Cold fusion: comments on the state of scientific proof

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Early criticisms were made of the scientific claims made by Martin Fleischmann and Stanley Pons in 1989 on their observation of heat effects in electrochemically driven palladium–deuterium experiments that were consistent with nuclear but not chemical or stored energy sources. These criticisms were premature and adverse. In the light of 25 years further study of the palladium–deuterium system, what is the state of proof of Fleischmann and Pons' claims?

Keywords: Cold fusion, Fleischmann, Pons, scientific proof.

Introduction

THE question under discussion is whether the phenomenon known as cold fusion has been proven to be existent or non-existent. This is an important question, for if real, the possibility exists that cold fusion might become a meaningful primary energy source with few of the disadvantages associated with the power sources that we have available to us today. One expects science to be able to rationally investigate and determine answers to questions such as this. Having studied this phenomenon almost full time for the past 25 years, I will state my preliminary conclusion up front and then proceed with a more nuanced discussion. Whatever it is and by whatever underlying mechanism it proceeds, the accumulated evidence strongly supports the conclusion that nuclear effects take place in condensed matter states by pathways, at rates and with products different from those of the simple, isolated, pairwise nuclear reactions that we are so familiar with in free space (i.e. two-body interactions). The implications of this statement are profound and we will proceed with caution on the basis of validation of the envisaged new science.

Discussion

Occasionally, with decreasing regularity, one hears statements to the effect that 'Cold fusion has been proven to not exist or to have been based on errors'. Almost always the words 'long ago' are appended. Never are examples of error given at any level of scientific sophistication. If pressed the authority of experts in the fields of nuclear or particle physics are invoked, or early publications of null results by 'influential laboratories' – Caltech, MIT, Bell Labs, Harwell. Almost to a man these experts have long ago retired or deceased, and the authors of these early publications of 'influential laboratories' have long since left the field and not returned. The issue of 'long ago' is important as it establishes a time window in which information was gathered sufficient for some to draw a permanent conclusion – some time between 23 March 1989 and 'long ago'. Absurdly for a matter of this seeming importance, 'long ago' usually dates to the Spring Meeting of the American Physical Society (APS) on 1 May 1989. So the whole matter was reported and then comprehensively dismissed within 40 days (and, presumably, 40 nights). From what we now know is this sensible? Has pertinent new information and understanding developed over 25 years of further study been examined with the wisdom of hindsight? What is the status of these early null results?

Several questions lie on the table of increasing scientific interest and technical importance. Do nuclear processes ever occur at all in metallic lattices? If yes, do these occur by means differently than two-body interactions in free space? Before Martin Fleischmann and Stanley Pons' fateful press conference on 23 March 1989, most who had thought about it would have argued that nuclear processes can be caused or observed to occur on or beneath the surface of solids, but take no advantage from it. The size and timescales of atom–atom and inter-nuclear interactions are so vastly different that the chemical and physical state in which a nuclear process occurs was generally considered to have no influence over the nuclear reaction mechanism, rate or product distribution. The only case considered computationally for the involvement of materials in the nuclear process[1] was the tunnelling interaction of two like charged particles, which is strongly distance-dependent. The thinking was that the palladium lattice used by Fleischmann and Pons in their experiments might (somehow) confine deuterons sufficiently closely to 'meaningfully' (see note 1) increase the tunnelling cross-section. This popular line of reasoning ignored following three crucial details.

(1) At maximum loading of deuterium (D) into palladium (Pd), the centre-to-centre distance between adjacent

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deuterons in PdD is greater than that in D_2O. The rate of spontaneous fusion by pairwise interaction of deuterons in heavy water is not known, but it is very low. In PdD it would be less.

(2) Deuterons in PdD carry only a very small fractional positive charge. Their electrons are mostly localized so that their state is much more atomic than ionic. Calculations based on D^+ are irrelevant (see note 2)[1–4].

(3) If tunnelling interaction took place just between two deuterons, the products would be exactly those of hot fusion – a nearly 50 : 50 ratio of tritium and neutrons. Both these species would be very easily observed at the heat generation levels claimed by Fleischmann and Pons[5].

So the reaction that most people spent most time considering could not happen, did not happen, and if it did would not require palladium or electrochemistry. Given the power and energy densities of the heat effect claimed by Fleischmann and Pons[5], only one of two possibilities existed. Either they were wrong in their excess heat determinations or nuclear reactions occurred in metallic lattices by mechanisms and with product distributions different from similar reactions in free space. Is there such a thing as condensed matter nuclear science (CMNS)?

What is the state of proof? The case for cold fusion certainly has not been disproved. This would be a challenging thing to do. To proceed case-by-case and demonstrate that every instance where anomalous nuclear-products or nuclear-level excess heat were observed resulted from an identified experimental error or misunderstanding would be exceedingly arduous undertaking and nothing like this has been attempted or ever will be. The effort of finding a mistake in all of the thousands of published reports would be far too great an undertaking even to begin, thus proving a negative is difficult if not impossible. A preliminary flurry of objections, some valid and some not, was directed at early cold fusion results (not all of which were sound). Most of these criticisms were founded on the complaint that cold fusion does not behave like hot fusion. The counter argument was made compellingly by Julian Schwinger with the statement that 'the circumstances of cold fusion are not those of hot fusion'[6]. The fact that the reaction occurs in lattice-constrained space in intimate (and possibly coherent) association with unknown and uncountable numbers of other nuclei (and electrons) makes a difference in the reaction expectation and outcome. But this is not a theoretical matter – one cannot 'theoretically deny' a new experimental observation unless a fundamental law is clearly violated (see note 3). In the scientific method experiment always takes precedence. How far along the trail of experimental demonstration are we?

I would argue that the condition of certainty is approached asymptotically – we achieve the comfortable condition of 'knowing' by painstaking repetition and accumulation of knowledge over periods of years, decades or generations. Very few people ever attempt this exercise – those that do are called experts – those who do not look to these experts for answers. What is sought is not fact, but patterns and consistencies of behaviours. In his most recent book[7], Ed Storms reviews over 900 publications sorting through these patterns in the attempt to create systematic order for those of us with less time, talent or devotion. By any standards Storms is an expert on the subject of cold fusion – one could argue that he is the preeminent expert on this topic. But where does one go to get a countervailing 'expert' opinion? I would argue that there is no such place or person and has not been for more than two decades, and that this is a problem. Individuals selected to evaluate the accumulated evidence or some subset of evidence with an open mind invariably come to the conclusion that the case for cold fusion is not disproven (the experience of Rob Duncan and 60 min comes to mind[8]). To hear a counter argument one must approach experts in related fields and ask their opinion about matters that they have not studied. Of course, all one can expect is an intuitive, emotive or self-serving response.

How does one proceed as a thoughtful intelligent person simply wanting 'to know the truth (see note 4)', but not having years to devote to experimental studies or literature review? I would suggest beginning with Storms' books[7,9] as resources to identify sub-topic areas of personal interest and pointers to primary sources for further study. Obviously, I have neither the time, patience nor space to emulate Storms' efforts here. I restrict attention to the conclusions arrived at 'long ago' in the deluge of information achieved hurriedly in the biblical 40 days and 40 nights leading up to the 1 May 1989 APS meeting. The conclusion and 'voted consensus', that Fleischmann and Pons had made fundamental errors and elementary mistakes, was itself premature and in error. This leaves wide open the possibility that our free-space view of nuclear physics requires extension in potentially interesting directions.

Several authors have shown that what is now known as the Fleischmann–Pons Heat Effect (FPHE) is not observed with Pd wire cathodes until the D/Pd atomic ratio reached 0.85 or higher. This effect of the D/Pd loading ratio on excess power production was reported simultaneously and independently by McKubre *et al.*[10] and Kunimatsu *et al.*[11] at a conference in Japan in 1992 three and a half years after the 'APS consensus'. For bulk pure palladium wire cathodes such as those used by Fleischmann and most early replicators, the problem is compounded by the multi-threshold nature of the FPHE. Not only does initiation of the effect require D/Pd loadings rarely achieved before 1989, these loadings must be maintained for hundreds of hours in the presence of threshold current densities of 100 mA cm^{-2} or larger, well beyond the current density of maximum loading. Of equal importance, surface damage and poor interface conditioning

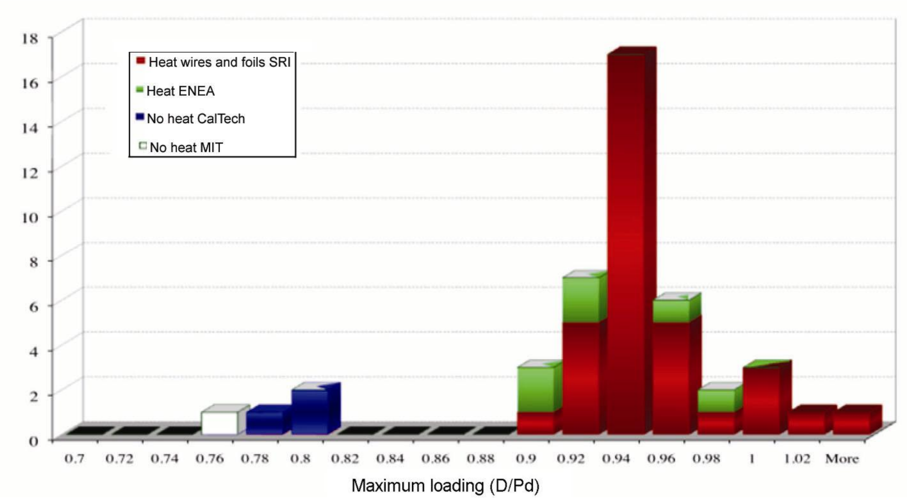

Figure 1. Histogram illustrating the number of early experiments at SRI International (formerly the Stanford Research Institute and ENEA (the Italian National Energy and Environment Agency) showing measurable excess power as a function of maximum cathode loading. Also illustrated are points for the Caltech[13] and MIT[14] and null experimental results.

and control, reduce the flux of deuterium through the interface. The magnitude (but not direction) of this flux is now known to be proportional to the magnitude of the excess heat effect as expressed in the following empirical equation[12]

$$P_X = M(x - x°)^2\ (i - i°)|i_D| \text{ at } t > t°, \tag{1}$$

where P_X is the excess thermal power, x the atomic loading ratio D/Pd, $x° \sim 0.875$, i the electrochemical current density for the cathode, i_D the absorption deuteron flux through the surface expressed as current density (2–20 mA cm^{-2}) and $t° > 50$ times the deuterium diffusional time constant in the cathode.

The failure to meet one or more of the (now) known threshold conditions provides an easy explanation for important early failures to reproduce what is now called the Fleischmann–Pons heat effect. As noted above, large significance was attached to early null heat results reported by a small number of groups at prestigious institutions. In light of the above discussion, it is useful to examine whether these experiments, as well as other early experiments, were operated in a relevant regime. The most cited early result reporting no anomalous effects was that of Lewis *et al.*[13] from Caltech, in which they stated that 'D/Pd stoichiometries of 0.77, 0.79 and 0.80 obtained from these measurements were taken to be representative of the D/Pd stoichiometry for the charged cathodes used in this work.' Also widely cited is the early null result of Albagli *et al.*[14] from MIT, who discuss 'average loading ratios were found to be 0.75 ± 0.05 and 0.78 ± 0.05 for the D and H loaded cathodes, respectively'.

The Caltech and MIT reports of no excess heat effects are shown in Figure 1, illustrating a number of early SRI and ENEA (Frascati) experiments producing positive excess power results as a function of maximum loading achieved (see note 5). Even lower loading results were estimated by Fleming *et al.*[15] at Bell Labs in another report of no excess heat. The authors[15] state 'the degree of deuterium incorporation was comparable to that for the open cells for the same time duration. The amount incorporated in longer electrolysis experiments was typically PdD_x ($0.45 < x < 0.75$).'

Conclusions

From what we know today, and Figure 1 clearly illuminates, none of the cells in any of these early cited null studies would be expected to produce any excess heat. Not only for the reasons of a loading deficiency (as stated explicitly); the durations of the experiments were wholly insufficient. The Caltech work[13] was completed and conclusions made public within 40 days of the Fleischmann and Pons public announcement. None of the Caltech experiments was operated for the 300 h (12.5 days) that was the minimum initiation time observed at SRI for bulk Pd cathodes and the entire set of Caltech experiments was complete well within the 1000 h (42 days) established as the minimum time of observation at SRI (see note 6). In addition, the current density stimuli used in these early null experiments were too small to reliably produce the effect and the deuterium flux was not measured. None of the criteria of eq. (1) was shown to be met, at least three demonstrably were not. In hindsight it is evident that the authors[13–15] were victims of 'unknown unknowns', and perhaps 'undue haste' – but this is understandable in the frantic circumstances of 1989. What is important is that these experiments be recognized for what they are, not what they are not. They are important members of the experimental database that teaches us under what conditions one encounters FPHE. They are not any part of a proof of nonexistence of the phenomenon and cannot be used to support such a conclusion; absence of evidence is not evidence of absence.

Notes

1. Here 'meaningfully' means some 50 orders of magnitude.
2. Initial attempts at calculation[1] also ignored electron screening effects, but even when applied correctly[2–4], the calculated rates were too low to account for the claims of Fleischmann and Pons, and objection 3 would still apply.
3. Such as the first law of thermodynamics or the equivalence of mass and energy – neither of which is violated obviously by the FPHE.
4. Or, perhaps more rationally and motivationally, 'to know where the truth might lead'?
5. Fleischmann and Pons were well aware of the significance of loading and the need to measure it, and they did so by means of the cathode overvoltage. Since it is now clear that the FPHE occurs at or near the cathode surface, this measurement is possibly more relevant than the average loading inferred from bulk resistivity measurements, but requires experienced interpretation.
6. Unknown to the SRI group, Fleischmann and Pons established a minimum observation time of 3 months before an experiment would be regarded as 'failed'. Having worked on observing the effect for more than 3 years before 1989, this clearly shows that they were aware of a long initiation time.

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