at the position of their thermometer, it turns out the average temperature is too high at that point. Now the Utah group had a more refined calorimeter, namely they had a kind of thermos bottle. But unfortunately, when you look at the picture of the thermos bottle, you see that there is no more thermos bottle effect at the neck of this thermos bottle. The neck is fused together, there is no vacuum between them. And depending on how high the water is inside and outside, heat can leak out through that neck. So, they have a more non-uniform temperature in their calorimeter than you would normally have if you had a proper thermos bottle. So that's very unfortunate. And therefore, because of this temperature variation and again, because of the position of where their thermometer was, they saw something that they interpreted as excess power, which unfortunately, is not true.

So I can summarize the findings of my calculation a little limerick, which I hope you don't take too seriously, although I think it has -- the whole thing has very, very serious consequences, it has upset the whole country; government agencies have been upset. So my limerick is the following: Tens of millions of dollars are at stake, dear sister and brother, because some scientists put a thermometer at one place and not another. (Laughter.)

DR. MORRISON: My name is Douglas Morrison. I come from CERN in Geneva, that's the European laboratory for particle physics. And to explain how I came here, I -- (inaudible) -- some of the experiments in the Fermi (?) Lab and we have a worldwide network; there's 8 European laboratories, 6 American, and 2 Indian laboratories. And I have the habit, when something interesting happens in physics news, of sending an electronic mail newsletter.

So Professor Fleischmann, Martin Fleischmann came to CERN and gave a wonderful lecture and everyone was very enthusiastic. This was the beautiful dream of how to get lots of energy with no pollution. Everyone loved it; great. So I sent out a news bulletin with some worries. But then we started checking up and then we got finally Martin Fleischmann's -- Fleischmann and Pons paper, and became more and more worried. So, since then, I've sent out a series of 11 of these news bulletins to all our colleagues. Originally it was only for the -- our own collaborators. But since there was no other information available on the Fleischmann work, these have gradually been copied and distributed around via electronic mail. They are an informal network. They're meant to be academic, purely confidential.

So people keep phoning me, because since I give out, I get more and more in and hear in great detail what people are doing even though they don't want it published, and then I filter it. So, it's only for academic people. I don't give it to the press; I turn down banks and oil companies who have also been asking for it -- (laughter) -- but that's the way it goes.

Now -- so here, there's three points I want to make, because I've been asked to give a survey talk this evening and put it in historical perspective. One is the results from Europe, because there's very few Europeans here because of shortage of time; secondly, the historical perspective; and thirdly, the question of the regionalization of the results.

The European results; there are quite a number of very good experiments being done in Europe, particularly in Harwell (?) where Martin Fleischmann is a consultant. They, however, will not publish at the end of the month -- month of May, but they have told me all the results in detail. And essentially, they are all "no." No for neutrons, no for calorimetry, no for tritium, no for gamma rays.

The other experiments, like that we did in Bushay (?), where again you find numbers which are something like five orders of magnitude less than the Fleischmann and Pons number of neutrons, which are two orders of magnitude less than the number of Jones (inaudible)

In Italy, on the other hand, the Italian National Center for Alternative Energy Sources, Professor Scaramuzzi (ph) has produced some results, loop phased tracking. And at first it looked very impressive. But then, when we started studying them and looking at them, one begins to worry. So, I've been talking with him a bit on the phone and we've been exchanging messages. And there are worries. Unfortunately, he had to publish very quickly before he had time to do his checks. So, I think these result from Fuscati (?) -- we have to wait until it had checks.

The next point is historical perspective. For the last dozen years or so, I have been interested in wrong results in physics or pathological science. And Irving Langmuir as the father of pathological science, and they've been extending -- giving various lectures on this, where you can have sort of 12 characteristics of wrong results. And normally if something turns out to be wrong, there's about six or eight out of these 12 criteria that are fulfilled. We're now at the stage -- well, one of the stages -- you find there are three phases. Phase I is when the original results are produced, and then there are some very fast confirmations. Stage II is when you have an approximately equal number of correct results -- of confirming results and negative results. Then Stage III is an avalanche, where all the results are negative.

The reason that you have the first result, when people do an experiment, if they agree with the first publication that had all the press conferences, they say, "Great, we can publish. We don't need checks." So, they publish. If, on the other hand, you get a negative result, then you worry. You say, "Have I made a mistake?" Or you say, "Is there some special secret that these people have that I don't know about?" -- you're scared to publish. So, in this Phase II -- and the Le Figaro for example just published the other day, saying, "Here are 26 experiments, and it's 13 against 13 --13 confirmation, 13 not." Now, the numbers keep changing, particularly since last night's meeting. (Laughter.) So, that's historical.

The other part is the regionalization, because when I collected all these results and started looking at them, I was very struck that the results depend on what region of the world they come from. If you're from Switzerland, Britain, France, Germany, all the results are negative. On the other hand, from Italy all the results are positive, because the Italian newspapers are absolutely filled with very constructive, encouraging things – the beautiful dream, they've been taking out patents. This is now an Italian discovery. They use pressure instead of electrolytic processes, and things like that. Eastern Europe, all the results that I have seen are a confirmation. In Asia, Latin America, it's all confirmation. In the United States, you find that the northeast region -- for the New York Times -- is very much appreciated (laughter) -- and I hope there's a New York Times reporter here -- the results are all negative. In the big centers for research like Los Alamos, Berkeley -- it was also negative.

So, there is this very interesting regionalization of which remarkably closely reflects the attitude of the papers. And to give you an example, the popular perception is different, that a Don Perkins (sp)e, who's a professor in Oxford, said to his class, "Well look. Let's go to up-to-date physics.

You've all heard of cold fusion. I will give you odds of 100 to 1 that is wrong." And not a single one in the class was willing to bet a pound against -- to win 100 pounds off of his professor.

So, the information is much better displayed, whereas in the state of Utah, it seems the other way around. In Utah, I'm told, all of the newspapers only print positive results. I'm sure there are exceptions. But nonetheless, there's a different atmosphere in Utah from the rest of the United States.

So -- conclusions -- two. Firstly, this situation was summed up very well by a couple of hundred years ago by David Hume (sp) who was Scottish like myself, a very famous philosopher, when he said, "Would you rather believe that all of the laws of nature were wrong or that one man has made mistake?" And the second conclusion, I think, is that really if you wanted the least possible solution, I think the correct answer to that hot fusion with a temperature of 100 million degrees, that people like Jech and Tokamack (ph) are working on.

DR. BOYD: Can you hear me? (Tests mike.) I guess. I'm Richard Boyd from Ohio State University. At Ohio State, we have a collaboration which confirms the fact that physicist and chemist can indeed work together, and even with metallurgists once in a while because we have all three groups, in fact, in our collaboration. We set out to do an experiment primarily to test the Fleischmann-Pons result at the University of Utah, and the reason for that was primarily that it looked like it would be easy to confirm or reject that result without a great deal of effort.

The preparation -- the chemical preparation of the electrodes seem to be very important in the paper that they wrote. And so, we spent a great deal of time worrying about the chemical preparation and the question here is whether or not you're really loading up the palladium with as much hydrogen as you need in order to bring about the effects that Fleischmann and Pons claimed. We feel that we did a very careful job of that and we feel that even had a confirmation of that because with a very slight provocation, after we had finished one of our experiments, one of our electrodes lit up like a light bulb.

What we set out to do was basically test the neutron fluxes which were acclaimed by the Pons-Fleischmann group and also which -- and in another sense, would have been required if what they were really observing in the way of heat output was explained by cold fusion. It was quite easy to disprove the second part of that, to actually check their neutron levels took a little more effort, but our level of sensitivity is well below that which would be implied and is stated in their publication.

We also tried to test another assertion which came from a group -- a theoretical group -- at Livermore, namely that muon-catalyzed fusion could be responsible for what was going on, and that if this were the case, each muon captured by a deuterium nucleus would be responsible for perhaps several hundred fusions. If that were the case, one would expect to see nuclear signatures resulting from the muon-catalyzed fusion emitted with a falloff in time which would look like that of the muon, that is, about two microseconds.

So, we set up our experiment with our neutron detector and a shield which went around the neutron detector which would detect muons coming through to look for this sort of characteristic time delay. Again, when one compares the yield with the electrolytic cell inside the apparatus and without it, a foreground to background comparison, we see nothing to indicate that muon-catalyzed fusion is occurring.

Finally, we checked on other result. This is preliminary result, but we see no reason why it will not hold up. Some of our chemists at Ohio State have a very sensitive mass spectrometric technique and they are quite capable of examining our electrodes after the experiment to check for mass 3 and mass 4 nuclids in them. One of the problems with experiments of this type that have been at other places is that it's rather difficult to distinguish between a deuterium molecule, which has a total mass of 4 and a helium 4 nucleus. This technique that our chemists have can easily distinguish between those two things to an extremely tight level of sensitivity, far below that which would be implied by the Pons-Fleischmann experiment. We find no mass 3, that is, no tritium, no helium 3 and no mass 4.

PROF. KOONIN: My name is Steven Koonin. I'm a professor of theoretical physics at the California Institute of Technology, currently on leave at the Institute for Theoretical Physics, the University of California in Santa Barbara. I'm one third of a team of three Cal Tech faculty members who've been working on the cold fusion problem since the initial announcement on March 23rd. The other two members are Professor Charles Barnes, who's a professor of physics, and Professor Nate Lewis who is an associate professor of chemistry. Professor Lewis gave a talk last night which some of you may have heard. Because neither of my experimental colleagues are here, I will divide my remarks into two parts, one discussing some of the theoretical calculations and the second discussing the Cal Tech experiments.

In the last month or month and a half, I've been involved in some theoretical work attempting to understand, really, two problems in this business. The first problem, which is common to both the Utah and Brigham experiments, is, how do you make any fusions happen at all? Michael Nowenberg (ph), who's a professor of physics at Santa Cruz, and I did some calculations in, if you like, a benchmark system the hydrogen molecule. We calculated the fusion rates for various isotopes of hydrogen in molecules.

Our results showed, first of all, that the rate for the DD reaction, which is the one that's been claimed so far, is roughly 10 to the 8th or 100 million times faster than the last calculation of that.

Second, we find that the rates for the various reactions can be different from what one would have expected naively. In particular, the fusion of protons with deuterium proceeds significantly faster than does the fusion of deuterium with deuterium. And so, if the experimentalists are seeing anything, we'll believe that that's what they should be seeing.

Finally, our calculations suggest that one needs to compress the molecule, or change the distance scale if you like, by between a factor of 5 and 10 to explain the BYU or Utah results respectively.

In my second theoretical work, I offered some speculations on how fluctuations in a solid could enhance the fusion rates over what one would have expected naively. And I find that it's not impossible, although perhaps implausible, that one could boost the rates up sufficiently to explain the numbers of fusion claimed in the BYU experiments or the Utah experiments.

On the other hand, the second problem is a real problem, and that's the problem of how do you hide the radiation while still having all these fusions going on to produce the heat that's claimed in the Utah experiments. Let me emphasize that this problem is specific to the Utah results. I considered various ways. I won't list them for you in detail, but the bottom line is that I can find no way to hide the radiation within the known laws of physics.

Let me then turn to the experiments. Let me emphasize that the experiments were done by my colleagues, Charles Bowens (sp?) and Nate Lewis (sp?), as well as a team of 15 graduate students, post-Docs, and technicians. They attempted to reproduce as closely as possible the results published in both journals and pre-prints by Professors Pons and Fleischmann from the University of Utah. In particular, they looked for three forms of radiation that would be characteristic of nuclear processes going on. They used a neutron counter which was 100,000 times more sensitive than would have been necessary to see neutrons if they were present at the level claimed by Pons and Fleischmann. They used photon counters, very sensitive, with very high resolution, to detect radiation from 50 kilovolts to 30 MEV, spanning the entire range that would be expected if there were any nuclear processes going on.

Finally, they also used conventional scintillation counting to look for tritium that would be produced if there were fusion. In addition, they also did a very careful job of calorimetry, measuring the heat produced or absorbed as one does electrolysis. This included a continuous calibration of the cells and a thorough mixing of the cells during electrolysis.

In their experiments, they were careful to try to vary all of the experimental parameters. They used palladium from four different suppliers. They annealed the palladium. They work-hardened some of the samples. They cast some of the samples. They ran electrodes of various diameters. They also heated some of the electrodes before electrolysis to drive out any hydrogen that would have been in there. They varied the pH of the electrolyte. Some of their runs lasted for more than 2-1/2 weeks. They also varied the ratio of protons to deuterons in their electrolytes in order to look for the P&D fusion which Nauenberg and I suggested might be more important.

The bottom line is that they see nothing anomalous in any of their experiments. In addition, we've also done a careful analysis of the Pons-Fleischmann pre-print and the Pons-Fleischmann-Hawkins publication. In this we were aided by conversations with many scientists, both physicists and chemists, all around the world. We believe that we've identified serious errors in each of the three members of the triad of results that Pons, Fleischmann, and Hawkins claim support the existence of heat generated by fusion.

In particular, they detect neutrons, or claim to detect neutrons by the presence of gamma rays. The gamma ray line that they show comes at the wrong energy from what one would have expected from neutrons. In fact, it comes at an energy which could be interpreted as due to radon. And the level of the line, the intensity of the line, is consistent with (what) one might expect to be a radon background in Utah. In fact, anywhere. There are other interpretations of that line as well, but none of them support the existence of neutrons.

Second, as you heard from Professor Lewis last night perhaps, the calorimetry is very difficult to unravel from the publication, but I think Nate succeeded in doing that. Basically, what he told us, is that these guys have made a bad fusion refrigerator, rather than a good fusion heater. Namely, the cell was getting cooler, but it was not getting as cool as expected. Finally, there are serious problems when you do scintillation counting to look for tritium, associated with chemical interferences from chemilluminesents (sp?), and that could be one explanation of the tritium that they're claiming. In any event, they're not claiming a spectacular amount of tritium.

These three claims, having been dealt with, I think we've raised the serious issues about each of them, they at least in my mind, seriously undermine the scientific foundations of their claims in the publication. In fact, couple that with the inability of many people around the world to reproduce the results, many of whom bring to bear expertise and resources far greater than were available at the University of Utah. Some of whom, even had Professor Fleishmann as consultants, as we heard. Nobody's been able to reproduce it. That, together with a lack of a theoretical basis, namely I can't find anywhere to hide the radiation, suggest to me that the experiments are wrong.

Steve reminds me of something that I had meant to say, and forgot, mainly the Cal Tech experiments address only the University of Utah results. They don't address the BYU results.

(Scattered laughter.)

I believe that that's a serious thing that we'd like to get across to the public. There are really two very different experiments -- experimental results being claimed here. The experimental results I reported, deal only with the Utah results. My experimental colleagues hope to have something to say about BYU shortly. On the theoretical side, as I said last night, I find the BYU results theoretically dubious but not impossible. I find the University of Utah results theoretically impossible.

PROFESSOR JONES: Good morning. Let's see, is this coming on? I'm Professor Steven Jones from Brigham Young University. We've been working in this area of cold nuclear fusion, the discussion at hand today, for about four years, starting with a theoretical paper which we published in 1986, and then experiments which we started in May of '86.

I'd like to say in that regard, that our experiments do not confirm the University of Utah results. They had been conducted entirely independently of that work and indeed, our work goes back to '86, and of course, predates any knowledge by far, by several years, at least 2-1/2 years of the work at the University of Utah. And of course, in a moment, I'll reiterate what Steve has said, and others last night, about the difference in level between the two experiments. They're worlds apart in the levels claimed.

First of all, I would like to say that at this meeting of the American Physical Society, it's pleasant to have you looking over our shoulder to see how science works. The scrutiny process is under way here in a big way, as you can tell. our abstract -- the one I submitted to this society, the American Physical Society, in February -- my abstract is entitled "Cold Nuclear Fusion: Recent Results and Open Questions." This is on page 1228 if you -- (laughs). But interestingly enough, this was submitted February 3rd to the American Physical Society and I think is one of the earlier submissions on this topic. I reported last night on experiments conducted by -- at Brigham Young University, also experiments that we are collaborating on with the University of Bologna. This is in the Gran Sasso Laboratory. Both experiments, both at Brigham Young University very recently, we've taken more data, and these are consistent with our previous results. We are using a much simpler electrolyte solution. That is, we use heavy water now with palladium lithium salts only, an acidified solution. It's quite distinct from the University of Utah approach, but -- and it's quite a bit simpler than that which we reported in our Nature paper.

University of Bologna-BYU collaboration has conducted experiments at the Gran Sasso Laboratory. These will be published in, I trust -- submitted to a journal anyway. They should be published within a couple of months. Again, we find confirmation there.

I would say, though, that it's not only Italy that has confirmation -- now this is confirmation at the BYU level -- I will explain that in just a minute -- not at the University of Utah level. Last night, Dr. Seliger (sp?) -- apologies for not knowing German. Is it Seliger?

MR. : Zeliger (ph) .

PROF. JAMES: Zeliger, thank you. The Technical University of Dresden of West Germany reported observation of neutrons at a rate of .1 per second. The average rate published in our Nature paper -- this is a copy of our Nature paper which was published last week -- our level is about .06. In fact, here it is .06 per second, so -- that's a production rate again. So, that's a production rate again. So, those levels are fairly close then.

I would also like to comment -- let's see -- well, I'd like to comment on the difference of the levels. Now, let's take this penny and let's let this represent the BYU level. Okay. Now, the claim -- and I'm going to hide this somewhere. Now, the claim -- because it's not easy to find one penny in a room this size. Now, the claim of the University of Utah is, relative to this penny, about a trillion pennies. Okay. We're talking about heat or about 100,000 pennies if we're talking about their claim to neutron levels. Now, I think you can see a trillion pennies would be hard to stash in the room. Anyone could find it. One hundred thousand pennies you could find, I think, fairly easily. All right. You could use a simply metal detector, you'd find if very easily. A single penny hidden anywhere in this room might be quite difficult to find. It takes specialized equipment which we developed over the past three years to look at this process. And we do see the signature for fusion, but at a very, very small level. You see the difference? Am I getting through? Before I compared our level with a dollar bill, in which case, the University of Utah heat claims would represent enough money, enough dollar bills, to pay off the national debt ten times, as a matter of fact. So, it's quite a distinct difference.

With regard to the press in Utah, it is true that they have been very favorable to this notion that cold nuclear fusion provides a shortcut to fusion energy. On the other hand -- I should clarify, Dr. Morrison, they have given me some time. I had to take a couple of reporters aside and say, "Please, can't you get this difference straight, you know, the difference in levels? And please, I've been trying to express caution about these claims of fusion energy for a long time."

The comparison I used at -- for Congress last week, which I'll use again today is this: This little plant, this little sprout, is a little bigger than the one I had for congress. This represents cold nuclear fusion, this little sprout. Now, it's just sprung out of the ground, really, even though we've been working on it for four years, it still has just come before the scrutiny of the scientific community, and it's been scrutinized very hard.

Now, some people, the University of Utah people in particular, have claimed that this is a tree, they're sure it's a tree -- quite sure. And it's going to grow up very quickly and give us enough wood to provide all our energy needs for generations. I don't think it's a tree. I've said that repeatedly and consistently, don't sell your oil well, you know. Don't even invest in palladium. But, I mentioned the contrast. I don't think this -- the tree needs a lot of fertilizer right now and I realize that has a little double-entendre, but what I meant was -- (laughter) -- but what I meant was that there is a lot of fertilizer out there. But what I meant was, it doesn't need a whole lot of money right now. It just needs to grow and we need the standard level of grants to check this out and see what happens -- but, clarifying that point. However, I do think that the -- that little sprout is living, based on the work that I've done. It's a very tiny sprout. I don't think it will grow into a tree, but I do think it will grow into a pleasant flower, a new addition to the garden of physics, which we can delight in because of its beauty, not necessarily because it provides wood, but because it gives us a picture of nature which is unique and new. Thank you.

PROF. JOHANN RAFELSKI: I am Johann Rafelski. I am professor of physics at the University of Arizona. I have had the pleasure of collaborating with Steven for a number of years in cold fusion, and we had about 4 years ago decided to look at this particular version of cold fusion, the metal-hydride fusion.

I will summarize, briefly, my theoretical remarks of yesterday and offer some additional comments as to where we stand today. I see, in my lecture at least, great opportunity for a new field which unifies many of the areas of physics and perhaps, even material sciences. It is the search for materials which would lend themselves to relatively large rates of fusion. Now, at present, the search is like the search of Edison for -you try something out and one thing works, one thing doesn't work. We see this very tiny tree right there and --

PROF. JONES: Flower.

PROF. JOHANH RAFELSKI: Flower, yes. (Laughter.) And, clearly, -- as you see, we have very small effect on opinion, but very, very small. Clearly, we -- it has been [an] accidental discovery, although we went in this direction on purpose, we recognize the potential, the set up which we have designed has been totally accidental. It is therefore necessary to understand it now why it works, theoretically, what are the essential elements, why we are seeing this very low rate of fusions. And if we understand it, then to see if we can create environments in which this small flower can become a blooming, large rose garden.

Now, perhaps this is a very optimistic assessment where we can go, but I should like to remind you that there is history to the subject. The history is that actually for about 60 years, people have been looking at these forms of cold fusion. And there have been remarks in the scientific literature as recently as ten years ago that certain anomalies of helium abundance in metallic foils could perhaps be due to in situ fusion reactions. Now, we have been very worried about these remarks, because the required fusion rates would be some place in between, between the two fusion experiments. And so obviously, something should be happening at a much greater level than we are currently seeing.

So I offered yesterday some number of possible scenarios as how this could happen. And the bottom line of the theoretical remarks is what a static situation, a piece of metal which you load with hydrogen and leave alone is never going to give you a lot of fusion. So, a parametric study which I did with some collaborators has shown convincingly that the fusion rates required will not be reached. But on the other side, we know that even a piece of metallic hydride is a living object, that you can squeeze it, you can bang into it, there can be waves propagating through it. It is electro-magnetically active.

And at this level of dynamic situation, a fusion rate which could explain our experiment and which perhaps could be of interest could perhaps explain the spotty abundances of helium which we have had about could be arrived at. So the theoretical work is perhaps exactly at that level -- I think it's a very nice picture. We're just starting and to jump now to conclusions that we know anything about it, I would think, is much too early.

One thing I know is that a new field is being just now born. Your interest and the interest of our peers shows that everyone has a perception that here is something which has to be studied much more thoroughly in a way that is appropriate for the scientific community.

And let me add one thing which I added -- I told one reporter yesterday. Science is about knowing, not about believing. It's unlike social environments. We have to know. To know, one has to proceed, there are well established paths. And you have to allow us to do it. The amount of attention that we are receiving right now prevents us from actually coming to a conclusion in our own leisurely fashion. We are humans and that's human nature. And with the attention we are receiving, some of us may be led to announce preliminary results and these get propagated in the community. And even if these preliminary results were to be not correct, they will be treated as if correct and then the puzzle becomes much more difficult because it is not only that we do not know some pieces in the puzzle, but we have some wrong pieces in our hands. So, I think it would be much better for the field if the attention would diminish, if I may put it that way.

Now, just as a last remark to the situation as I see it as of yesterday. There were several lecturers which have very strongly criticized our competitors. I should Jike to add that I find myself in a position that I need to defend my colleagues chemists. And the reason is that you cannot really -- I have been in the world of science for 20 years, roughly. I have seen -- I have been sometimes. controversial and I have seen my colleagues criticize me. And I know that the only way one can deal with is by being pleasant and being able to point out that things which are brought together from different corners of a particular circumstance are really not fitting. In other words, it is the presence of a person -- of a scientist -- who is being accused of wrongdoing which is essential in the process of clearing up a field. And I think therefore that yesterday's session, although it has certainly dented the credibility of our competitors, should not be taken as meaning that it is now -- the matter is settled. I would warn against that. And the basic reason for saying so is that they were not there to defend themselves, to be asked questions, and that each time -- and the lack of theoretical interpretation is in my judgment not -- it is -- if the experiment were to be true, it would be then taken to mean that people who worked on the theory were not good enough to find a solution, which has happened often enough in the past. So, it's a question of have the experimental criticisms -- I mean, funded -- I mean, the results or assumptions for interpretation? Have these been without assumptions? And the answer is no. I carefully listened to the lectures yesterday, and because of the lack of information provided by the Utah researchers, everyone had to make crucial assumptions about what has really been done. And in my judgment, if they were there, in each instance they would have objected. I don't know if the objection would have been substantial, but I am quite persuaded that they would have objected and therefore, without them being present, we cannot dismiss the subject matter, although, as I said, I think their credibility, which has not been -- which I considered from the beginning as being limited, has been dented by the amount of criticism which I think is justified. They should have provided us with information needed to actually verify or repeat or otherwise deal with their results.

So, their credibility has been dented. But I think their story is far from over at the present moment. Thank you.

DR. GAIe: My name is Moshe Gai, and I am an Assistant Professor of Physics at Yale University, and I spoke yesterday for the Yale-Brookhaven collaboration. We presented our result yesterday at the meeting.

Our experiments started somewhere around 3-1/2 weeks ago. It was about the time when we received the first papers which came, like everybody else got them, on FAX machine, where you couldn't possibly read anything on the paper aside from the big handwritten note on it, "Confidential. Do not copy." (Laughter.) When we read these papers, it really left us with a lot of worries. We are nuclear physicists. In our laboratory, we have been doing nuclear physics for years and years. Myself, I've been measuring neutrons for years. And it was very clear to me that inadequate equipment was used by Pons and Fleischmann. It was very clear to me that the nuclear physics part of the program was not done in a very careful way, and we felt that it would be almost our responsibility to carry out a good experiment that will cover the nuclear physics part. Another thing that we learned from Pons and Fleischmann is that one should not do something that one doesn't understand, and we right away decided that we need chemistry on board. We need good chemists on board. We cannot do the experiment unless we have people who thoroughly understand the chemists, who can collaborate with people who can thoroughly understand the nuclear physics. At that time, my collaborator, Jack Greenberg, Full Professor Rackel (ph), have brought to my attention that people in Brookhaven are working on this subject. And a contact was made with Calvin Lynn (ph), who is a director of materials science department in Brookhaven, and in fact, about two days after we started assembling our experiment, they came up with a sample, and an experiment started running.

What I can say about our experiment, and I don't know how much details you would like to know, but basically, we feel that we handled the background very well. We did it in several ways. We feel that the shielding that we have put was adequate. We took something of the order of two or three days just to study the sources of background in our experiment and how to remove the background. We feel that we handled the cosmic rays very well by looking on very large Vecol (?) counters. And, in fact, in some of our spectrum, after several hours of counting, we see no background and just a few neutrons on top of this background.

As far as the chemistry is concerned, again I'm not in a position to present to you a full account of this but I can tell you that my chemist colleagues -- I have the full trust in them -- they have done everything which is possibly within the lack of information that we had and, in fact, even more than that within the network of scientists that got together and exchanged information. We have done all the tricks of the trade -- coldworking, annealing in vacuum, annealing in argon. At some point, there was a suggestion that maybe fusion will be ignited by a catastrophic event, something like an alpha particle penetrating through the palladium. So we went to my home and took my smoke alarm and took the alpha source in the smoke alarm and tried to ignite fusion in one of our electrodes. We got negative results.

We had tried a similar experiment to the one which was done at Fuscalli (?) except that we didn't go to low temperature. We went to a pressurized electrode -- a pressurized powder at high temperature. We used titanium; we used palladium; we used several electrolytes with very pure water -- 99.8 percent, 97.5 percent. We used the Jones solution; we used the lithium-hydride solution. We feel that, in fact, as far as the chemistry is concerned, we have done all possible things and basically, our results exclude without any doubt the Pons and Fleischmann results. Concerning the BYU results we feel that the results are at a level -- such a low level that we really now have to do a lot of thinking whether this is an effect which comes directly from the cell or is it something which is a little bit more delicate. We are now at such a low level that a lot of things can contribute, and we really have to look at it very carefully.

And some of you members of the press would know that our experiment, under extreme pressure from the press to reveal what we have, I think, because those of you who read accounts of our experiment -- we're very proud that we have never let anything go out to the press even though we've known about these results for quite some time. There was a conscious decision, together with the Chairman of the Department of Physics at Brookhaven, Peter Bond, we have decided that we will not go out to the press and yield any of our results, even though, you know, being only 60 miles away from Manhattan, it's very easy for somebody from any magazine to hop to our lab and try to get results out of us, and basically, the results were discussed in what I would call scientific channel. Last Friday, I gave a seminar at Yale, and yesterday was the first time that we discussed it in public. Thank you.

MODERATOR: Are there any questions for any of the speakers?

Q Doctor Rafelski indicated that he thinks there may still be just a tiny breath of life in the University of Utah experiment. Several of the panel members seem to have come perilously close to declaring it dead. Is anyone on the panel prepared to sign the death certificate this morning?

PROF. JONES: Do you have it?

PROF. MEYERHOF: I would.

PROF. JONES: Could I do something? Could I extend this as a straw poll? I mean, could we do, if no one minds, I wonder if we could take two votes. First, have the -- (laughter) -- that's good, I appreciate that. This is the gentleman from Utah, ty the way, which is good. And he's getting at the facts here. Here we go, and it will be published in the Utah press and so, we're sure to -- but let me, if l could, just -- I thought this would be fun. If you wouldn't mind gentleman, how many would be willing to say at a -- let's say a 95 percent confidence level, okay, because obviously -- they know what I mean. You know roughly what I mean. You know, the weather, it's going to sun, a 95 percent chance. Okay, it's going to be sunny. All right, how many would say that the chances that the heat claimed -- let's put it the other way, that we can rule out, at the 95 percent confidence level at this time, that the heat claimed by the University of Utah, that that should be due to fusion. How many think we can rule that out at this time? Almost, that is fairly complete. All right. Now, how many -- let's be fair, let's put BYU on the line here too. How many at the 95 percent confidence level would say that we can rule out the production of neutrons at the low level reported in the BYD-University of Arizona paper.

MR. :Ummmmmmm

DR. MORRISON: Let me try and explain what I mean -- (Scattered laughter) -- I think Steve Jones gave a very nice analogy, saying that Fleishmann and Pons had a beautiful tree, and you look for that, you find it very easily. He has a little sprout here, and he claims that's very hard to look at. Now, there are many experiments who have much better apparatus, that they have devised, particularly for looking for neutrons, which not merely would find a sprout easily, but would find a seed. And several of these experiments have not found any seeds.

Q We heard some strong statements, bad scientist is one of them, scientific flock is another one. We've also heard some very light-hearted Limericks about this research. At what point do you think there needs to be some talk of misrepresentation? At what point is the crossover to possible fraud?

DR. MORRISON: May I try answering that, because to me, this is a perfectly natural phenomena that's happened several times. I mean this is not the first example this has happened, I mean there are many famous examples in history. One of the best studied one is Henri's. We went back and read all of the original French papers in 1902-1903 . And it very often happens that your people with a first class reputation, both Steven Jones and Martin Fleischmann I have met, and I find that they are excellent physicists. And they have a very good reputation, and particularly for Martin Fleishmann, I have met several --

Q Chemists – scientists

DR. MORRISON: Scientists, thank you. That they have excellent reputations, have been impressed by the way their friends are exceedingly loyal to them. Now, there is a sort of very general problem that as a scientist, you are trained to be very careful and to pay attention to results.

There is a French phrase called "deformation professionelle" which I don't know to translate it into English. But it means that everyone has a great spectrum of character, if you're good people, bad people, careful people, enthusiastic people; but nonetheless because of your scientific training, you tend to go in a certain direction; but there are still all of these extremes. And it very often happens that someone makes a mistake, it's very enthusiastic, and it happens. Now, these people usually keep quiet about afterwards, and it blows over, and they can often do very well. But it is not abnormal, it's just human nature and one should be prepared to accept it. But what we have to do, is to try and devise a system where the community -- the scientific community can examine these problems reasonably quickly before they get out of hand and start going to Congress and things like that.

Q Question. If we want them to make an honest mistake and perhaps to get -- in the excitement of the moment, to maybe over-promote or over-sell what you found. This is quite another thing to go to Congress and ask for $25 million in a week.

Are there any --

DR. MORRISON: I think maybe one should qualify this. I mean, up to now --

PROF. KOONIN: (Off mike), please. You know, it seems to me that scientists are not particularly well qualified to judge human behavior.

[UNIDENTIFIED PANELIST]: That's right.

PROF. KOONIN: We're all well qualified to judge science. We can present the facts to you. You can observe what people do, and then I think it's for you to draw the conclusions.

[UNIDENTIFIED PANELIST]: Yeah.

DR. KOUMHANS: I'd like to make a comment, if I could. Just following that up, I think what we're seeing is again, as well put by Dr. Morrison, that we're humans, we're professionals. And what you have seen is science in action in a very professional way now over the last month in particular. It's time for the headlines to change. It isn't that physicists challenge cold fusion. Now we're evolving and trying to bring in all of the people who should be examined, and the headlines should say "Chemist-Physicist Teams Examine Cold Fusion." That's the kind of thing that's happening.

Now -- and we can continue to do this and eventually, with all the facts out -- and I hope that the Pons-Fleischmann experiments will eventually be given every chance for examination and advocacy. But that's different. Scientists are different. When we do our science, we shouldn't be making evaluation of motives; and that's a different matter.

Abstracts submitted to the Special Session on Cold Fusion

1

Cold Nuclear Fusion: Recent Results and Open Questions

S. E. Jones, Brigham Young University

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. For example, muon-catalyzed fusion occurs rapidly when negative muons are added to liquid deuterium-tritium mixtures, forming small muon-bound d-t molecules that fuse in picoseconds. Recent experimental results illuminate the rich tapestry of processes that constitute the muon catalysis cycle, while a number of questions remain yet unresolved [1]. 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.

Supported by the U.S. Department of Energy, Advanced .Energy Products Division

[1] S. E. Jones, J. Rafelski, H. J.M onkhorst, eds. "Muon Catalyzed Fusion 1988", AIP Publication 181, pp.1-469 (1989).

Abstracts for Cold Fusion Session
Baltimore Meeting, 5/1/89 7:30 PM

INVITED PAPERS

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2

Cold Fusion: Can it be True? A Theoretical Point of View

J. Rafelski, University of Arizona, Tucson

It is shown that the fusion rates observed by the BYU team of S.E. Jones during electrolytic infusion of hydrogen into Pd and Ti cathodes can readily be explained by combination of standard nuclear physics data and WKB penetration integrals in the metal lattice environment. A specific mechanism for the process invoking formation of Bose macroscopic state (drop) of deuterium ions neutralised by an electron cloud will be described.

State of the attempts to skew the branching ratios of nuclear reactions by 12 orders of magnitude towards processes not involving production of neutrals (neutrons, gammas) will be given. This would be needed to account for production of heat without penetrating radiation in a nuclear process, as suggested by the press release of the University of Utah.

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3

Theoretical Issues and Problems Raised by Cold Fusion Experiments.*

S. E. Koonin, Institute for Theoretical Physics, UCSB.**

I will discuss several challenges to our current understanding posed by recent cold fusion experiments. In particular I will review calculations of the rates for various hydrogen fusion reactions in molecular and condensed matter systems. I will also discuss the potentially large effect of lattice fluctuations on fusion rates in solids. Finally, I will review the shortcomings of various proposals to "hide" the radiation produced ind + d and p + d fusion.

*Supported by the National Science Foundation, grants PHY86-04197 and PHY88-17296.

** On leave from the California institute of Technology.

Abstract for an Invited Paper
for the Baltimore Meeting of the
American Physical Society
Special Monday Night Session
May 1, 1989

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4

Calorimetry, Neutron Flux, Gamma Flux, and Tritium Yield from

Electrochemically Charged Palladium in D20

Nathan Lewis, Charles Barnes, and Steve Koonin, California Institute of Technology

We report the results of our work on cold fusion using palladium. We have used extremely sensitive neutron, gamma ray, and photon counters, and can place strict upper limits on the flux of expected nuclear products emitted from charged Pd cathodes. Liquid scintillation counting has been used to measure tritium production, which was found at background levels for extended periods of time. However, a subtle chemical interference that generates chemiluminescence has been shown to yield tritium signals and lead to overestimates of the fusion yield based on tritium production. We have also performed accurate, calibrated calorimetry, and have identified several serious errors that can make the measurements appear to show excess power production. When these common errors are eliminated, a correct energy balance is obtained. We will also discuss the calorimetric experiments performed by the Utah researchers, will explain their calculations to the physics community, and will clearly state the assumptions and corrections implicit in the Utah calculations.

Calorimetry, Neutron Flux, Gamma Flux, and Tritium Yield from Electrochemically Charged Palladium in D_2O

Nathan Lewis, Charles Barnes, and Steve Koonin

California Institute of Technology

Pasadena, CA 91125

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5

Boson Screening of Deuterium in Metals

K.B. Whaley, University of California, Berkeley

We analyze the role which bose nuclear statistics of deuterium can have in enhancing local density fluctuations and coulomb screening deuterium in metals. Results of boson tight binding calculations for D in Pd are used to assess the feasibility of rate enhancements for D-D nuclear fusion, due to boson screening and lattice fluctuations. The possible relevance of a bose condensate, and implications for experimental observation of cold nuclear fusion of deuterium in metals are discussed.

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6

An Investigation of Cold Fusion using a Sensitive Neutron Detector

W.K. Brooks, D.G. Marchlenski, J.D. Kalen, M.S. Islam, M. Kaitchuck, R. Mccreery*, R.N. Boyd, P. Holbrooke, H. Dyke, The Ohio State Univ

A careful measurement of neutron production from a Pd electrode in an electrolytic cell has been performed. The neutron detection system consisted of a BC 501 liquid scintillator contained in a 4.0 cm thick, 18.5 cm dia. pyrex cylinder, surrounded by a plastic anticoincidence shield and lead housing. Pulse shape discrimination was used to identity neutron signals. This system yielded low backgrounds with approximately 1% counting efficiency. Initial results indicate no neutron production over a period of about 40 hours of counting. Estimates will be presented of how this may be compared to previous data. Further plans for more detailed studies of cold fusion will be described, including chemical analyses of the palladium electrode.

*Department of Chemistry

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Suggested title of session in which paper should be placed: Special Session on Cold Fusion.

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*Department of Chemistry

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7

Search for Neutron Production in a Palladium-Heavy Water Electrolytic Cell*

R. Hirosky, E. Buchanan, J. Jorne, A.C. Melissinos, and J. Tom, University of Rochester**

We have searched for neutrons produced in an electrolytic cell filled with heavy water (D20) and having a Palladium cathode. We set a limit of 1 count/sec from 0.7 cm3 of Pd, operated continuously for five days at a current of 2A. This limit is $4X10^4$ lower than the rate claimed by Pons and Fleischmann1 for a similar cell.

* Submitted by A. C. Melissinos

** Supported by the DOE and the NSF.

[1] M. Fleischmann and S. Pons, paper submitted to Journal of Electroanalytical Chem., March 20, 1989.

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Abstract Submitted for the
1989 Spring Meeting of the
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1-4 May, 1989

Suggested title of
session in which paper
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Cold Fusion

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(X) Special Session on Cold Fusion

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8

A Search for Cold Fusion Neutrons at ORELA

D.P. Hutchinson, R.K. Richards, Ca. Bennett, C.C. Havener, C.H. Ma, F.G. Perey, R.R. Spencer. J.K Dickens, B.D. Rooney, Ornl*; J. Bullock Iv and G.L Powell, Y-12 Development

A number of experiments were begun on 29 March 1989 to look for neutron emission from a palladium cathode in an electrolytic cell using a deutrated electrolyte. Several different electrode configurations were tried. The fast neutron detector utilized a pair of NE213 scintillator/photomultiplier pairs in a shielded enclosure. Data will be presented on the efficiency and background level of the detector system. At present no neutron counts above the background level have been detected.

*Operated by Martin Marietta Energy Systems, Inc. for the U.S. Department of Energy under contract No. DE-AC05.840R21400.

Abstract Submitted
for the Spring Meeting of the
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Baltimore, Maryland
May 1-4, 1989

<u>A Search for Cold Fusion Neutrons at ORELA.</u> D.P. HUTCHINSON, R.K. RICHARDS, C.A. BENNETT, C.C. HAVENER, C.H. MA, F.G. PEREY, R.R. SPENCER, J.K. DICKENS, B.D. ROONEY, <u>ORNL</u>* J. BULLOCK IV, and G.L. POWELL, <u>Y-12 Development</u>--A number of experiments were begun on 29 March 1989 to look for neutron emission from a palladium cathode in an electrolytic cell using a deutrated electrolyte. Several different electrode configurations were tried. The fast neutron detector utilized a pair of NE213 scintillator/photomultiplier pairs in a shielded enclosure. Data will be presented on the efficiency and background level of the detector system. At present no neutron counts above the background level have been detected.

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9

Analysis of "Excess Power in Cold Fusion"

W. E. Meyerhof, Stanford University,* D. L. Huestis and D. C. Lorents, SRI International

The apparent excess energy release of 4 MJ in heavy-water electrolysis with Pd electrodes [1] is impossible to explain with known chemical or physical processes. Solution of the heat equation for cylindrical calorimeters with the geometries of Ref. 1 or 2 show that in steady-state calorimetry temperature gradients exist even with weak stirring. Hence, fictious excess power can be found, depending on the placement of the thermometer. This is particularly severe in Pd+D electrochemical reactions because the dissipative part of the 0.8 to 2 V overvoltage [1] releases heat at the surface of the Pd electrode. The observed differences between ordinary and heavy water [2] can also be explained because for Pd+H the overvoltage is much smaller than for Pd+D.

[1] M. Fleischmann and S. Pons, J. Electroanaly. Chem. 261,301 (1989).

[2] A. Belzner, U. Sischler, C. Crouch-Baker, T. Gur, G. Lucier, M. Schreiber, R. A. Huggins, to be published.

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Abstract submitted
for the Baltimore meeting
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Special session on
Cold Fusion

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*Supported in part by NSF grant PHY 86-14650.

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9A

Generation of 0-0 Fusion-Reaction Bursts in Metal Deuterides

H. Furth, S. Bernabei, S. Cowley, and R. Kulsrud, Princeton Plasma Physics Laboratory, Princeton University.*

The emission of D-D fusion neutrons from "cold" objects could be due to bombardment by bursts of energetic deuterons. One key test of this interpretation is the consistency of the observed neutron-count statistics with the predicted Poisson distribution for intense, short neutron bursts. We find that the data shown in Fig. 2 of Ref. 1 are fitted perfectly by this mathematical model. The smaller count rates of Ref. 2 do not lend themselves to as sharp a statistical test-though perhaps serving to exclude "large-burst" theories such as cosmic μ- meson catalysis. A possible means for the acceleration of deuterons is mechanical fracture-as in the reported generation of neutrons during impact of high-velocity projectiles on lithium deuteride crystals. Repeating the experiments of Refs. 1-3 with mixtures of hydrides and deuterides could provide a measure of the relative importance of quantum-mechanical tunneling versus simple cold- target bombardment.

*Work supported by U.S. D.o.E. Contract No, DE-AC42-76CH03073.

[1] A. DeNinno, et al., submitted to Europhysics Letters.
[2] S. E. Jones, et al., Nature, 338, April 27 (1989).
[3] V. A. Klyuev, et al., Sov. Tech. Phys. Lett. 12, 551 (1986).

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* Work supported by U.S.D.o.E. Contract No. DE-AC02-76CHO3073.
1 A. DeNinno, et. al., submitted to Europhysics Letters.
2 S.E. Jones, et. al., Nature, 338, April 27 (1989).
3 V.A. Klyuev, et. al., Sov. Tech. Phys. Lett. 12, 551 (1986).

10

Gammas from Cold Fusion

D. Bailey,* University of Toronto**

The absence of both neutrons and gamma rays can be used to constrain possible cold fusion processes in deuterium-metal systems. In particular, milliwatt cold fusion processes in palladium producing fast protons, tritium, 3He or 4He nuclei would also usually produce easily observable numbers of Coulomb excitation palladium gamma rays. Typical expected yields are approx. 104 - 106 gammas per joule of fusion energy in lines at 0.374, 0.434, 0.512 and 0.556 MeV. Reported [1] 2.2 MeV np capture gamma rays are consistent with the ubiquitous radon daughter ^{214}Bi 2.204 MeV background line.

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** Supported in part by NSERC (Canada).

[1] M.Fleischmann, S. Pons, and M. Hawkins, J. Electroanal. Chem. 261 (1989) 301, and errata.

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for the Special Cold Nuclear Fusion Session
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American Physical Society
May 1, 1989

Gammas from Cold Fusion. D. Bailey* University of Toronto: **. – The absence of both neutrons and gamma rays can be used to constrain possible cold fusion processes in deuterium-metal systems. In particular, milliwatt cold fusion processes in palladium producing fast protons, tritium, ^3He or ^4He nuclei would also usually produce easily observable numbers of Coulomb excitation palladium gamma rays. Typical expected yields are $\sim 10^4 - 10^6$ gammas per joule of fusion energy in lines at 0.374, 0.434, 0.512 and 0.556 MeV. Reported[1] 2.2 MeV np capture gamma rays are consistent with the ubiquitous radon daughter ^{214}Bi 2.204 MeV background line.

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Special Cold Fusion Session ________________

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11

Sources of Neutrons. and Tritium from D-Li-6 Mixtures

Lawrence Cranberg, TDN, Inc.

The work of Fleischmann, Pons, and Hawkins (1) claims detection of room temperature fusion of deuterons based in part on detection of neutrons and of tritium in electrochemical experiments with vessels containing mixtures of compounds of deuterium and lithium-6. Alternative, well-known nuclear reactions induced by ambient gamma-rays and neutrons in the experimental materials are suggested, together with suitable control experiments to measure those effects. It is significant to note that a negative result on (1) or on the work of Jones et al. (2), with experimental cells replaced by a blank or hydrogen-filled cell is not a check on the proposed background sources.

[1] M. Fleischmann, B. Pons, M. Hawkins, J. Electroanalytical Chemistry, 261, 301 (1989).
[2] S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen, J. M. Thorne, S. F. Taylor, and J. Rafelski, Preprint of article submitted to Nature.

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Abstract,
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Cold Fusion, May 1, 1989

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April 23, 1989

12

Searches for Cold Fusion*

E. B. Norman, B. Sur, K. T. Lesko, K.R. Czerwinski, H. L. Hall, R. A. Henderson, and D. C. Hoffman

Lawrence Berkeley Laboratory

Following the reported observations of nuclear fusion reactions of deuterium nuclei loaded into metallic crystalline lattices, [1,2] we have searched for neutrons and gamma rays that should be produced by such processes. Two separate D_2O cells containing the electrodes and electrolytes described in Refs. I and 2 have been operated over a period of three weeks. Fast neutrons have been searched for using liquid scintillators and dosimetry film. Prompt gamma rays have been searched for using Nal detectors; induced radioactivity in the electrodes was searched for using Ge detectors. Background measurements have been conducted with the cells turned off. Measurements of the mass of a palladium electrode before and after electrolysis showed that the number of deuterium atoms loaded was 0.5 per Pd atom. No excess of neutrons or gamma rays above background has been observed. Upper limits on the possible rates of fusion reactions occurring in these cells will be presented.

* Supported by the U.S. Dept. of Energy under Contract No. DE- C03- 765F00098.

[1] M. Fleischmann and S. Pons, preprint
[2] S. E. Jones et. al., preprint

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Bulletin Subject Heading
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Cold Fusion

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1. M. Fleischmann and S. Pons, preprint
2. S. E. Jones et. al., preprint

Prefer Standard Session

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13

Search for Cold Fusion in Electrolytic Cells

D. R. McCracken, J. Paquette, R. E. Johnson, N. A. Briden, W. G. Cross, A. Arneja, D. C. Tennant, M.A. Lone, and W. J. L. Buyers,

Chalk River Nuclear Laboratories

A variety of electrolytic cells have been studied having palladium cathodes in the form of wires, tubes, rods or foil and having anodes of platinum wire or foil, or of nickel tube. Some of these cells have a cylindrical configuration similar to the cell in which cold fusion is claimed by Fleischmann and Pons to have occurred. The electrolyte was 0.1 molar LiOD in virgin D_2O. An AECL wet proofed catalyst above the cell was used to recombine the evolved D_2 and O_2. Current densities up to 140 mA/cm^2 have been applied. Arrays of 3 to 5 ^{3}He detectors were mounted beside each cell in a central 20 cm cavity of a large 130 cm x 120 cm x 90 cm wax neutron shield. This gives a very low, constant background of 30±2 counts/hour summed over all five detectors or 18±2 counts/hour for three detectors. After running the cells for times of three to four days no excess neutrons were observed above background. The cells were run mainly in continuous mode but a search for transient neutrons was also done after switching on the current. No measurable excess heat was observed in the water from the cooling jacket. In a cell without a recombiner the enrichment in tritium in the electrolyte was not inconsistent with the range of D/T separation factors that occur at palladium electrodes.

Search for Cold Fusion in Electrolytic Cells, D.R. McCRACKEN, J. PAQUETTE, R.E. JOHNSON, N.A. BRIDEN, W.G. CROSS, A. ARNEJA, D.C. TENNANT, M.A. LONE AND W.J.L. BUYERS, Chalk River Nuclear Laboratories, - A variety of electrolytic cells have been studied having palladium cathodes in the form of wires, tubes, rods or foil and having anodes of platinum wire or foil, or of nickel tube. Some of these cells have a cylindrical configuration similar to the cell in which cold fusion is claimed by Fleischmann and Pons to have occurred. The electrolyte was 0.1 molar LiOD in virgin D_2O. An AECL wet proofed catalyst above the cell was used to recombine the evolved D_2 and O_2. Current densities up to 140 mA/cm^2 have been applied. Arrays of 3 to 5 ^{3}He detectors were mounted beside each cell in a central 20 cm cavity of a large 130 cm × 120 cm × 90 cm wax neutron shield. This gives a very low, constant background of 30 ± 2 counts/hour summed over all five detectors or 18 ± 2 counts/hour for three detectors. After running the cells for times of three to four days no excess neutrons were observed above background. The cells were run mainly in continuous mode but a search for transient neutrons was also done after switching on the current. No measurable excess heat was observed in the water from the cooling jacket. In a cell without a recombiner the enrichment in tritium in the electrolyte was not inconsistent with the range of D/T separation factors that occur at palladium electrodes.

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Search for DD-Fusion Neutrons

D. Seeliger, K. Wiesener, A. Meister, D. Ohms, D. Rehner, R., Schwierz, P. Wustner, Technical University, Dresden

Using a large volume liquid scintillation detector and other neutron and gamma- ray detectors, we measured the radiation arriving from the electrolysis of heavy water with a palladium cathode. Using an efficient proton recoil neutron spectrometer (NE-213 scintillator coupled to an XP-2040 phototube) equipped with electronic depression of gamma rays and cosmic ray muon background, evidence was found for a weak fast neutron production. In the proton recoil energy range between 2 MeV and 3 MeV at an average background rate of about 85 counts per hour, the order of 20±5 counts per hour coming from the 60 x 47 x 3 mm^3 palladium sheet was observed. This results in a neutron producing reaction rate of approximately 0.1 s^{-1} in the whole volume of the electrode.

Abstract Submitted

for the Spring Meeting of the

American Physial Society

May 1, 1989

Neutron Detection

Cold Fusion Special Seminar

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15

Fusion Rates for Hydrogen Isotopic Molecules of Relevance for Cold Fusion*

K. Szalewicz, J.D. Morgan III: U. Delaware; H.J. Monkhorst: U. Florida

In response to the recent announcements of evidence for room-temperature fusion in the electrolysis of D_2O, we have analyzed how the fusion rate depends on several factors, including the reduced mass of the fusing nuclei and the degree of vibrational excitation. Calculations have been performed within the adiabatic approximation employing an accurate Born-Oppenheimer potential energy curve and including the adiabatic and relativistic corrections. We have also used the WKB approximation which displays the essence of these factors. Our results predict fusion rates for the ground vibrational states up to 14 orders of magnitude larger than previously estimated and exhibit a strong dependence of the Coulomb barrier penetration factor on the reduced mass of the pair of nucleons. We have found that fusion out of vibrationally excited states is enhanced by several orders of magnitude, which may be of particular significance in light of the experimental evidence for the importance of non-equilibrium conditions. To assist in the investigation of whether a 'heavy' electron arising from complicated collective solid-state effects could play a role in the enhanced fusion rates seen in the experiments, we study how the Coulomb barrier penetration factor depends on the mass of a hypothetical particle (or quasi-particle) of charge -1. We examine the issue of whether the excess heat observed in one of the experiments could arise from the aneutronic fusion reaction p + d - 3He + γ. We find that under the conditions implied by the measurements of the neutron flux from the reaction d + d - ^{3}He+n, it is unlikely that the excess heat observed by one of the groups could arise from p + d fusion.

* Supported by the NSF and by the Division of Advanced Energy Projects, DOE.

Abstract submitted
for the special session on cold fusion
at May 1989 General Meeting of APS in Baltimore
April 21, 1989

Fusion Rates for Hydrogen Isotopic Molecules of Relevance for Cold Fusion* K. SZALEWICZ, J.D. MORGAN III: U. Delaware; H.J. MONKHORST: U. Florida. — In response to the recent announcements of evidence for room-temperature fusion in the electrolysis of D_2O, we have analyzed how the fusion rate depends on several factors, including the reduced mass of the fusing nuclei and the degree of vibrational excitation. Calculations have been performed within the adiabatic approximation employing an accurate Born-Oppenheimer potential energy curve and including the adiabatic and relativistic corrections. We have also used the WKB approximation which displays the essence of these factors. Our results predict fusion rates for the ground vibrational states up to 14 orders of magnitude larger than previously estimated and exhibit a strong dependence of the Coulomb barrier penetration factor on the reduced mass of the pair of nucleons. We have found that fusion out of vibrationally excited states is enhanced by several orders of magnitude, which may be of particular significance in light of the experimental evidence for the importance of non-equilibrium conditions. To assist in the investigation of whether a 'heavy' electron arising from complicated collective solid-state effects could play a role in the enhanced fusion rates seen in the experiments, we study how the Coulomb barrier penetration factor depends on the mass of a hypothetical particle (or quasi-particle) of charge -1. We examine the issue of whether the excess heat observed in one of the experiments could arise from the aneutronic fusion reaction $p + d \rightarrow {}^3He + \gamma$. We find that under the conditions implied by the measurements of the neutron flux from the reaction $d + d \rightarrow {}^3He + n$, it is unlikely that the excess heat observed by one of the groups could arise from $p + d$ fusion.

*Supported by the NSF and by the Division of Advanced Energy Projects, DOE.

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16

Upper Limits to Fusion Rates of Isotopic Hydrogen Molecules at High Electron Density Interstitial Pd Sites*

L. Wilets, M. Alberg, J. J. Rehr and J. Mustre de Leon, Univ. of Washington.

We have studied upper bounds for p-d and d-d fusion rates in a degenerate electron gas as a function of screening electron density ($\propto r_s^{-3}$) and confinement potential in a Pd lattice. At tetrahedral (T) and octahedral (O) sites of saturated PdD we estimate r_s to be between 2.0 and 2.8 a_0, which gives an upper limit of 10^{-57}/s for p-d and 10^{-67}/s for d-d. A rate 10-^{21}/s would require an r_s of 0.27 a_0 for p-d. Confinement by the Pd atoms considerably enhances these rates. With a T-site hard cell radius of 0.65 a_0 we obtain upper bounds of 10^{-30}/s and 10^{-34}/s respectively: rates at O-sites are lower. However, a more realistic confinement potential at the T-sites is softer and gives only 10^{-49}/s: moreover, occupation of T-sites is chemically (and perhaps structurally) unfavorable, given a D_2 confinement energy of about 30 eV. We conclude that fusion in Pd is most favorable at the T-site, but even there at rates significantly less than quoted experimental values of 10^{-19} - 10^{-23}/s.

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Suggested Session
Fusion

Upper Limits to Fusion Rates of Isotopic Hydrogen Molecules at High Electron Density Interstitial Pd Sites.* L. WILETS, M. ALBERG, J.J. REHR and J. MUSTRE de LEON, **Univ. of Washington.**—We have studied upper bounds for p-d and d-d fusion rates in a degenerate electron gas as a function of screening electron density ($\propto r_s^{-3}$) and confinement potential in a Pd lattice. At tetrahedral (T) and octahedral (O) sites of saturated PdD we estimate r_s to be between 2.0 and 2.8 a_0, which gives an upper limit of $10^{-57}/s$ for p-d and $10^{-67}/s$ for d-d. A rate $10^{-20}/s$ would require an r_s of 0.27 a_0 for p-d. Confinement by the Pd atoms considerably enhances these rates. With a T-site hard cell radius of 0.65 a_0 we obtain upper bounds of $10^{-30}/s$ and $10^{-34}/s$ respectively; rates at O-sites are lower. However, a more realistic confinement potential at the T-sites is softer and gives only $10^{-49}/s$; moreover, occupation of T-sites is chemically (and perhaps structurally) unfavorable, given a D_2 confinement energy of about 30 eV. We conclude that fusion in Pd is most favorable at the T-site, but even there at rates significantly less than quoted experimental values of $10^{-19} - 10^{-23}/s$.
* Supported in part by the DOE and the NSF.

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17

"Solid-State" Effects Cannot Enhance the Cold Fusion Rate Enough

A. J. Leggett and G. Baym, Department of Physics, University of Illinois at Urbana-Champaign, 1110 W. Green St., Urbana, Illinois 61801

To achieve the rate of neutron production, approx. 10^{-23}/sec/deuteron pair, by cold fusion of deuterium in solid Pd or Ti, requires the solid-state environment to produce either an unusual enhancement of the fusion reaction rate, or a large suppression of the Coulomb barrier between deuterons-the latter presumably arising from some kind of sophisticated many-body screening effect. We point out that a very severe exact quantum-mechanical constraint is imposed on all such enhanced screening mechanisms in solids in equilibrium by observable behavior of a ^{4}He atom in the metal in question. Unless the latter is quite anomalous, or the deuteron pair correlation function is of order 10^{12} at atomic separations, no enhancement of the Coulomb barrier penetration anywhere near the magnitude required to explain the fusion rates inferred from the experiments is possible in a solid in at zero temperature; in thermal equilibrium at room temperature such an enhancement would require at a minimum very exotic long range influences on the tunneling process.

"SOLID-STATE" EFFECTS CANNOT ENHANCE THE COLD FUSION RATE ENOUGH

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18

Electrochemically Induced Excess Heat in a "Cold Fusion" cell with Zr2Pd Electrode

Joseph Cantrell, Dept of Chemistry and William E. Wells, Dept. of Physics, Miami University, Oxford, OH

A "Cold Fusion" cell patterned after that of Fleischmann and Pons' was constructed using Zr2Pd foils instead of Pd rods. The total volume of the electrode was 0.014 cm^3. At a room temperature of 239 K, the electrodes drew 90 mA with 4.8 V applied, and presented a 6 K change in temperature. When a 10 ohm resister, drawing 219 mA in the heavy water bath, was used to produce heating instead of the electrodes, the temperature rise over the 289 K background was 3 K. No neutron measurements have been made as yet. The temperature dependence of the process is positive. The process continued for more than 100 hours, before decaying. DOE Mound Labs-EG&G examined the cell electrode, electrolyte solution, and a copious precipitate in the bottom of the test tube, with SIM microprobe, XRD, Auger, and Atomic Absorption. These results will be presented.

[1] Fleischmann and Pons J. Electroanal. Chem., 261 301-308 (1989)

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to the Special Session on Cold Fusion

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Baltimore, Md.

May 1, 1989

Electrochemically Induced Excess Heat in a "Cold Fusion" cell with a Zr_2Pd Electrode Joseph Cantrell, Dept of Chemistry and William E. Wells, Dept. of Physics, Miami university, Oxford, Ohio—A "Cold Fusion" cell patterned after that of Fleischmann and Pons[1] was constructed using Zr_2Pd foils instead of Pd rods. The total volume of the electrode was 0.014 cm^3. At a room temperature of 289 K, the electrodes drew 90 mA with 4.8 V applied, and presented a 6 K change in temperature. When a 10 ohm resister, drawing 219 mA in the heavy water bath, was used to produce heating instead of the electrodes, the temperature rise over the 289 K background was 3 K. No neutron measurements have been made as yet. The temperature dependence of the process is positive. The process continued for more than 100 hours, before decaying. DOE Mound Labs-EG&G examined the cell electrode, electrolyte solution, and a copious precipitate in the bottom of the test tube, with SEM-microprobe, XRD, Auger, and Atomic Absorption. These results will be presented.

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19

Search for Fusion Products Using X-Ray Detection

M. R. Deakin, J. D. Fox, K. W. Kemper, E.G. Myers, W. N. Shelton, and J. G. Skofronick, Florida State University*

The fusion of deuterons should produce energetic protons in about half the reactions in an electrolysis cell with Pt anode and Pd cathode. Our cell is specially constructed with a thin window so that K x-rays of Pd, excited by charged fusion products (mostly protons) can be detected. The background of the x-ray detector, 3 counts per hour in the vicinity of the Pd K x-rays, corresponds to fewer than 50 fusions per second or fusion energy release rate of less than 10-10 watts in the Pd cathode. The cell has been operated for two weeks as of 4/29/89.

*Supported by the National Science Foundation and the State of Florida.

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Search for Fusion Products Using X-Ray Detection. M. R. DEAKIN, J. D. FOX, K. W. KEMPER, E. G. MYERS, W. N. SHELTON, and J. G. SKOFRONICK, Florida State University.* -- The fusion of deuterons should produce energetic protons in about half the reactions in an electrolysis cell with Pt anode and Pd cathode. Our cell is specially constructed with a thin window so that K x-rays of Pd, excited by charged fusion products (mostly protons) can be detected. The background of the x-ray detector, 3 counts per hour in the vicinity of the Pd K x-rays, corresponds to fewer than 50 fusions per second or a fusion energy release rate of less than 10^{-10} watts in the Pd cathode. The cell has been operated for two weeks as of 4/29/89.

*Supported by the National Science Foundation and the State of Florida.

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20

Search for Neutrons and Gamma-Rays from "Cold Fusion" in Deuterided Metals*

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A search for neutrons and gamma-rays emitted in "cold fusion" in electrolytically deuterided metals was carried out with a very low background and a sensitive neutron detection system. composed of an array of six liquid-scintillator neutron counters, with efficiency of approx. 1%. Pulse shape, pulse height and time of flight were measured for scattered neutrons. Gamma-rays were detected in two large (12.5 cm) Nal(TI) detectors, with efficiency of 0.1% at 5.5 MeV. The detection system was shielded from background radiation and two large area cosmic-ray veto counters were utilized. Up to four electrochemical cells, similar to the ones used by Fleischmann and Pons and Jones et al., ran concurrently, with Pd or cold worked Ti rods as cathodes. The Pd electrodes were cold worked or annealed in vacuum or argon, one electrode was predeuterided and various surface treatments were carried out. The metals were electrochemically charged with deuterium in heavy water (97.5% or 99.8% D20) electrolytes containing LiOD or a variety of salts. Ti alloy powder deuterided at high temperature and pressure was also used for comparison. During electrochemical charging, no statistically significant deviation from the background was observed in either gamma-ray or neutron detectors, after some of the cells were on for almost three weeks. Using our neutron detector system we estimate (e.g., for a 7 hour run at the end of two weeks of cell electrolysis) the rate of "cold fusion" of d + d in our Pd and Ti samples to be smaller than the order of 10-25fusions/atom pair/sec (3σ limit), and the gamma ray data yield a rate of "cold fusion" of p + d smaller than the order of 10^{-22} fusions/atom pair/sec (3σ limit). The p + d reaction was recently estimated to be eight orders of magnitude larger than the d + d rate. The estimated neutron flux in our experiment is at least a factor of 100 smaller than that reported by Jones et al. and some million times smaller than that reported by Fleischmann and Pons. Cosmic rays have been observed to produce neutrons with energies expected for fusion events. An attempt to initiate "cold fusion" with 5 MeV alpha particles produced no measurable effect.

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SEARCH FOR NEUTRONS AND GAMMA-RAYS FROM "COLD FUSION" IN DEUTERIDED METALS*

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ABSTRACT

A search for neutrons and gamma-rays emitted in "cold fusion" in electrolytically deuterided metals was carried out with a very low background and a sensitive neutron detection system, composed of an array of six liquid-scintillator neutron counters, with efficiency of ~1%. Pulse shape, pulse height and time of flight were measured for scattered neutrons. Gamma-rays were detected in two large (12.5 cm) NaI(Tl) detectors, with efficiency of 0.1% at 5.5 MeV. The detection system was shielded from background radiation and two large area cosmic-ray veto counters were utilized. Up to four electrochemical cells, similar to the ones used by Fleischmann and Pons and Jones et al., ran concurrently, with Pd or cold worked Ti rods as cathodes. The Pd electrodes were cold worked or annealed in vacuum or argon, one electrode was predeuterided and various surface treatments were carried out. The metals were electrochemically charged with deuterium in heavy water (97.5% or 99.8% D_2O) electrolytes containing LiOD or a variety of salts. Ti alloy powder deuterided at high temperature and pressure was also used for comparison. During electrochemical charging, no statistically significant deviation from the background was observed in either gamma-ray or neutron detectors, after some of the cells were on for almost three weeks. Using our neutron detector system we estimate (e.g. for a 7 hour run at the end of two weeks of cell electrolysis) the rate of "cold fusion" of d + d in our Pd and Ti samples to be smaller than the order of 10^{-25} fusions/atom pair/sec (3σ limit), and the gamma ray data yield a rate of "cold fusion" of p + d smaller than the order of 10^{-22} fusions/atom pair/sec (3σ limit). The p + d reaction was recently estimated to be eight orders of magnitude larger than the d + d rate. The estimated neutron flux in our experiment is at least a factor of 100 smaller than that reported by Jones et al. and some million times smaller than that reported by Fleischmann and Pons. Cosmic rays have been observed to produce neutrons with energies expected for fusion events. An attempt to initiate "cold fusion" with 5 MeV alpha particles produced no measurable effect.

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21

A Survey of Cold Fusion

Douglas R. O. Morrison*, CERN, Geneva, Switzerland

The history of fusion of hydrogen to helium from 1926 until today is reviewed. World results are tabulated and summarized, Problems with the 1989 original papers from Utah and BYU are described. Consequences from the structure of palladium hydrides are drawn. Possible explanations are considered. Conclusions on cold fusion are made and placed in historical perspective.

*Member APS

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*** Member APS.**

22

Dynamic Response of Thermal Neutron Measurements in Electrochemically Produced Cold Fusion Subject to Pulsed Current

J. R. Granada, J. Converti, R. E. Mayer, G. Guido, P. C. Florido, N. F. Patino, L. Sobehart, S. Gomez, and A. Larreteguy, Centro Atomico Bariloche and Institute Balseiro, Comision Nacional de Energia Atomica and Universidad Nacional de Cuyo, 8400 S.C. de Bariioehe, Rio Negro, Argentina

Submitted to *Physical Review Letters*, April 28, 1989

ABSTRACT

The present work shows the results of measurements performed on electrolytic cells using a high efficiency (22%) neutron detection system in combination with a procedure involving a non-stationary current through the cell's circuit.

Cold fusion was produced in electrolytic cells containing LiH dissolved in heavy water with a Palladium cathode. The dynamic response to low frequency current pulses was measured. Characteristic patterns showing one or two bumps were obtained in a repeatable fashion. These patterns are strongly dependent on the previous charging history of the cathode.

The technique employed seems to be very convenient as a research tool for a systematic study of the different variables governing the phenomenon.

DYNAMIC RESPONSE OF THERMAL NEUTRON MEASUREMENTS IN ELECTROCHEMICALLY PRODUCED COLD FUSION SUBJECT TO PULSED CURRENT

J.R. Granada, J. Converti, R.E. Mayer, G. Guido,
P.C. Florido, N.E. Patiño, L. Sobehart, S. Gomez and
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ARGENTINA

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23

Examination of Nuclear Measurement Conditions in Cold Fusion Experiments

D. Abriola, E. Achterberg, M. Davidson,** M. Debray, M. C, Etchegoyen.- N. Fazzini. J. Fernandez Niello,T A. M. J. Ferrero, A.

Filevich, M. C. Galia, R. Garavaglia, G. Garcia Bermudez,T R. T. Gettar.* S. Gil. H. Grahmann. It Huck, A. Jech. A. J. Kreiner,T A. 0. Macchiavelli, J. F. Magallanes,* E. Maqueda, G. Marti. A J. Pacheco, M. L. Perez, C. Pomar, M. Ramirez, and M. Scasserra. Departamento de Flsica, Comision Nacional de Energia Atornica, 1429 Buenos Aires. Argentina

The possible production of nuclear fusion through electrochemical processes was studied by the simultaneous detection of γ-rays and neutrons. The importance of high energy resolution for γ-ray measurements is discussed. Both types of measurements yield consistent results for the upper limits of the neutron production rates in this experiment.

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EXAMINATION OF NUCLEAR MEASUREMENT CONDITIONS IN COLD FUSION EXPERIMENTS

D.Abriola, E.Achterberg, M.Davidson[**], M.Debray,
M.C.Etchegoyen[+], N.Fazzini, J.Fernández Niello[+],
A.M.J.Ferrero, A.Filevich, M.C.Galia, R.Garavaglia,
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H.Huck, A.Jech, A.J.Kreiner[+], A.O.Macchiavelli,
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Departamento de Física, Comisión Nacional de Energía Atómica
1429 Buenos Aires - Argentina

ABSTRACT

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* Departamento Química Analítica, Comisión Nacional de Energía Atómica.

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24

γ-Ray Spectra in the Fleischmann, Pons, Hawkins Experiment*

R.D. Petrasso, X. Chen, K. Wenzel, R. R. Parker, C. K. Li, and C. Fiore, Plasma Fusion Center, Massachusetts Institute of Technology, Cambridge, MA

Fleischmann, Pons, and Hawkins (FPH) [1] recently announced that significant fusion heating was occurring in their cold fusion experiments. As compelling evidence of fusion processes, they reported the detection of 2.2 MeV y rays that result from neutron-capture-on-hydrogen. We have carefully analyzed their published y-ray spectra. We have also performed detailed terrestrial γ background measurements and neutron-capture-on-hydrogen experiments. From our analyses we conclude that the FPH γ line is specious on the basis of three quantitative considerations: (1) It has a line width a factor of 2 smaller than the detector instrumental resolution at 2.2 MeV; (2) There is no evidence of a Compton edge at 1.99 MeV (i.e., 2.22 MeV - 0.23 MeV), and this edge should be distinctly prominent; and (3) FPH's estimate of the neutron source rate is a factor of 40 too large. Additionally, from terrestrial γ background considerations, we conjecture that FPH's purported γ line actually resides at 2.5 MeV rather than 2.2 MeV. Based solely on the three quantitative arguments, we conclude that the γ signal reported by FPH cannot be the 2.2 MeV neutron-capture-on-hydrogen γ ray.

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*MIT Report PFC/JA-89-24.
[1] J. Electroanal. Chem. 261 (1989) 301-308; and errata.

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American Physical Society

γ-Ray Spectra in the Fleischmann, Pons, Hawkins Experiment,* R.D. PETRASSO, X. CHEN, K. WENZEL, R. R. PARKER, C. K. LI, and C. FIORE, Plasma Fusion Center, Massachusetts Institute of Technology, Cambridge, MA. -- Fleischmann, Pons, and Hawkins (FPH)[1] recently announced that significant fusion heating was occurring in their cold fusion experiments. As compelling evidence of fusion processes, they reported the detection of 2.2 MeV γ rays that result from neutron-capture-on-hydrogen. We have carefully analyzed their published γ-ray spectra. We have also performed detailed terrestrial γ background measurements and neutron-capture-on-hydrogen experiments. From our analyses we conclude that the FPH γ line is specious on the basis of three quantitative considerations: (1) It has a line width a factor of 2 smaller than the detector instrumental resolution at 2.2 MeV; (2) There is no evidence of a Compton edge at 1.99 MeV (i.e., 2.22 MeV - 0.23 MeV), and this edge should be distinctly prominent; and (3) FPH's estimate of the neutron source rate is a factor of 40 too large. Additionally, from terrestrial γ background considerations, we conjecture that FPH's purported γ line actually resides at 2.5 MeV rather than 2.2 MeV. Based solely on the three quantitative arguments, we conclude that the γ signal reported by FPH cannot be the 2.2 MeV neutron-capture-on-hydrogen γ ray. Supported in part by U. S. Department of Energy Contract No. DE-AC02-78ET51013. *MIT Report PFC/JA-89-24.

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25

Measurements of Neutron and Gamma Ray Emission Rates and Calorimetry in Electrochemical Cells Having Palladium Cathodes

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Results of experiments intended to reproduce the excess heat and neutron emission from electrochemical cells reported in Ref. 1 are presented. Radiation emission and power balance measurements were carried out on a set of electrochemical cells consisting of Pd cathodes, Pt anodes, D_2O or H_2O solvent with LiOD or LiOH electrolyte. The current density at the Pd cathode was 32 mA/cm^2 to 250 mA/cm^2 at applied voltages of 3.0 V to 15.0 V. Moderated BF_3 neutron detectors were absolutely calibrated; for a source strength of 160 neutrons/sec count rates would be twice the background level. X-ray pulse height spectroscopy with NaI(T1) detectors monitored the neutron capture process p(n,γ)d. Power balance during electrolysis was monitored by means of a constant temperature calorimeter in both D_2O and H_2O electrolytic cells with accuracy of +15mW.

[1] M.Fleischmann, S.Pons, and M. Hawkins, Journal of Electroanalytical Chemistry, 261,301 (1989).

Measurements of Neutron and Gamma Ray Emission Rates and Calorimetry in Electrochemical Cells Having Palladium Cathodes. S.C. LUCKHARDT, X. CHEN, C. FIORE, M. GAUDREAU, D. GWINN, P. LINSAY, R. PARKER, R. PETRASSO, K. VENZEL, Plasma Fusion Center, R. CROOKS, V. CAMMARATA, M. SCHLOH, D. ALBAGLI, M. WRIGHTON, Department of Chemistry, R. BALLINGER, I. HWANG, Department of Material Science and Engineering. MIT--Results of experiments intended to reproduce the excess heat and neutron emission from electrochemical cells reported in Ref. 1 are presented. Radiation emission and power balance measurements were carried out on a set of electrochemical cells consisting of Pd cathodes, Pt anodes, D_2O or H_2O solvent with LiOD or LiOH electrolyte. The current density at the Pd cathode was 32 mA/cm^2 to 250 mA/cm^2 at applied voltages of 3.0V to 15.0V. Moderated BF_3 neutron detectors were absolutely calibrated; for a source strength of 160neutrons/sec count rates would be twice the background level. X-ray pulse height spectroscopy with NaI(Tl) detectors monitored the neutron capture process p(n,γ)d. Power balance during electrolysis was monitored by means of a constant temperature calorimeter in both D_2O and H_2O electrolytic cells with accuracy of $\pm$15mW.

1. M.Fleischmann, S.Pons, and M.Hawkins, Journal of Electroanalytical Chemistry, 261, 301 (1989).

26

Tests of "Cold Fusion" in a New Configuration

F. Skiff, H. M Milchberg, and J. Rogers, Laboratory for Plasma Research, University of Maryland. College Park, MD 20742

Loading palladium metal with hydrogen isotopes is accomplished in a plasma environment as opposed to an electrolyte in order to permit sensitive tests of potential nuclear events. A palladium electrode is immersed into a plasma of deuterium and ion absorption is enhanced by drawing ion current. The plasma environment permits rapid loading of the metal, sensitive tests of gas composition, as well as searching for neutrons without moderation by water. Preliminary results will be discussed.

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27

Cold Nuclear Fusion in Dense Metallic Hydrogen: Implications for Astrophysics

C.J. Horowitz, Nuclear Theory Center, Indiana U.*

The rate of nuclear fusion from tunnelling of zero point motion in very dense metallic hydrogen is calculated assuming a simple crystal of nuclei interacting via screened coulomb potentials. At a density of five g/cm^3 the fusion rate is 10^{-50} per H-D pair per second. Thus fusion may not contribute to the heating of Jupiter unless a more efficient mechanism is found. However increasing the density to 300 to 2600 g/cm^3 increases the rate to 10^{-21} to 10^{-12} sec^{-1}. It is speculated that a cold condensed object with a small amount of deuterium could be reheated via p + D cold fusion and start conventional thermonuclear fusion.

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Special Session on Cold Fusion

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*Supported by the DOE.

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28

Theory of Cold Fusion

M . Danos, NIST

The lowest order Feynman graph leading to dd fusion in the vicinity of a lattice nucleus, X, is given by the tree graph Fig. 1. We assume that the deuteron d_1, is trapped (trapping wave function . . .) and the deuteron d_2 flies by with relative velocity v^2 = ... All momenta $t_1 < 50$ meV are thermal. Hence the initial state is given by a density matrix. In the final state E_f . . .. The electromagnetic vertices F(q) and f(q), even though off-the- mass-shell, are given in order of magnitude by the form factors known from electron scattering, and . . . is the momentum space wave function of the d-d component of the ^4He ground state, which can be estimated from nuclear structure data. The order of magnitude of the resulting rates corresponds to the observed rate of 10^{-10} sec^{-1}. (The reaction mechanism is easiest understood by considering the time-reversed reaction.) The suppression of the emission of protons or neutrons arises from the replacement of f(q) by the break-up from factor $f(q,q_1)$. Fig. 2. and by the replacement of the 2-body by the 3-body density of states. Similarly, the photon emission is suppressed by the replacement of the fusion vertex . . . Fig. 3. The details will be presented.

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Abstract Submitted

for the Spring Meeting of the

American Physical Society

May 1, 1989

Cold Fusion

Cold Fusion Special Session

<u>Theory of Cold Fusion</u>, M. Danos, <u>NIST</u>. --The lowest order Feynman graph leading to dd fusion in the vicinity of a lattice nucleus, M, is given by the tree graph Fig. 1. We assume that the deuteron d_1 is trapped (trapping wave function $\psi_0(t)$) and the deuteron d_2 flies by with relative velocity $v^2 = (2T/m)$ ($\hbar = c = 1$). All momenta $t_i \lesssim 50$ meV are

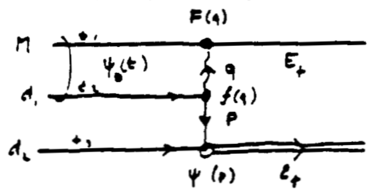

Fig. 1

thermal. Hence the initial state is given by a density matrix. In the final state $E_f + \epsilon_f = Q = 24$ MeV. The electromagnetic vertices F(q) and f(q), even though off-the-mass-shell, are given in order of magnitude by the form factors known from electron scattering, and ψ(p) is the momentum space wave function of the d-d component of the ^{4}He ground state, which can be estimated from nuclear structure data. The order of magnitude of the resulting rates corresponds to the observed rate of 10^{-10} sec^{-1}. (The reaction mechanism is easiest understood by considering the time-reversed reaction.) The suppression of the emission of protons or

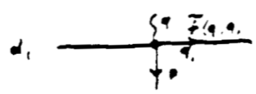

Fig. 2

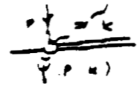

Fig. 3

neutrons arises from the replacement of f(q) by the break-up from factor $\tilde{f}(q, q_1)$, Fig. 2, and by the replacement of the 2-body by the 3-body density of states. Similarly, the photon emission is suppressed by the replacement of the fusion vertex ψ(p) by $\tilde{\psi}$(p,k), Fig. 3. The details will be presented.

Submitted by [signature]

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Limits on Cold Fusion in Matter: a Parametric Study*

J. Rafelski, M. Gajda, D. Harley and S.E. Jones**, University of Arizona

The rate of nuclear fusion of d-d hydrogen isotopes is studied as a function of several parameters, and is found to be critically sensitive in a regime of the parameter space that could be of physical relevance and also account for the fusion rate recently measured by Jones et al. The fusion rate in the (dde)+ ion- like structure is computed as a function of the maximum allowed hydrogen separation and as a function of an effective electronic mass and charge, leading to a fusion rate of the needed magnitude. These numerical exercises highlight the extraordinary sensitivity of the fusion rate to the physical parameters and the environment characterizing the system in which the (dde)+ complex is embedded. It is further shown that the effect each of these parameters has on the fusion rate is cumulative and that a neutron rate of 10-23 s- per atom is obtained with a plausible combination of these parameters. The fusion rate resulting from a low energy, less than 100 eV d-d scattering description is also computed and is shown to be too small.

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** Brigham Young University

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Date

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30

Electron Catalyzed Fusion in Metals'

D. A. BROWNE, R. G. GOODRICH, P, N. KIRK and E. F. ZGANJAR, L.S.U.

We present a simple model for the induction of nuclear fusion in metals through the formation of neutral and charged deuterium complexes similar to the mechanism of muon catalyzed fusion. The role of various materials properties of Pd and other metals in enhancing the fusion rate will also be discussed. We are currently taking measurements on a sample of Pd and a heavy fermion material and will present the results of our experiment in light of the model.

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*Supported by LSU Center for Energy Studies

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Prefer Standard Session

S. E. Jones
Suggested Chairperson

31

The Cold Fusion Rate of d-d in PdDx Hydride and the Branching Ratio of the He-4 to (p,n) Production Reactions

Hiroshi Takahashi, Brookhaven National Laboratory

Many electrons from the d ands conduction bands of PdDx hydride pile up near deuterons. This accumulation results to large screening of potential between deuterons and enhances the cold fusion rate. The number of the piled up electron is approximately proportional to the inverse of the density of the conduction electron level at the Fermi level; the linear response theory underestimates the number of electrons by about a factor of 4 less than the nonlinear response theory. The branching ratio of the production process of He-4 to (p and n) is extremely small in the collision experiment, and the transition from the s wave channel in cold fusion to the ground He-4 O+ state by emitting gamma-ray is prohibitive. The He-4 production process of emitting the surrounding electrons becomes appreciable, and to get an extremely large branching ratio requires the coherent direct excitation of optical phonons of PdDx hydride or coherent excitation through the surrounding conduction electrons by a strong electron lattice coupling. This work is supported by DOE Advanced Energy Project Division.

The Cold Fusion Rate of d-d in PdDx Hydride and the Branching Ratio of He-4 to (p,n) Production Reactions. Hiroshi TAKAHASHI Brookhaven National Laboratory

Many electrons from the d and s conduction bands of PdDx hydride pile up near deutrons. This accumulation reults to large screening of potential between deutrons and enhances the cold fusion rate. The number of the piled up electron is approximately proportional to the inverse of the density of the conduction electron level at the Fermi level; the linear response theory underestimates the number of electrons by about a factor of 4 less than the non-linear response theory.

The branching ratio of the production process of He-4 to (p and n) is extremely small in the collision experiment, and the transition from the s wave channel in cold fusion to the ground He-4 0+ state by emitting gamma -ray is prohibitive. The He-4 production process of emitting the surrounding electrons becomes appreciable, and to get an extremely large branching ratio requires the coherent direct excitation of optical phonons of PdDx hydride or coherent excitation through the surrounding conduction electrons by a the strong electron lattice coupling.-- This work is supported by DOE Advanced Energy Project Division.

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Criterion for Cold Fusion in the Condensed State

E. A. Stern,* Physics Dept. FM-15, University of Washington, Seattle, WA 98195

To increase the rate of tunneling through the coulomb barrier between two nuclei of isotopic hydrogen in the condensed state, the surrounding electrons must provide a more efficient shielding than occurs in the molecule. Koonin and Nauenberg(1) expressed this increased shielding requirement in terms of at least a five-fold increase in the electron mass to be consistent with claims of experiments. From Thomas-Fermi screening theory this requirement translates to at least a $5^3 = 125$-fold increase in the electron density from its value in the molecule. This required density is several orders of magnitude greater than occurs in metallic hydrides in either the interstitial sites or any defect sites where hydrogen can reside. Cold fusion cannot occur in the condensed state under conditions employed in the reported experiments.

*Research supported by DOE grant DE-FG06-84ER45163.

[1] S.E. Koonin and M. Nauenberg, Santa Barbara Institute for Theoretical Physics preprint NSF-ITP-89-48. April 1989.

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APR 27 1989
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Abstract Submitted
for the May 1st 1989 Meeting of the
American Physical Society

26 April 1989

Special Session on Cold Fusion
Sorting Category

Criterion for Cold Fusion in the Condensed State

E.A. STERN* *Physics Dept. FM-15, University of Washington, Seattle, WA 98195* -- To increase the rate of tunneling through the coulomb barrier between two nuclei of isotopic hydrogen in the condensed state, the surrounding electrons must provide a more efficient shielding than occurs in the molecule. Koonin and Nauenberg(1) expressed this increased shielding requirement in terms of at least a five-fold increase in the electron mass to be consistent with claims of experiments. From Thomas-Fermi screening theory this requirement translates to at least a $5^3 = 125$-fold increase in the electron density from its value in the molecule. This required density is several orders of magnitude greater than occurs in metallic hydrides in either the interstitial sites or any defect sites where hydrogen can reside. Cold fusion cannot occur in the condensed state under conditions employed in the reported experiments.

*Research supported by DOE grant DE-FG06-84ER45163.

(1) S.E. Koonin and M. Nauenberg, Santa Barbara Institute for Theoretical Physics preprint NSF-ITP-89-48. April 1989.

Signature of APS Member

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Theoretical Estimates of the Enhancement of Cold Fusion of Deuterium in Deuterated Palladium Systems

M. W. C. Dharmawardana and G. C. Aers. Division of Physics, National Research Council of Canada, Ottawa, Canada KIA OR6. [Bitnet: Chandre at NRCVMOI, FAX: (613)957-8734.]

We have estimated the enhancement of the nuclear fusion rate of Pd-D type systems and the D_2^+-muonium molecule in comparison with the fusion rate in a D_2-molecule at room temperature. The theoretical model uses standard ideas on screening and nuclear reaction rate. If very conservative estimates are made the enhancements for a pair of D^+-nuclei in Pd, PdD and in the D_2^+-μ molecule are found to be 10^{14}, 10^{21}, and 10^{64}. We also discuss the dependence of the enhancement on temperature, localization of D^+ in Pd etc. These results are quite encouraging for the possibilities of cold fusion of deuterium in palladium.

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SUBMISSION TO APS COLD FUSION SESSION 1 MAY 1989

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Chemical Forces Associated with Confinement of Deuterium in Palladium

B. I. Dunlap, J. W. Mintmire, D. W. Brenner, R. C. Mowrey, H. D. Ladouceur, P. P. Schmidt, C. T. White, and W. E. O'Grady, Naval Research Laboratory

First-principles and embedded-atom methods were used to study the effective interaction between two deuterons in a palladium lattice. At scales ranging from 0.1 to 1.0 A no effects are found to suggest that the effective interaction between two deuterons in palladium is significantly reduced from what is expected for gas phase D_2. Our results show clearly that molecular D_2 in palladium should dissociate to distances of the order of 1.0 Å or greater even in PdH_2 lattices.

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Molecule-Nucleus Resonance Enhancement of Cold Nuclear Fusion

A. V. Barnes and Heath Pois, Center for Atomic and Molecular Physics at Surfaces and Department of Physics and Astronomy, Vanderbilt University, Nashville, TN 37235

Resonance between molecular and nuclear states is considered as a possible means of enhancing fusion rates. Calculations of fusion reaction rates based on a two state description of the resonating system are presented. In particular we show the deuterium-deuterium gas phase fusion rates with resonance are orders of magnitude larger than rates without resonance.

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The Bond Length of the Deuterium Molecule in a Metallic Lattice

A. B. Hassam, * Department of Physics and Astronomy, University of Maryland, College Park, and A. N. Dharamsi,* Department of Electrical and Computer Engineering, Old Dominion University, Norfolk

The bond length of the D_2 molecule in vacuum is .7 A. The lattice constant of palladium is 4 A. If the D_2 molecule forms inside a primitive lattice cell, what is the bond length? We suggest that the bond length of the D_2 molecule is reduced by lattice effects as follows: Because of the nature of the metallic bond, a preponderance of electronic charge is expected at the center of the lattice cell from the Fermi sea. The D^+ nuclei in a D_2 molecule forming at the center of the cell, therefore, are subject to an extra attractive force from this preponderance, leading to a reduction in bond length.

We present a numerical solution of the ground state wavefunction of the D_2 molecular ion in the presence of an externally imposed negative charge concentration. For a total charge on the order of one electronic charge and scale size of the concentration of order 1 A, we show that up to a 50% reduction in the bond length of D_2 is effected. Results of the numerical solution for various charge distributions are presented. Similar results for the D_2 molecule, obtained by a model calculation, are also discussed.

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Abstract

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Fluctuations and Cold Fusion*

Ming Li, University of Maryland, College Park

We examine more closely the recent suggestion of Koonin that fusion rate can be enhanced by fluctuations. We look at several possible mechanisms for the fluctuations. The relevance to the heat generation in the core of Jupiter is also discussed. To gain some insight into these fluctuations, we propose to exactly soluble models: the one-dimensional model of an open quantum system for a harmonic oscillator and the two-dimensional lattice gas model. The fusion rate reported by Jones et al. requires fluctuations of such magnitude which are unlikely to be present in the palladium lattice.

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Simple yet Accurate Model Potential for Calculating Cold Fusion Rates

J. D. Morgan III, Harvard University,* and R. J. Monkhorst, U. of Florida**

Following the fundamental analysis of Jackson [1] and more recent work by van Siclen and Jones, [2] we have developed a very simple model potential which allows us to calculate with remarkable accuracy the Coulomb barrier penetration factor which appears in thee fusion rate. Our approach is very useful in showing how the Coulomb barrier penetration factor depends on various physical parameters, and in allowing one to make a simple yet accurate estimate of fusion rates. We will show how one can use our result to relate the measured d-d fusion rate to the rates of other fusion reactions involving hydrogen isotopes.

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**Supported by the Division of Advanced Energy Projects of the Dept. of Energy

1 J. D. Jackson, Phys. Rev. 106,330 (1957).

2 C. DeW van Siclen and S.E. Jones, J. Phys. G 12, 213 (1986).

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Exotic QED and Cold Nuclear Fusion*

Ming Li, University of Maryland, College Park

If one could see any signal of cold fusion at all using the best state of the art neutron detector, the corresponding fusion rate would still be many orders of magnitude larger than what would be expected on the basis of conventional wisdom. Should unmistakable evidence for cold nuclear fusion be detected in the future, we suggest that non-linear and non-perturbative aspects of QED may provide an explanation for the discrepancy in the fusion rate. Specifically, we explore one such possibility that is motivated by the GSI experiments of anomalous $e^+ e^-$ peaks.

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Search for Radiations from Cold Fusion in Pd-D System

R. S. Raghavan, L. C. Feldman, M. M. Broer, A. James and D. Murphy, AT&T Bell Laboratories, Murray Hill, NJ

We report on a search for neutrons from dd fusion in Pd rods loaded electrolytically with deuterium. Three Pd rods were used: 1) 0.125dia. x9cm long, drawn and cold worked; 2) 0.125dia. x3 cm long, drawn and annealed; 3) 0.41 dia. X 8cm long, cast and annealed. The rods were held in two different electrolytic cells (D_2O (99.5% D)±0.1 M LiOD), current density 64 mA/cm^2 placed before a 12.5dia. x 12.5cm Nal(T1) detector with 5cm of polyethylene (PE) moderator interposed. A pair of plastic scintillator plates above and below the Nal(T1) vetoed cosmic muons. The entire set-up was housed inside 10cm thick PE surrounded on the outside with Pb and borax. Fusion neutrons are moderated, creating inside the PE housing a slow neutron gas that can be detected by two signal modes of γ-ray producing reactions (1) n-capture by protons in the PE (2.224 MeV γ); (2) ^{23}Na and ^{127}I n-capture γ-rays in the range 3-7 MeV. The latter is a more sensitive signal since it is produced inside the NaI(T1) and the background is mostly due to cosmic rays, much less than that below 2.62 MeV (due to natural radioactivity). From the overall n detection efficiency (measured with an Am-Be source at the cell position) and the cosmic ray background limit, we deduce that a neutron production rate of approx. 1 n/sec in the cells can be measured with high confidence. After measuring for approximately three weeks we observe < 0.08 n/sec/g Pd, (0.4 cm dia. rod) compared to ~ 2.7x10^{3}n/sec/g Pd, claimed in recent work* for a closely similar Pd rod.

*M. Fleischmann and B. S. Pons, J. Electroanal. Chem, 261 (1989)301.

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