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Panel Discussion

Cold Fusion: What Do We Know? What Do We Think?

Remarks of Lawrence B. Rees[1]

For several years, Steven Jones of Brigham Young University has actively led a group of researchers in the study of muon-catalyzed fusion. Recently, we have also begun an investigation of the sort of cold fusion which has come to be known as solid-state or table-top fusion. This statement is an overview of the current status of research at BYU in both of these areas.

The process of muon-catalyzed fusion is illustrated in Fig. 1. A muon, created by the decay of a pion produced by a particle accelerator, is introduced into a target chamber containing deuterium and tritium. The muon is first captured by either a deuteron or a triton. If it is captured by a deuteron, it is quickly transferred to the more massive triton where it is more tightly bound. The triton-muon system is then readily attracted to a hydrogen molecule. Since a muon is very similar to an electron except in its mass, which is about 200 times heavier, the muon will form a complex with a deuteron and a triton which is about 200 times smaller than a normal d-t molecule. This d-t-muon complex then serves the same role as a positive hydrogen ion in a normal hydrogen molecule. It happens that the d-t-muon-d and d-t-muon-t mesomolecules form very readily because of a resonance condition wherein the energy released in the mesomolecular formation is readily absorbed by the production of excited molecular states. And once a mesomolecule forms, fusion occurs rapidly because the distance between the deuteron and triton bound by the muon is sufficiently small to make penetration of the potential barrier easy. The deuteron and triton then fuse to form an alpha particle and a neutron, the muon usually being left free to catalyze another fusion reaction.

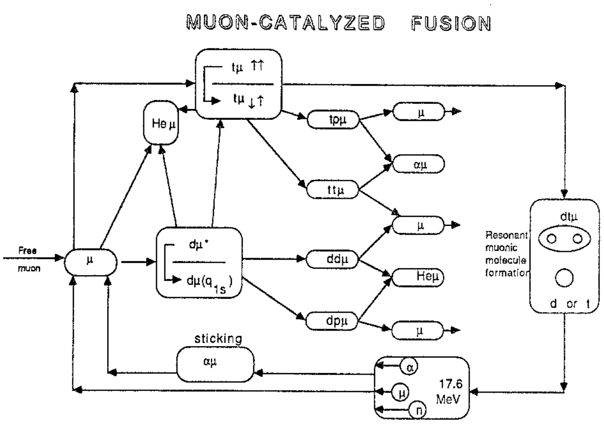

Fig. 1. Chief channels for μ-in a D-T target.

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Unfortunately, the lifetime of the muon is only about 2.2 microseconds. If a muon lasted forever, all our energy problems would be solved. But since it doesn't, we have to try to get as many fusions as we can from each muon before it decays.

It turns out that the muon lives long enough to produce more than enough fusions for energy output, but there is another problem which plagues us at this point. Some fraction of the time, typically less than 1%, the negatively-charged muon ends up sticking to the positively-charged alpha. If that happens, the muon usually remains bound until it decays, and is hence removed from the catalysis cycle.

Over the years, a number of experiments have measured the fusion rate as a function of such variables as temperature, density, and the ratio of deuterium to tritium. If the density of the d-t mixture is 1.2 times liquid hydrogen density, about 150 fusions per muon have been observed.[1] This is certainly a sizable number. However, it costs a lot to make muons since a particle accelerator is required. So even though you can get a lot of energy out of a single muon, it takes too much energy to produce the muons to be efficient for energy production.

In order to have energy breakeven, we probably need on the order of a few thousand fusions per muon. So we are really not near breakeven at this time.

There are, however, several research efforts which are proceeding. Target cells are currently being designed to increase the range of temperature and pressure where data have been taken. A target cell recently has been built which will allow pressures up to about 10,000 atmospheres. We hope to be able to actually do some experimentation with this target next spring or summer.

Hopefully, this will give us new insight into the density dependence of the fusion process. That, in turn, may lead theorists to a better understanding of the details of the mechanism involved. The highest density at which we've currently taken data is 1.2 times liquid hydrogen density. This new cell will allow us to measure fusion rates at about 2.3 times liquid hydrogen density. We probably don't have a good enough theory to accurately predict what will happen, but doing simple linear extrapolation, we might expect as many as about 300 fusions per muon at those densities. It's not clear that it will produce that many, but it's probably not likely that we'll have a substantially larger number either. So it is still improbable that we'll be in the range which is useful for energy production.[1]

The real problem to increased energy output is the matter of the sticking of muons to alpha particles. At this time, we don't have a complete understanding of the sticking process. Theoretical estimates differ somewhat from the data, but both experiments and theory suggest that there is about 0.2–0.7 percent sticking.[(1)]

Occasionally, nature helps us out by breaking a muon off from the alpha particle which has trapped it. This is called regeneration. Regeneration occurs naturally to some extent. We don't understand this process as well as we'd like either, there still being a considerable difference between experimental and theoretical values.

In the end, what we need to do is to reduce sticking and to increase regeneration. However, there are no obvious ways to do either at this time.

One last thing that might make the whole process feasible is a cheaper method of producing muons than the methods we currently have. This causes us to be somewhat pessimistic right now about the future status of muon-catalyzed fusion. Though, of course, research is still being done, we seem to be more or less at a standstill. And were likely to remain at a standstill unless future experiments produce something that's quite surprising.

Because of this situation, Steve Jones started looking more seriously into new areas of fusion research which he and others had considered previously.

Paul Palmer of our physics department, who happens to do a little teaching of geology on the side, wasn't particularly satisfied with the geologists' explanation of the earth's heat source. In pursuing this subject, he discovered that in regions of the earth's crust which are being subducted under other sections of the crust—and hence where you have a great deal of volcanic and geothermal activity—the ratio of helium-3 to helium-4 is about three orders of magnitude larger than it is over the continental crust. That made him wonder where the helium-3 was coming from. One of the possible sources of helium-3, of course, is fusion.

Certainly, there is water in the compounds forming the rocks being subducted under the earth's crust, and in this water there will naturally be found some deuterium. So d-d fusion could possibly occur.

Further geological evidence is provided by tritium levels in the atmosphere. During an eruption of Mauna Ulu, a side crater of Kilauea, a substantial tritium concentration was observed at a station near the volcano. In the subsequent period of time, the volcano died down for a while and the tritium level subsided. Then during another period of activity, the level of tritium increased again (see Fig. 2).[(2)]

It's not completely clear that fusion really has anything to do with the geological activity, but it was a tantalizing suggestion to do some experiments in that regard.

The idea basically was to get deuterium into a crystal lattice so we could see if we could somehow induce fusion in a solid. When we asked chemists how to do this, they suggested that the easiest way is by electrolysis. It turns out that if you take certain metals, such as nickel, titanium, and palladium, you can get about the same number hydrogen atoms in the material as there are metal atoms. If there is one atom of hydrogen per one atom of metal, this is equivalent to several liquid hydrogen densities. If you're busy building expensive high pressure chambers to increase hydrogen density, it's alluring to realize that the crystal lattice of a solid

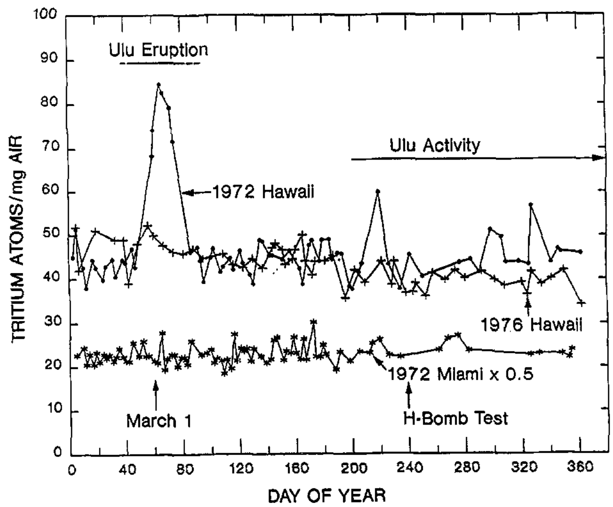

Fig. 2. Measurements of tritium in the atmosphere from various monitoring stations, correlated with volcanic eruptions of Mauna Ulu, Hawaii.

can accomplish the same thing quite easily. Of course, it should be acknowledged that if hydrogen molecules dissociate and the atoms occupy interstices in adjacent lattice sites, the atoms are farther away than atoms in hydrogen molecules.

Since the process is easy, we decided to see if anything interesting could be detected. So we just attached some simple DC power supplies to electrodes made of various materials such as titanium, palladium, and so forth. Then we put these electrodes in glass jars containing electrolytes and did some straightforward electrolysis. The hard part, however, was to detect fusion products at very low levels.

When Paul Palmer originally came up with the idea of fusion occuring in the earth, almost 3 years ago, he suggested that the neutrons produced in d-d fusion should be detectable. So he and his colleagues took the detectors they had available at the time and looked for neutrons. Unfortunately, the detectors weren't good enoug to produce any conclusive results. Sometimes there seemed to be a signal, and sometimes not. So the idea was tabled until some better detectors were available.

In the meantime, Bart Czirr and Gary Jensen of our department were in the process of developing neutron spectromeeters. So when Steve Jones revitalized interest in solid-state fusion, Bart and Gary used their latest detector to look for neutrons. This detector operates in a fairly simple manner.[3] It contains plates of lithium glass surrounded by a liquid scintillator. The liquid scintillator serves to moderate the neutrons, giving off light in the process. Once the neutrons have slowed down to thermal energies, they're absorbed in the lithium-6 glass which also scintillates.

In order to get an acceptable event, one signal from the liquid scintillator must be followed by one signal from the glass. The pulse shapes from the liquid and glass are quite different and hence can be easily distinguished. Since the glass is insensitive to radiation other than neutrons, we can be fairly certain that a neutron has stopped in the detector whenever both signals are present. So it's a very good detector to discriminate against most kinds of background. In the mode of operation used at that time, about three-fourths of all background events were cosmic ray neutrons.

But we also can get a measurement of the neutron energy by determining the total light output of the liquid scintillator. It is not a very accurate spectrometer when compared to charged particle detectors, but it does give a rough measurement of neutron energy. The energy can be calibrated by means of neutrons produced by our 4-MeV Van de Graaff accelerator.

The background spectrum is seen in Fig. 3. It was very constant over time. We never saw any variations in the shape of the background.

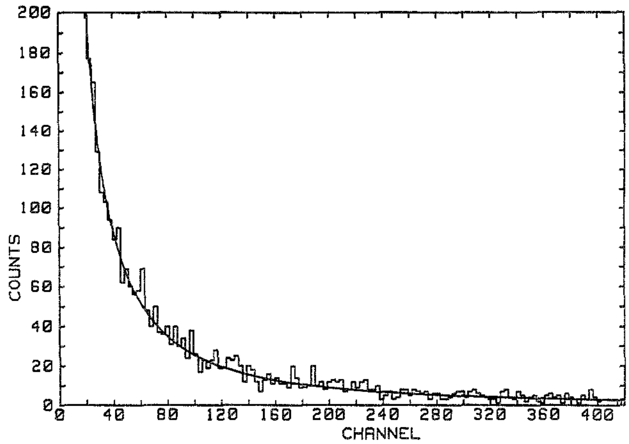

Fig. 3. Background spectrum with least-squares fit to the data (solid line) as described in the text.

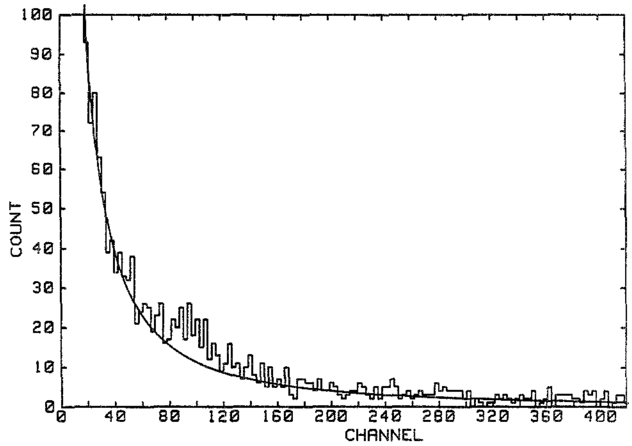

Fig. 4. Foreground spectrum with least-squares fit to the background (solid line) superimposed.

The foreground data (Fig. 4) show, however, a definite peak above the background. This peak is located at the region where we would expect to see the 2.5-MeV neutrons from d-d fusion.[4]

In our best analysis of these data to date we fit the background with a three-parameter function of the form $A/(x^2+Bx+C)$. One parameter provides the normalization and the other two the shape. The fit is excellent, as can be seen in Fig. 3. For the foreground signal, we used exactly the same shape parameters but added to that of a Gaussian peak. Again the fit is excellent, with the significance of the peak being five to six standard deviations.

So we might ask then, what causes this peak? The statistical significance of the peak suggests that it cannot simply be random fluctuations in the background. The detector is a good enough detector that we are convinced that the peak must be neutrons. The energy calibration tells us the neutrons have an energy of about 2.5 MeV. That's a pretty good signature for fusion, at least better than a lot we've seen in the news lately. Even then, we tried to do a number of things to be sure of our results.

The first thing we checked was to see that the spectrum in the regions to the side of the peak matched the background signals in magnitude and shape. That it did.

We then tried looking at cells identical to those which produced the peak, but with no current flowing. We got exactly the same results as the background runs in both magnitude and shape. We tried the process again with light water and with current in the cells. Again we had the same results as the background runs. The background shape, in fact, always seemd to remain constant, even with changes of cosmic ray flux and solar activity during that period of time.

We never could do anything to reproduce the peak, except during the runs where we had deuterium and current both at the same time in the cells. So at this time we believe the peak to be a genuine signature for fusion.

It should be emphasized that the rate of fusion is very low. The foreground spectrum was accumulated over a period of several days. We should also emphasize that this was not a carefully outlined experiment. It was rather a survey to see if an experimental program should be undertaken. We used different electrode materials, different electrolytes, and even a little different electronic configuration for some data than other data. Now, we need to do some serious research to follow up that initial survey.

One thing we have done very recently has been to perform similar experiments in collaboration with a group at Los Alamos.(5) The detectors that were used, however, were helium-3 proportional counters. These counters are very good for discriminating against anything but neutrons; however, they don't have energy information available.

In the electrolysis data, the foreground signal was generally, within statistical uncertainty, at the same level as the background. There did, however, appear to be a time dependence to the fusion rate. This is consistent with results of the BYU experiment. We usually would see no counts above background for about an hour after the electrolysis began. After that, we saw a fair number of counts above background for a period of time. Later on the signal seemed to go away again, perhaps because of corrosion of the electrodes.

In any case, if there is an indication of fusion, it is at a very low rate. It is estimated that there are no more than about 0.8 neutron produced per second. At BYU, we estimated the fusion rate to be about 0.6 per second. We probably can't take the Los Alamos electrolysis data as clear evidence of fusion, as there are certain weaknesses to this experiment. The detectors can't discriminate between lower energy and higher energy neutrons. Most of the background comes from cosmic ray neutrons and the helium-3 counters can do nothing to differentiate cosmic ray neutrons from 2.5-MeV neutrons.

But another aspect of this experiment was very interesting. With the same experimental configuration, multiplicities of neutrons within a time window of about 150 microseconds were monitored. It seems that periodically a number of neutrons were emitted in a single burst, about 53 in one burst and roughly half that in two other bursts.

To compare this result to background, the cells were removed at a certain point in time. The background seems to be quite flat with no bursts occurring when the cells were removed.

It should also be noted that these data are quite recent, so the analysis is still preliminary.

Another method of inducing fusion was originally tried by a group at Frascati in Italy. Rather than using electrolysis, they loaded deuterium under pressure and at liquid nitrogen temperatures in certin metals, and then looked for fusion events as the samples were slowly heated.

This idea was also tried at Los Alamos, and again there were bursts about 50 neutrons within a time range of about 150 microseconds (see Fig. 5). Sometimes no events were observed in given samples. Sometimes it required several heating and cooling cycles before any neutron bursts were seen.

It's hard to say exactly what's happening, but it's interesting to note that the neutron bursts usally occurred at a temperature of about $-30°C$. It is postulated that this might have something to do with a phase transition in the material but there isn't any clear evidence of that yet.

Using the same pressure-loading method, the total neutron event rate also seems to be in substantial excess of the background. And again, as is seen n Fig. 6, when data were taken with a dummy cell, nothing was observed over background levels. Preliminary analysis of these data indicates that the neutron signal is significant at a level of 11 standard deviations.

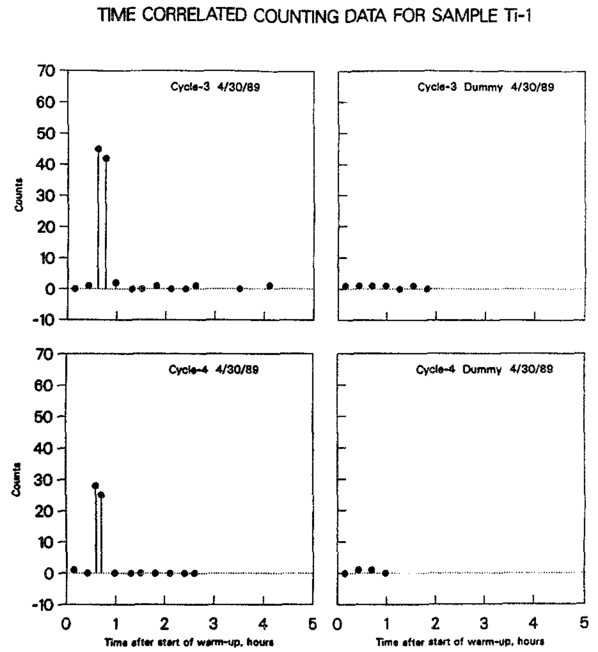

Fig. 5. Burst events from pressure loaded titanium.

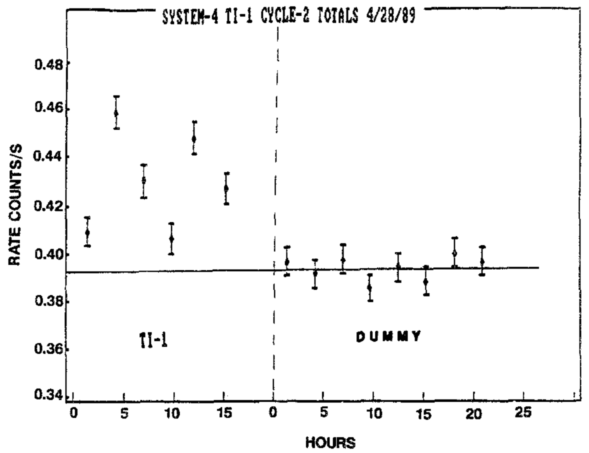

Fig. 6. Total events from pressure loaded titanium in comparison with events from a dummy cell.

How does all this occur? We don't really know. There are basically a couple of ideas people have had.

One is what Steve Jones likes to call piezonuclear fusion—pressure bringing particles closer together and causing fusion to occur. The lattice provides the source of pressure in this case. Fusion then would likely occur at a regular rate rather than in bursts. The rate would also depend strongly on the reduced mass of the system,

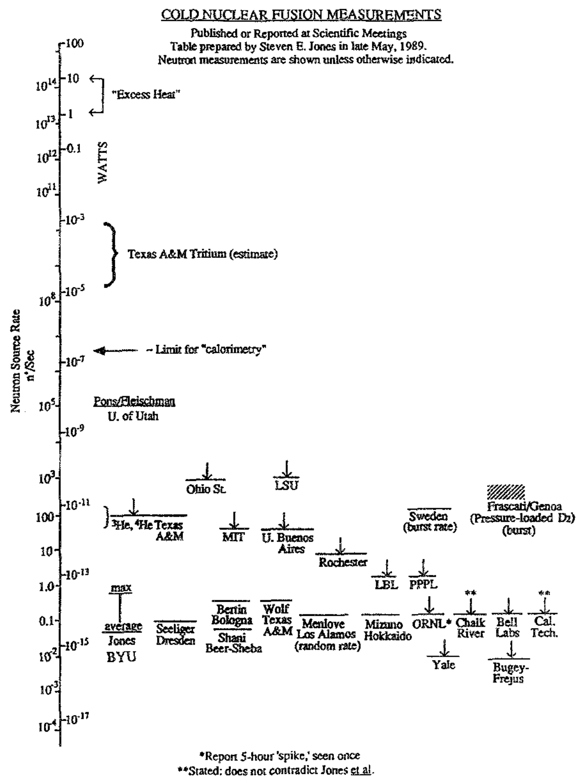

Fig. 7. Cold nuclear fusion measurement.

so we would expect to see the p-d fusion rate to be greater than the d-d rate and that to be greater than the d-t rate. That is, the lower the mass, the shorter the distance is between the particles and the greater is the fusion rate.

The other idea is what is called fractal fusion. This is an idea conceived by a collaboration from Los Alamos and Washington. This explanation is based upon the fact that in Washington it has been noticed that charge can collect across fractures in materials so as to produce large electric fields in the fracture. It is thought that in the process of heating and cooling, such fractures could develop. The field then might be strong enough to accelerate deuterons across the fissure to produce hot fusion locally. So if that's true, it's not really a cold fusion process after all. In this case, we would expect to see bursts rather than a continuous fusion process and, as happens with hot fusion, the d-t rate would be greater than the d-d rate.

The experimental evidence suggetss that we have both bursts and a continuous fusion process at the same time, so we can't draw any conclusions yet.

This system hasn't been well studied at this time, so we don't know really what is happening. Certainly other explanations are likely to be forwarded. It should be noted that a number of researchers at BYU, Frascati, Los Alamos, etc., have observed fusion rates comparable to the BYU level of 0.6 neutrons per second in samples of roughly the same size. The current experimental status of cold fusion is illustrated in Fig. 7.

In regard to Pons and Fleischmann, we simply don't believe their results to be due to fusion because of the lack of fusion products. Even if some sort of a new fusion process were occurring, ther ought to be x-rays produced. It is hard to believe that you could lose over 20 MeV in a single event and see nothing at all coming out.

In summary, the current status of solid-state fusion is that neutrons do seem to be observed at a level of five or six standard deviations in electrolytic cells, and perhaps as much as 11 standard deviations with pressure loading. But the rate is very low, corresponding to a power output of about 10^{-13} watts.

Probably the most interesting aspect of solid-state fusion is that we really don't know what is going on or why it's going on. In fact, the most important ramifications may be in geology rather than in physics. Certainly, right now we can't make any claims that cold fusion is likely to have any viability as a power source. And there are no results yet that lead us to believe that traditional fusion research should be pursued with any less vigor than has been undertaken in the past.

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Remarks of Donald P. Dautovich[1]

1. INTRODUCTION

The announcement on March 23rd from the University of Utah, which reported a demonstration of fusion at room temperature in a test tube, has brought unprecedented attention to fusion, and worldwide spontaneous activity to duplicate the findings. The effect even reached the business world causing speculation on the price of palladium, one of the test cell components.

This was followed by widespread reports through the media of subsequent confirmations, negative results, and retractions of previous positive results. This period was characterized by widespread rumor, misinformation and lack of scientific reporting and review.

Experiments were performed on variants of two different approaches. One approach which could be called the wet cell, involved electrolysis experiments with a palladium or titanium cathode and platinum anode in heavy water solutions. The other approach referred to here as the dry cell, first used at Frascati in Italy, used titanium chips in a container, pressurized with deuterium gas, cooled to liquid nitrogen temperature, and then allowed to warm up. Observations have included measurement of various forms of radiation, tritium, helium, and heat production.

2. WORK AT UNIVERSITIES AND INDUSTRY

The response in Canada to these events was rapid as it was elsewhere. The University of Toronto, McGill University in Montreal, the University of British Columbia, and the University of Alberta all started to do electrochemical or wet cell tests. York University studied deuterium discharge onto palladium. Alcan and a small company called Electrofuels also did electrochemical work. Of this work as of June 1989, only Electrofuels claimed a temperature increase.

[1] Canadian Fusion Fuels Technology Project, 2700 Lakeshore Road West, Mississauga, Ontario L5J 1K3.

3. WORK AT ATOMIC ENERGY OF CANADA LIMITED

A large effort was initiated at Atomic Energy of Canada Limited, both at their Chalk River laboratory and their White Shell Laboratory. They have used the electrolysis approach with a variety of electrodes and solutions, including palladium-silver, palladium rod, foil, sheet, and wire from a variety of sources some in cast form, basic solutions, and acid solutions. They have tried the dry cells with palladium and titanium.

General conditions included sample sizes of 1–10 grams in wet cell tests and 100 grams in dry cell tests. Wet cells were run at 50–100 mA/cm2 over durations from days to weeks. Measurements included calorimetry to 2% accuracy, neutrons to low levels, tritium both in the electrolyte and in the electrode, gammas, protons, and x-rays, and He-4 in the palladium electrode. All results have not been different from background.

4. WORK AT ONTARIO HYDRO

Ontario Hydro also started this work fairly early and have tried both the dry cell and wet cell approaches. The electrochemical work has shown no evidence in experiments designed to detect large scale heat effects. Efforts are underway to refine the calorimetry.

In the dry cell approach, a cluster of four individual samples of palladium bars, titanium chips, titanium sponge, and uranium powder, each individually in a container pressurized with deuterium gas, were located around a helium-3 neutron detection system within a paraffin moderator block. Initial work has provided evidence of neutrons above background in repeated cycles using the original samples. At this time, it has not been determined which of the samples are responding. These reports are considered preliminary and subject to additional confirmation.

5. CONVINCING RESULTS

In reviewing the available information three criteria may be used to qualify the reported results and permit some assessment of the validity of the data. The criteria used here are that the observations are significantly above background, that the instrumentation is good and that

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the work be documented and available for detailed review and assessment.

On this basis, five different sets of data have been selected as convincing as shown in Fig. 1. In wet cell tests, these include the Texas A&M data of Bockris and co-workers, where excess heat and tritium above background were reported, Huggins and co-workers at Stanford University reported low levels of excess heat, and Jones and co-workers at Brigham Young University reported significant neutrons above background.

In dry cell tests, Scarramuzzi and co-workers at Frascati reported 1000 times background neutron pulses, and Menlove and co-workers at Los Alamos National laboratory also found significant neutrons above background.

6. THEORETICAL CONSIDERATIONS

Figure 2 shows the scale of fusion probabilities comparing the probability for DD fusion at room temperature with the rates implied by selected observations. Based on these probabilities, it is difficult to accept fusion as an explanation when the probability in deuterium gas is at 10 to the minus 70 and the fusion rates that are implied by these various results are 10 to the minus 20 for the titanium in the Jones and the Frascati experiments and around 10 to the minus 8 or 10 for the Pons and Fleischman observations.

Figure 3 shows a variety of chemistry-based reactions and corresponding energies that could account for

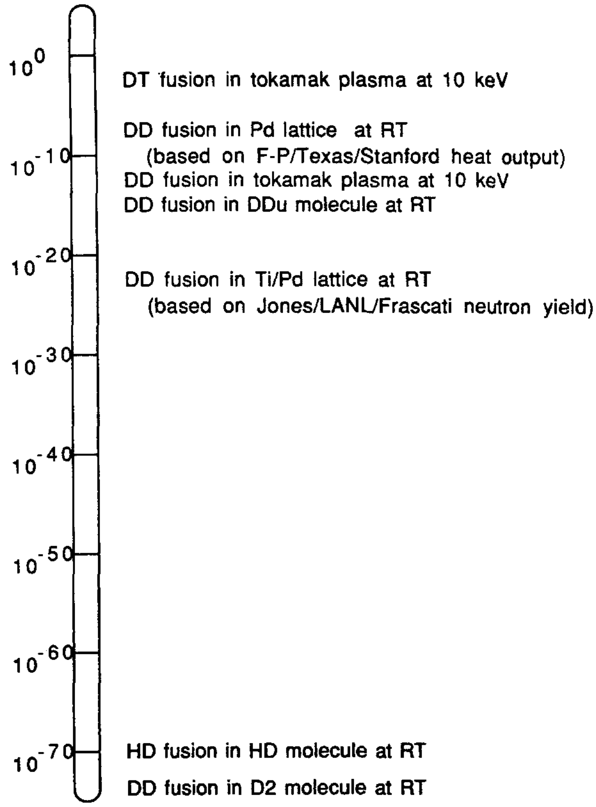

Fig. 2. Fusion probabilities (/DD/s).

<u>Wet (electrochemical cells)</u>

TEXAS A&M:
- 10% excess heat
- 1000 x T background
- No excess He-3/-4 in electrodes

STANFORD U.
- 10-20% excess heat

JONES
- 3 x background n in Utah
- repeated under Gran Sasso (Italy)

<u>Dry (high pressure D2):</u>

FRASCATI
- 1000 x background n pulses

LANL
- 100 x background n pulses

Fig. 1. "Cold fusion" convincing results.

CHEMICAL REACTIONS:
- Chemical bond energies (-2 to +7 eV/Pd)
- Hydriding energy (+0.4 eV/Pd2H)
- Corrosion (+0.9 eV/PdO)
- Li or other alloying energies
- Phase change (-0.2 eV/Pd to melt)

MECHANICAL LATTICE ENERGY:
- Dislocations in deformed metal (<0.4 eV/Pd)
- Surface energy (free or g.b.)

INTEGRAL I*(V-1.54) STORED IN LATTICE
- 1 day incubation (10 eV/Pd)

IN-SITU RECOMBINATION:
- 10% in-situ recomb (0.1 W/mL)

Fig. 3. Chemistry-based explanations.

those experiments showing low levels of excess heat. Included are energies related to palladium bonding, hydriding, lithium alloying with palladium, lattice deformation, stored energy, and recombination.

Attempts have been made to consider variations which might affect nuclear-based explanations. These have included examining the effect on tunneling caused by changing the effective mass or charge of the electron, reducing the lattice spacing of deuterium by a factor of 2, incident cosmic ray muons and high pressure deuterium in microvoids. While as a theoretical exercise, the probabilities may be raised there is difficulty in describing known processes which could cause such changes to the physical properties.

Other attempted explanations have included a hot fusion cascade initiated by the high energies associated with electrical charge built up on the sides of a crack or deposited by cosmic rays.

7. SUMMARY

Considering the experiments which have been listed as convincing in Fig. 1, two groups have provided evidence for the existence of an unidentified small heat source. The low level of heat that is produced in the Texas A&M and Stanford work, can most plausibly be explained by a chemical explanation. The absence of helium and neutrons is consistent with this explanation, however, the high tritium level observed by Texas A&M is not.

In the dry cell tests two groups present good evidence for an unidentified small neutron source. The best guess for an explanation may be hot fusion cascade. In our opinion, there is really no convincing case yet for nuclear fusion, certainly not of any practical value, but there seems to be a real effect and it has to yet be identified.

There are far more groups with good equipment who found no effect at all. There may be some possible reasons for that, but there is certainly no clear reason for it. We think that the evidence suggests that more work is appropriate. We think that funding should be commensurate with the understanding of this phenomenon and of its possible usefulness.

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Remarks of Rulon K. Linford[1]

After the detailed summaries given by the previous speakers, I will just give some general thoughts about where we are.

The conference that was held in Santa Fe a week or so ago was a very timely event. Before the conference, a lot of people had called me wondering about the appropriateness of the conference because it was scheduled such a short time following several other meetings on cold fusion. Dr. Gajewski (Director of Advanced Energy Projects, Office of Basic Energy Sciences, DOE) had called me and asked us to hold the conference. I think that his ideas regarding the purpose and timing of the conference were sound and were borne out by what happened at the conference.

The purpose was to get the people representing the full spectrum of expertise of those working on cold fusion together so they could talk about their work in a more scientific atmosphere for a period of a few days, and to draw from that interaction the necessary information that would allow more efficient scientific progress. Certainly we were not expecting that major conclusions regarding cold fusion would result from the conference itself. However, we did expect that the conference might foster more focused research activities and cooperative efforts between various research groups. I believe it accomplished those purposes very well. We are pleased with the outcome of that conference.

Table I is my summary of the observations to date. It lists the type of experiments in which excess heat and possible fusion products have been measured. The types of experiments include electrolysis, or the so-called wet cells, and the dry pressurized gas cell experiments.

As pointed out by previous speakers, the reported excess heat has ranged from the tens of milliwatts up to several watts, depending on the sizes of the electrodes, etc.

Neutron measurements of both random single-neutron events and correlated burst events have been reported. The term "burst" sometimes is used to describe events that are minutes or hours long, as in the case of the Frascati pressurized D_2 results. However, that is not what I mean by this term in Table I. Measurements at Los Alamos have identified bursts of neutrons that came out within a window of about 100 microseconds. These neutrons are probably emitted in a time much smaller than that. The characteristics of the detector prevent the narrowing of the measurement window much below 100 microseconds. The helium-3 detector uses polyethylene for a moderator. Even if, say, 100 neutrons were emitted within a nanosecond, they must be thermalized by the moderator before they are detected in the helium-3 tubes. This slowing down process spreads the detection of the neutrons in time with a characteristic width of about 50 microseconds. The overall efficiency of the detector would allow up to 33 of the 100 neutrons to be counted in the 128 microsecond window. This type of detector has been used to detect both bursts and random single-neutron events in both electrolysis and pressurized gas type experiments.

Table I points out a large discrepancy between the amount of excess heat reported and the equivalent number of watts that would be consistent with the measurements of fusion products. This is particularly true of the neutron output, either in the random mode or the burst mode. The measurement that comes closest to being consistent with the heat is the amount of tritium reported by Bockris (Texas A&M), which is still too low by at least an order of magnitude.

As Keith Thomassen pointed out, this tritium was obtained from cells where the calorimetry had not been done, and in the case of the Srinivasan-Appleby (Texas A&M) work, where they have measured the excess heat in a rather careful calorimeter, a check for tritium and helium was made but none was found.

With regard to helium and gammas, I am not aware of anybody that is now claiming a positive measurement. If there are any contradictions to that statement, I would be interested to know at this point.

Table II summarizes mechanisms that might explain

Table I. Observations

Type	Equivalent power (watts)	Electrolysis Pd	Electrolysis Ti	Pressurized D_2 gas (Pd + Ti mixture)
Excess heat	10^{-2}-10	X		
Neutrons:	$<10^{-8}$			
Random		X	X	X
Bursts		Mixture		X
Tritium	$<10^{-3}$	X		
^{3}He, ^{4}He, γ	0			

[1] Los Alamos National Laboratory, Controlled Thermonuclear Division, P.O. Box 1663, MS E529, Los Alamos, New Mexico 87545.

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Table II. Possible Mechanisms to Explain Observations

	Requirements to Explain Observation of:	
Mechanism	Neutrons	Net heat
Cold	Reduced nuclear spacing[a] by 5	Reduced nuclear spacing[a] by 10 and change branching ratio by > 10^6
Hot	Cracking yields large electric fields[b]	(Branching ratio requirement not met)[b]
Other	?	?

[a] Compared to the 0.74 Å spacing in the D_2 molecule.
[b] Branching should not change for this mechanism.

the observations. One mechanism, identified as "cold" in the table, is based on the increased probability of fusion due to quantum-mechanical tunneling as the spacing between the nuclei is reduced. The observed neutron production rate could be explained by reducing by a factor of 5 the nuclear spacing in the deuterium molecule. In order to explain the amount of net heat, you have to decrease that spacing by a factor of 10, only another factor of 2 beyond the amount needed to explain the neutrons. Even though a difference of many orders of magnitude exists between the observed neutron rates and the neutron rates required to be consistent with the excess heat, in terms of the nuclear spacing, the difference is only a factor of 2.

The impact of reducing the nuclear spacing has been well documented in muon-catalyzed fusion research. In the muonic molecule, the spacing is reduced by 200, 20 times what is needed to yield the amount of excess heat indicated in Table II. The specific mechanism that might result in reduced spacing in a solid, like palladium, has not yet been identified. Several plausibility arguments have been put forward, but none of these has yet been developed into a sound self-consistent theory.

Even more difficult to explain than the reduced spacing is a possible cause for a significant change in the fusion branching ratio. As indicated in Table II, the branching ratio would have to change by more than a million to explain the disparity between neutrons and excess heat. Again, some ideas have been offered to explain how this might happen between closely-spaced deuterium nuclei in a solid lattice, but more detailed calculations have so far failed to verify these ideas.

A rather different type of mechanism is labeled "hot" in Table II and has been called the fractile or cracking mechanism by previous speakers today. In this model, cracking of the lattice generates large electric fields which, in turn, accelerate some deuterium nuclei sufficiently to cause fusion. Because the fusing process associated with this mechanism is expected to be very similar to the process in standard hot fusion experiments, one would not expect that the branching ratio would change. Therefore, this model is not a plausible mechanism for explaining the excess heat in the face of such a small neutron production.

Of course, other possible mechanisms might explain either the neutrons or the excess heat. Significant progress could be made to narrow the possible mechanisms by careful but straightforward experiments. These experiments should focus on finding or ruling out the existence of fusion products in the amounts that are consistent with the excess heat. These fusion products have come to be known as the "smoking gun." If the smoking gun does not exist, the mechanism responsible for the excess heat is not fusion. If the smoking gun is found, the specific types of products will determine the nature of the reaction and the branching ratios that must be explained, thus providing a needed focus for the theoretical work.

The reported observation of small numbers of neutrons is scientifically intriguing even if the excess heat does not prove to have nuclear origins. However, more solid statistics are needed to facilitate research into the origin of the neutrons. The scientific community would also feel more comfortable if the observations were more repeatable and a larger number of people had observed the same kinds of results.

In the areas of excess heat measurements, it is clear that the calorimetry that is being done now is more sophisticated, and as reported in the conference, continues to indicate excess heat production. Further improvements are needed, including closing the systems and recombining the gases from the electrolyte.

In conclusion, improved collaboration between various organizations is needed in order to get more working experiments and to being the best possible diagnostic tools to bear on the experiments that are producing excess heat and/or fusion products. Fortunately, such collaboration is increasing and should yield important results in the near future.

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Remarks of Keith I. Thomassen[1]

After 2 months of intense scrutiny by scientists around the world, we have taken only the first steps toward understanding the phenomena that launched our efforts. I'd like to give my views on what we know, what we don't know, and what it means. My conclusions are that an energy source is not likely to be found in cold fusion, but that some genuine scientific curiosities may have been uncovered and should be pursued at a more normal pace. An alternative conclusion is that these phenomena are not real and the results are a misinterpretation of measurements near the limits of detectability.

Let me begin by stating what is becoming apparent—that there may be some unexpected heat flow out of these electrolytic cells and some unexpected neutrons produced in solid materials under mysterious circumstances. Furthermore, I believe these two phenomena are not connected. As has been widely noted, it is not easy to reproduce these phenomena, and it may be that we are in fact observing what Irving Langmuir called "pathological science." In a 1953 lecture to the GE Research & Development Center, he gave six symptoms of pathological science, and we can recognize in cold fusion many of these symptoms (see Table 1). Neutron output at the limit of detectability, theories of d-d reactions that are contrary to experience, *ad hoc* explanations of the heat by a new form of fusion, and the division of supporters and critics that has changed as events unfolded are symptoms we have seen.

For over 2 months at LLNL, our work has turned up no "excess heat" and we have seen no neutrons above background. Our experience is similar to that of a large majority of other major institutions, yet these null results do not disprove assertions made by scientists who do claim to see these phenomena. My summary today then is based on observations reported by other groups, and in the case of one Texas A&M group on our work with them.

Starting with the "excess heat" phenomena, it has been claimed by several groups that the heat flow from a cell operating in steady state exceeds the heat being put into the cell by the voltage source driving it. The definition of heat input taken by all these groups (except Stanford) is that it is the power from the voltage source less the amount needed to separate the water molecules into deuterium and oxygen atoms. The separation power, 1.5 volts times the current, does not appear as heat unless the gases recombine and I'm inclined to believe as do most electrochemists that the recombination heat is small. If that is the case, we have a scientific mystery which Pons and Fleischmann claimed had a nuclear solution.

Their speculation (Pons and Fleischmann) is based on dividing this excess heat among all the deuterium atoms in the palladium and noting that it amounted to several hundred electron volts of energy per atom, an amount that cannot be explained chemically. Among other conjectures, not nuclear, are the release of energy stored during the time when the material is not producing excess heat, insufficient accuracy in the calorimetry, or chemical reactions involving many times more atoms than the deuterium atoms in the lattice.

As for the release of stored energy the proponents of a positive effect will point to the steady-state output over times long compared to the charge time. As for inaccuracy in calorimetry, perhaps the most convincing case for accuracy has been made by Appleby, Srinivasan *et al.* at Texas A&M who in their experiments used a sophisticated microcalorimeter sensitive to μ-watts of heat flow. After low current charging for over 40 hours they increased the total current to 100 mA at 4 1/2 volts for another 100 hours. The 1 1/2 volts for separating the water molecule consumes 150 mW of the 450 mW applied to the cell. For some time after the current was

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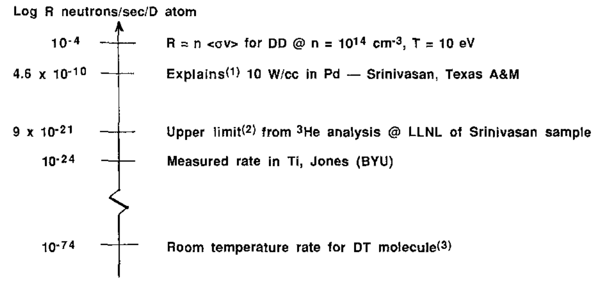
increased the heat output to the calorimeter was a steady 300 mW, but then rose to 340 mW, an "excess" of 40 mW. This excess was not observed with light water or other cathode material. The 40 mW level was not sustained when NaOD replaced LiOD, but came back when the NaOD was removed and LiOD put back in. For the small volume of Pd used in the experiment this heat translates to about 20 W/cc.

An even more dramatic claim comes from Huggins at Stanford who reports more heat output by 12% than the total power into the cell, not subtracting the work of separation. His results are based not on conventional calorimetry, but by "calibrating" the cell with an independent heater. Monitoring the temperature difference $\triangle T$ between the cell and its bath, a plot of $\triangle T$ with power is made. For calibration the heater is used; otherwise, the power is the total voltage times the cell current. Initially, the slope of this line is less than that generated during the calibration run, but as the cell runs for a few days the slope exceeds that of the calibration run, indicating excess power production over that from resistive heating. The slope measurement is made at various times during the experiments and the calibration repeated throughout. A key issue here is a thorough analysis of this method as a substitute for calorimetry, but on the surface it would imply excess heat generation.

Evidence against a nuclear explanation in the Appleby experiment came from LLNL, where analysis of the Pd wire showed no helium generation and analysis of the electrolyte showed no buildup of tritium over the amount in the original electrolyte. Our method for measuring the ^3He content in the Pd is sensitive down to 3 $\times$ 10^5 atoms, three orders of magnitude more sensitive than equipment used at Rockwell. Since the sample weighed ~ 20 mg, the reaction rate is limited to 10^{-18} D-D neutrons/sec per deuterium atom if the ^3He were accumulated in 100 hours (see Fig. 1). In cells run at Stanford with purported excess heat, the helium analysis has not been done, and results of analysis from material used by Pons and Fleischmann have not been announced. Another set of cells at Texas A&M, cells not put into a calorimeter, were sampled at various times during their operation and substantial tritium buildup was reported, contrary to the LLNL analysis. This issue needs to be resolved.

Turning to neutron production, perhaps the most convincing evidence of room temperature fusion events comes from the Frascati-type experiments now repeated at LANL. There, the neutron bursts begin an hour after warm-up from ℓN_2 temperatures. At that time, the high pressure gas cylinderswith Ti and Pd materials in them reach $-30°C$. These bursts give off 10–300 neutrons in



Notes: (1) Pd $\Rightarrow$ 106 g/mole, 12 g/cc $\Rightarrow$ 5.5×10^{22} D atoms/cc in 0.8D/Pd
So, 10 W/cc = $5.5 \times 10^{22} \times R \times 2.5 \times 10^6$ eV $\times 1.6 \times 10^{-19} \Rightarrow R = 4.6 \times 10^{-10}$
(2) 3×10^5 atom ^3He/0.02 g in 100 hr $\Rightarrow$ 500 atom ^3He/sec/cc Pd
So, 500 atom ^3He/sec/cc Pd $\div$ 5.5×10^{22} atom D/cc Pd = 9×10^{-21} events/sec
(3) R $\Rightarrow$ 10^{+10} when muons catalyze DT molecules

Fig. 1. Fusion reaction rate—cold fusion.

less than 200 μs, and the bursts cease as the cylinder continues to warm up. These experiments are reminiscent of the Soviet work by Klyuev *et al.*[1] on high-energy processes accompanying the fracture of solids. They observed electric fields between the walls of a propagating crack on the order of 10 V/cm, and accelerating voltages of 100 kV and above as detected from x-rays with energies >> 50 keV. They also claim a neutron yield of about 10^{-11} per deuteron when the material (a LiD pellet) is fired against a metal plate and shattered. Here again there is a valid scientific curiosity in my view, but the neutron output is so low as to be uninteresting as a potential energy source. Further, there are questions being raised concerning spurious signals generated in the detectors in these experiments. Until those questions are resolved, one cannot assume the signals come from neutrons in the samples.

Finally, I'd like to comment on the negative results from numerous institutions carrying out cold fusion experiments. While such results dominate the reports from around the world they cannot negate well-documented positive results. Will the mystery be resolved by finding the key to replicating the phenomena of excess heat and neutron production or by finding that the positive results were in fact, spurious? That is the issue that must be addressed, and it is likely to require much more time and care than has been expanded to date.

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