EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

CERN/EP 90-36
15 March 1990

THE RISE AND DECLINE OF COLD FUSION

Douglas R. O. Morrison
CERN, Geneva, Switzerland

ABSTRACT

The continuing history of Cold Fusion is reviewed. The initial world-wide explosion of interest in a potential source of unlimited energy with little pollution, was initially confirmed experimentally but soon major discrepancies were recognised and many experiments could not find any evidence for Cold Fusion. There is a new phenomenon – the Regionalisation of Results where now Cold Fusion is only observed in some region of the USA, Japan and India while results are now all negative in Europe and much of the USA and elsewhere. These contradictory results are explained in terms of Pathological Science. The role of the media and electronic mail are described.

Earlier Version Published in Physics World, February 1990

1. INTRODUCTION

We are all ecologists now. So when Profs. Fleischmann and Pons announced on 23rd March 1989, that at Utah, they had caused deuterium ions to fuse giving out heat using electrolysis in a simple cell at room temperature - Cold Fusion - we all wanted to believe it. At first we were a bit sceptical, but then came more information - they had measured excess heat and observed neutrons, gammas, and tritium! And next day there was independent confirmation from Steve Jones [1] of nearby Brigham Young University. Other confirmations followed quickly. The early days of April were the high point when perhaps 500 million people had heard of Cold Fusion, Fleischmann and Pons, and had dreams of sea water yielding limitless amounts of heavy water that could provide energy without pollution! But scientists quickly realised a terrible discrepancy - for each Watt of power there should be 10^{12} neutrons per second but only a few were observed - less than one per second for Jones. Hopes were briefly revived again on 18 April when Prof. Scaramuzzi of Frascati showed results of apparently high statistical significance and suggested fusion as a dynamical effect. Since then hundreds of experiments have been reported and most of them found no effect while some found positive results. The world became divided between "Sceptics" and "Believers" with the latter concentrated in some parts of the world such as Utah and Texas. After a while one felt that Cold Fusion could not be understood by normal Science alone and Pathological Science was invoked. This article reviews the present status of experimental results and attempts to understand the phenomenon of Cold Fusion.

It has been well known for a century that Palladium and certain other metals can absorb large quantities of hydrogen. The idea came independently to Fleischmann and Pons and to Jones that if deuterium could be forced into Palladium, two deuterium nuclei would come so close together as to fuse giving out more power than was being put in. To do this they used a simple electrolytic cell with heavy water as the electrolyte and with Palladium or Titanium as the cathode at which the deuterium would be released.

The d-d fusion processes are well-studied and are;

2. MILESTONES

- 13 March Fleischmann informs David Williams at Harwell who starts experiments.
- 23 March Fleischmann and Pons at press conference claim heat, neutrons, gammas and tritium.
- 24 March Jones press conference - claims neutrons.
- 31 March Fleischmann lectures at CERN - very successful but admits they had not tested with normal water.
- 7 April Meeting of American Electrochemical Society, Texas - great triumph.
- 9-12 April First copies of Fleischmann and Pons paper [2] received - found to be unsatisfactory. Growing doubts, especially because of discrepancy between amount of excess heat and very low numbers of neutrons claimed.

David Williams says Harwell have not observed neutrons at the levels claimed. The number of characteristics of Pathological Science keeps rising.
- 15 April Most people believe in Cold Fusion except those widely informed (e.g. by electronic mail) who know of null experiments and of discrepancies.

- 18 April Scaramuzzi (Frascati) [3] apparently finds strong evidence for a dynamic origin for Cold Fusion using Titanium, D₂ gas pressure and temperature.
- 24 April Report that Fleischmann and Pons claim helium has been detected. Doubts about Scaramuzzi results on Email [4].
- 1-2 May American Physical Society meeting. Strong negative results from Nathan Lewis (Cal Tech) [5] and Moshe Gai(Yale-BNL)[6]. Regionalisation of Results reported - negative results in Northern Europe and in Region 1 of the USA (major labs plus North-east), positive results from Southern and Eastern Europe, Region 2 of the USA, and the rest of the world. High score on Pathological Science characteristics - 7 out of 12 [7].
- 8 May American Electrochemical Society meeting - media triumph for Cold Fusion - but Sceptics are excluded except a few after protests.
- 23-25 May Santa Fe meeting on Cold Fusion organised by Los Alamos for DOE. Most Americans interested (though not Fleischmann nor Pons) plus some others attended. Attention given to neutron bursts reported by Menlove of Los Alamos and Jones [8]. Gai and Jones agree to do joint experiment at Yale. Although there were more negative results than positive, the organisers tried to be "fair" and have equal numbers of positive and negative results presented so that for most watching on satellite TV, the conclusions were unclear.
- 15 June Harwell press conference [9] - major series of experiments costing 320 000 pounds and using 4 million pounds worth of equipment, have found no effect and hence are being stopped. And this despite initial help from Martin Fleischmann. However other positive and negative results continue to be reported. Utah particularly encouraged by reports of large amounts of tritium found at Texas A&M.
- 12 July DOE panel [10] give interim report - say "the experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomenon attributed to cold fusion". "No special programmes to establish cold fusion research centres.... are justified"
- August National Cold Fusion Research Institute established in Utah with money from the State of Utah. It is hoped to get funding from the EPRI (Electrical Power Research Institute) which has been subsidising other groups.
- August Japanese Cold Fusion Research Institute established.
- 15-16 Sept. Conference in Varenna. Italian groups either cannot repeat positive results or find alternative explanations of effects.
- September Reports of experiment [11] in Pons's lab with counters under a table which had Pons's cells on top. They did not find neutrons or gammas. Lecture in Utah summarising there for the first time all results and theory and concluding is Pathological Science. Director of National Cold Fusion Research Institute, Hugo Rossi says they have found nothing so far and if still unsuccessful by February will consider stopping.
- 28 October Gai and Jones report no neutrons nor neutron bursts.
- October NSF and EPRI sponsor conference - restricted attendance with token sceptics - media success for Cold Fusion.
- 12 November DOE Panel final report confirms earlier opinion.
- November Rossi leaves as Director of National Cold Fusion Research Institute.
- December Japanese press (and Wall Street Journal) gives prominence to reports of large number of neutrons from two Japanese groups.(Later examination causes these results to be doubted - see below). Reports of some 200 scientists in Japan

working on Cold Fusion. Book of papers [12] from the Bhabha Atomic Research Centre, Trombay, Bombay describing some 6 experiments where neutrons or tritium were observed. Over 50 scientists and engineers besides a large number of technicians from more than ten divisions worked on these experiments.

1990

- 29-31 March Conference on Cold Fusion, Salt Lake City.
- 26-29 July Tesla Symposium, Colorado Springs.

3. EXPERIMENTAL STATUS OF EXPERIMENTS

The present (up to early December 1989) experimental situation based on experiments that are at least moderately well described (most are unpublished and are received by electronic mail or privately, so that the collection cannot be complete but is large and any bias small).

3.1 Neutrons

(a) Steady production

Ten positive results of which two have been withdrawn (Fleischmann and Pons and Georgia Tech). Of the six of these for which the actual measurement rate (before correction) and the background rate are known, all had a neutron level 3 to 5 times the background. Thus although the claimed rate varied between 0.04 and 40000 neutrons per second, no one had observed a rate that was many times background and all were far from the rate of 10^{12} n/s which one Watt of power should give. Nineteen experiments have reported no significant production of neutrons. If we take the level of Jones et al. as unity, then in general terms, eight of the experiments report upper limits that are about a factor of ten lower and four give upper limits that are about a factor of hundred lower than Jones et al.

(b) Dynamic Effects - temperature and pressure changes, Frascati-type

Prof. Scaramuzzi showed provisional results [3] suggesting that by varying the pressure and temperature, the resulting non-equilibrium conditions could produce neutrons. He has not been able to reproduce these results since April. Three other groups were able to observe this effect initially but then were not able to reproduce it and indeed found reasons to explain that their positive observations were mistaken (acoustical effects, humidity, etc.). Four other groups have reported finding no effect and two have given upper limits that are a factor of a thousand less than that of Frascati.

(c) Bursts of Neutrons

At Los Alamos, Howard Menlove, Steve Jones et al. [8] found bursts of neutrons at a very low level. Four other groups have found no bursts. Steve accepted Moshe Gai's invitation to do a joint experiment at Moshe's lab at Yale. They reported to the Dept. of Energy Panel [13] that they had found no bursts that could not be accounted for by cosmic rays. Wada et al. [14] of Nagoya wrote that after a high voltage discharge through D₂ gas from lightly loaded Pd cathodes ($D/Pd < 0.3$), a series of decreasing bursts of neutrons were detected. They claimed a peak rate 10 000 times background but only used one BF₃ counter and these are notoriously unreliable, could not check if the counts were caused by neutrons (since the cathode was damaged) and did not try a control with normal hydrogen gas.

3.2 Gammas

The only positive result was that by Fleischmann and Pons and that has been withdrawn. Nine groups have reported finding no gammas, with levels as low as one gamma per second.

3.3 X-Rays

When palladium is excited it emits 21 keV X-rays. The creation by fusion of protons, tritons, ^3He or gammas in the palladium should cause the palladium to be excited. Four labs have reported that no 21 keV X-rays have been observed giving strong evidence for the absence of fusion products and hence of fusion.

3.4 Tritium

The situation is confused. The original claims of Fleischmann and Pons have been withdrawn. Texas A&M [15] report copious production with published rates corresponding to 10^{-3} to 10^{-8} Watts. In Los Alamos a few cells are said [16] to give tritium but some 70 do not. Two groups have unpublished reports of finding deuterium. Five groups find no tritium production and give low upper limits. If the Texas A&M findings were correct, enormous rates of neutrons should have been produced (since the rates of reactions (1) and (2) are known to be equal) and these are not observed, this is assuming the known physics results are applicable.

3.5 Charged Particles

In a recent headline-making report in Japanese newspapers, Taniguchi et al. [17] of Osaka report measurements of charged particles using a silicon surface barrier detector placed next to one wall of the detector which is actually a 10 micron thick palladium and steel cathode. The counting rates are very low and no attempt was made to exclude cosmic rays which must have given occasional higher counts (as was found for instance by Gai and Jones).

3.6 Calorimetry

The original Fleischmann and Pons electrolytic cells had a simple design and are "open" which means the D_2 and O_2 gases produced are allowed to escape. Many later workers used similar cells. The estimates of excess heat depend on the calibration where the cell is heated and its rate of cooling observed. It has been shown that the results depend critically on the calibration and there are important assumptions frequently employed. A safer technique is to use a constant temperature bath, where the cells and their surroundings are heated to a temperature slightly higher than ambient - any excess heat is measured by the reduction of heating required to restore the constant temperature. The best design is a "closed" cell where the D_2 and O_2 gases are recombined with a catalyst (usually Pd) inside the cell and the whole kept at a constant temperature.

Of the 8 labs that reported excess heat, all were "open" and not kept at constant temperature. While most of the reports were of 8 to 50% excess heat (or more accurately power), Fleischmann and Pons have claimed 10 to 50 Watts. Of the 14 labs reporting no excess heat, 7 were of this "open" type and gave upper limits of 0.2 to 2% excess heat or < 0.3 Watts.

Of the 5 labs using constant temperature cells, all found no excess heat giving upper limits between 0.3 and 9% or 0.1 Watt.

Two labs (British Columbia [18] and Karlsruhe[19]) used "closed" cells. They gave upper limits of 0.3% of the 4 to 18 Watt range and 1 to 3% of the 10 to 30 Watt range resp.

The balance of the evidence is that excess heat cannot be produced in a useful manner. The positive results are generally said to give excess heat erratically and in bursts which are claimed to last for many hours. It is hard to prove or to disprove such claims and many neutral people feel that some interesting physics might come out of further careful peer-reviewed studies. *On the other*

hand when Dr Salamon and his colleagues [11] had his neutron and gamma detectors installed under the table in Dr Pons's lab and Dr Pons's group had four cells running on that table for a total running period of 5 weeks in May and June, it is surprising that they were not able to produce any excess heat for their colleagues and indeed no neutrons and gammas were observed.

3.7 Muon - induced Fusion

Since it is known that muons can replace electrons in a D_2 molecule pulling the nuclei together and causing fusion (Steve Jones is an expert on this), it was hoped that muons would do the same in palladium. Muon beams have been fired into Palladium at MIT and KEK but no effect has been found. KEK deduce that cosmic ray muons should produce less than 10^{-6} neutrons per second. Tests with cosmic rays confirm this.

3.8 High Pressure

High D_2 gas pressures of 105 kbar and a megabar have been tried but no appreciable number of neutrons have been observed.

3.9 Is there a Secret?

In Pathological Science when an effect cannot be repeated, it is often said that there is a secret and the reason that someone does not find it, is not because the effect does not exist, but because he does not have the special technique or secret. Hence in early April asked both Martin Fleischmann and Steve Jones if there were a secret - both replied laughingly that there was no secret - a simple table-top experiment!

4. PREVIOUS KNOWLEDGE OF DEUTERIUM IN PALLADIUM

A major criticism of the original proposers who expected deuterium fusion in metals such as palladium, is that the literature shows that the deuterium ions are actually much further apart in the Palladium than they are in simple gaseous deuterium so that no useful fusion is to be expected. In gaseous or liquid deuterium the two deuterium nuclei have a separation of 0.74 Å. The palladium nuclei in the crystal are 3.89 Å apart but as the deuterium is loaded in they are moved apart to 4.03 Å. When the deuterium is first loaded, up to $D/Pd = 0.8$, the deuterium nuclei go into the octahedral spaces and are 2.85 Å apart. If it is possible to increase the loading (e.g. by ion implantation), then the O-D separation is 1.74 Å. To increase the probability of fusion it is necessary to bring the deuterium ions much closer together, e.g. when a muon replaces an electron, the nuclei are pulled together and the separation is only 0.0035 Å and the fusion rate is reasonable. For deuterium placed statically in a palladium lattice the barrier penetration probability is too low by a factor of about 10^{-40} [20]. Thus the expectation of fusion in a continuous fashion is unreasonable. For it to occur in a dynamic fashion is also unlikely as there is lots of space for the deuterium ion to wander between the ions in the lattice. Furthermore there is a theory of hydrogen ions in Palladium which is very well tested (by neutron and muon scattering, etc.) and which describes the distribution of the electrons very completely. One does not expect muons to cause measurable fusion in loaded palladium because the negative muons are quickly captured by the palladium.

5. THEORIES OF COLD FUSION

Most theories assume that Cold Fusion exists with the rate claimed and then try to explain how it occurs. They do not start from what is already known about the distribution of ions and electrons in the metal. A frequent assumption is that since the fusion products (neutrons, protons, gammas) are not observed but heat is, then the whole lattice takes up the energy. But since a single ion can take up only keV while the energy released in fusion is a thousand times more, MeV, it is

assumed that thousands of ions must share the energy. However there is a major objection to such theories - the time for the fusion interactions is less than 10^{-20} seconds while the time for the lattice to respond and pass along energy is about 10^{-10} seconds so that there is a discrepancy of many orders of magnitude.

6. EXPERIMENTAL AND THEORETICAL CONCLUSION

In conclusion, both experimentally and theoretically, the balance of the evidence is that fusion does not occur in metals such as palladium in electrolytic cells at any significant rate. As for the question of the heat excursions claimed, if they have any reality, they are not associated with fusion at cold(room) temperatures.

7. INFLUENCE OF THE MEDIA AND ELECTRONIC MAIL

One respect in which cold fusion differs significantly from previous examples of Pathological Science, is the major role that the media has played. The initial announcements were made by press conferences - this is not conventional in Science, but could be justified if it was really believed that this was a major new source of pollution-free energy. But then there was a rash of press releases. The State of Utah was very anxious to solve its economic problems and to contribute in a major way to science, but it is questionable whether it was wise to employ immediately the originator, Dr. Cassidy, of "pork-barrel science" - this is the technique of funding projects not by peer-review and going through the normal democratic and open channels, but by arranging for a local senator or congressman to add the project on to another bill which had nothing to do with the proposal. Since then media techniques have been fully exploited to promote cold fusion, but what is new is that twice meetings were organised to which invitees were carefully chosen - "we only want people who have found something as they have something positive to contribute" - but excluding those who have upper limits as this is negative!

Electronic mail has expanded dramatically, particularly in the particle physics community who normally engage in large experiments with international collaborations. The extensive use of press conferences and press releases stimulated scientists but did not provide the detailed experimental information needed to evaluate the effects claimed, and the scientific papers written were often weak in details (perhaps because of patent lawyers) or slow in appearing. This gap was filled by extensive exchange of electronic mail by many physicists and by Fax. The author sent out an electronic news letter on Cold Fusion to his physics collaborators after Martin Fleischmann's 31st March lecture at CERN and this grew into a regular informal Email newsletter. There is also network bulletin board where people exchange news and opinions on Cold Fusion. While it is sometimes said that cold fusion has allowed non-scientists to see Science in action, it is not sure this is necessarily a typical example, more a very extreme poor example which could cause harm - as one person said to me "I used to think doctors never made a mistake, now with cold fusion, I am beginning to change my mind about Science too".

8. FINAL REPORT OF THE DOE PANEL

The Final Report in November by the American DOE Cold Fusion Panel concluded very diplomatically, that "the experimental results on excess energy from calorimetric cells reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion". Further it said that as not all results are fully explainable, some small experiments are justified - but unlike the Interim Report it did not say that these experimental proposals should be peer-reviewed before being funded. This is a pity as one of the arguments about "pork-barrel science" was that funding was given without peer-review.

9. PATHOLOGICAL SCIENCE

How to understand the contradictory results? In 1953 Irving Langmuir gave a delightful lecture on Pathological Science (reprinted in the Oct. 1989 issue of Physics Today) where he discussed some cases such as N-Rays, where a number of good scientists reported wrong results. He gave six characteristics of such cases. One, which I have slightly modified, is to say that there are three phases; in Phase One the original report is quickly confirmed; in Phase Two there are about equal numbers of positive and negative results; and in Phase Three there is an avalanche of negative results. In preparing a review [7] of Cold Fusion for the May APS meeting, was surprised to find that in Northern Europe and the USA area 1 (the major labs and the North-East) the results were almost all negative whereas in Eastern and Southern Europe, Asia, Latin America and USA area 2 (the rest of the USA) the results were almost entirely positive. The numbers were statistically significant being one positive and 18 negative in the first regions and 25 positive and 2 negative in the second case. Thus the first area was already in Phase Three while the other was in Phase One. During the month of May this Regionalisation of Results continued with the first area giving 2 positive and 16 negative while the remaining regions of the world switched to six positive and 11 negative, i.e. it entered Phase Two. This regionalisation has continued with now most of the world finding negative results (in Phase Three) and only Utah, Texas, India and now Japan being in Phase Two where both positive and negative results are reported. The final conclusion may be that a desire to achieve the result expected in one's local community, does influence a certain number of scientists for a long or a short time. Most face up to the totality of evidence fairly quickly, but a few never do. Thus after Blondlot's N-Rays were exposed as illusionary, he none the less published a book on N-Rays a year later, omitting the negative evidence.

10. CONCLUSION

This is a brief summary of what has become in a short time a subject with a vast literature which touches on many aspects of chemistry and physics and which is sometimes very interesting scientifically, sometimes funny and sometimes sad. Cold Fusion will live for a long time in people's memory.

REFERENCES

- [1] S.E. Jones et al., Nature 338 (1989) 737.
- [2] M. Fleischmann, S. Pons and M. Hawkins, J. Electroanal. Chem. 261 (1989) 301 and erratum 263 (1989) 187.
- [3] A. de Ninno et al., Europhys. Lett. 9 (1989) 221.
- [4] D.R.O. Morrison, Cold Fusion News, No. 10, Email.
- [5] N. Lewis et al., Nature 340 (1989) 525.
- [6] M. Gai et al., Nature 340 (1989) 29.
- [7] D.R.O. Morrison, review talk at APS Meeting, Baltimore, 2 May 1989 and Cold Fusion News No. 13, Email.
- [8] H.E. Menlove et al., Workshop on Cold Fusion, Santa Fe and submitted to Nature.
- [9] D.E. Williams et al., Nature 342 (1989) 375.
- [10] Interim Report of the Cold Fusion Panel to the Energy Research Advisory Board (US-DOE), Co-chairmen John Huizenga and Norman Ramsey.
- [11] M.H. Salamon et al., submitted to Nature.
- [12] "BARC Studies in Cold Fusion", edited by P.K. Iyengar and M. Srinirassen, Bhabha Atomic Research Centre, India, BARC-1500, Dec. 1989.
- [13] Cold Fusion Research, A report of the Energy Research Advisory Board (US-DOE), Co-chairmen John Huizenga and Norman Ramsay.
- [14] N. Wada and K. Nizhizana, Japanese J. of Appl. Phys. 28 (1989) 2017.
- [15] N.J.C. Packham et al., J. Electroanal. Chem. 270 (1989) 451.
- [16] E. Storms, private communication.
- [17] R. Taniguchi et al., Japanese J. of Appl. Phys. 28 (1989) 659.
- [18] M.E. Hayden et al., Univ. of British Columbia preprint, Santa Fe. Workshop, May 1989.
- [19] G. Kreysa, G. Marx and W. Plieth, J. Electroanal. Chem. 268 (1989) 437.
- [20] S.E. Koonin and M. Nauenberg, Nature 339 (1989) 690.