

The embarrassment of cold fusion

David Lindley

The variable and transient claims of experimental evidence for cold fusion made a moving target which attracted too much enthusiasm and too little derision.

THOSE readers who consider that this journal has published too much on 'cold fusion' should be grateful for what they have been spared.

In the year since Stanley Pons and Martin Fleischmann made their famous announcement at a press conference at the University of Utah, we have received a barely imaginable quantity of letters — most of them respectably typed, one or two handwritten entirely in capital letters — attesting to remarkable latent powers of creative thinking by scientists around the world. For only a few of these offerings was it thought necessary to consult expert reviewers; fewer still survived that scrutiny.

Of the fraction of papers submitted that have thus reached print, the account appearing this week (see page 401) is of special significance. Michael Salamon and his colleagues, like the teams from Yale University and Brookhaven Laboratory, from the California Institute of Technology, and from the UK Atomic Energy Authority Laboratory at Harwell, have searched for nuclear emissions (neutrons, gamma-rays, electrons and protons) from cold fusion cells, and found nothing. The difference is that Salamon works at the University of Utah, and the cold fusion cells he examined were in the laboratory of Pons and Fleischmann.

This is not quite the same as saying that what Salamon *et al.* looked at were Pons and Fleischmann's cold fusion cells. As the article makes clear, Pons will not concede that any of the electrolytic cells were, at the time they were being examined, in fact producing anomalous heat. There was a two-hour period when, according to Pons, a cell under examination was producing excess heat — but at that time the detectors and computers of Salamon and his colleagues were not working, having been put out of action by a power cut.

To this, Salamon *et al.* provide a clever response. Any neutrons emitted by the working cold fusion cell would have created some secondary radioactivity in the neutron detector, which should have been measurable once the equipment was switched on again after the power loss. But no such secondary activity was found, which allows Salamon *et al.* to conclude that no conventional fusion reactions could have produced the claimed excess heat.

The tidy summary of their paper is therefore that during a five-week period which came some time after the press

announcement of Pons and Fleischmann on 23 March last year, cold fusion cells which may or may not have been producing excess heat certainly produced no anomalous nuclear emissions.

In the light of the known history of cold fusion, one has to ask whether this news will change the terms of the debate.

During the past year, the original claims of Pons and Fleischmann have diminished; the experimental evidence has been subtracted from not added to. In their 'preliminary note' (*J. electroanal. Chem.* **261**, 301–308; 1989 and erratum **263**, 187–188; 1989) Pons and Fleischmann said that they had found, as well as excess heat, production of tritium in the cell, and the emission of neutrons, detected by characteristic secondary gamma-rays from the surrounding water-bath. But the tritium concentration in a working cell turned out to be only three and a half times greater than that in the original stock of heavy water. Critics were quick to point out that such an increase is consistent with a recognized difference in the electrolysis rate of deuterium and tritium on account of their different masses.

Later, Petrasso (see *Nature* **339**, 183; 1989) argued that the gamma-ray signals presented by Pons and Fleischmann as evidence of neutron emission had the wrong characteristics to be genuine detections, and were more likely to be instrumental artefacts.

This left only the claims of excess heat,

which were themselves challenged at the Baltimore meeting of the American Physical Society (see *Nature* **339**, 4; 1989).

Pons and Fleischmann must be given credit for declaring from the outset that the amount of excess heat they saw was too great, by orders of magnitude, to be consistent even with the levels of nuclear emission that they described in their preliminary note.

Supporters of heat production by cold fusion have always said that some new physical process, by which deuterons can fuse in secret without giving off tell-tale nuclear by-products, was indicated by their results.

Thus arises the schism that separates those who believe in cold fusion from those who put the whole episode down to wishful thinking. On one side, non-believers see the negative results of Salamon *et al.* as incontrovertible proof that nothing unusual is happening; on the other, believers take the same results as affirmation that cold fusion indeed demands new physics, which they knew already.

The two sides are separated by a matter of faith, not one of science.

Efforts to bridge this divide have been made all the more difficult by the unceasing refusal of Pons and Fleischmann to say clearly and fully what their experimental evidence for cold fusion is. In the wake of their preliminary note and its published errata (which were themselves an extended version of a preliminary list of errata handed out as a photocopied sheet with the 10 April 1989 copy of *Journal of Electroanalytical Chemistry*), there came rumours (subsequently and vehemently denied at the Los Angeles meeting of the Electrochemical Society on 8 May last year) of heat generation by cells containing ordinary water rather than heavy water, and of the production of ^4He in significant amounts. The discovery of ^4He in the gases coming from a cold fusion cell was briefly trumpeted as a demonstration that deuterons absorbed into the palladium electrode were indeed fusing, but by a reaction that produced ^4He and a gamma-ray rather than tritium and proton or ^3He and a neutron, as conventional nuclear physics would have it.

Inconveniently, however, materials scientists pointed out that if helium had been formed in the palladium electrode, it would have stayed there; the correct test would have been to look for helium in the palladium itself, not in the gases evolved

IMAGE
UNAVAILABLE
FOR COPYRIGHT
REASONS

Early optimism: Pons (left) and Fleischmann with large-scale model of their 'cold fusion' flask at their press conference. (AP)

by the cell. But because Pons and Fleischmann had given out the suggestion about helium production in their usual teasing and informal way, it could be retracted without much pain on their parts. During April and May last year, there were constant assurances that a detailed account of their experimental methods and results was in preparation, would be properly submitted to a scientific journal, and would appear during the summer, or in the autumn — or perhaps a little later.

But what was reprehensible a year ago has now become absurd.

Still there are whispers of a hundred-page manuscript, replete with facts and figures, which the world will soon see. Most of the world, sadly for Pons and Fleischmann, is unlikely to care, except perhaps out of historical curiosity and a desire that the tale be neatly ended.

The waxing and waning of these various pieces of ancillary evidence, not to mention the sporadic nature of the successes achieved even by expert cold fusion researchers, was a wonderful liberation to those who rummaged through their undergraduate physics textbooks in search of forgotten phenomena that could be adapted into theories of cold fusion. It was a boon too that solid-state physics and quantum mechanics conspire to offer such a variety of unexpected and counter-intuitive effects to the eager enquirer.

If the ideas submitted to this journal are a representative sample, theories of cold fusion generally have the same logical structure as the assertion: "If we had some ham, we could make a ham sandwich, assuming there's some bread handy". Starting from some bona fide physical effect, one argues that if it were many orders of magnitude more important than it actually is, cold fusion would be possible; left unspoken is the assumption that the effect in question is of some relevance in palladium saturated with hydrogen.

In the proliferation of theoretical proposals, there were surprisingly few basic ideas. A number of early suggestions referred to the Oppenheimer–Phillips effect: at very low energies, two deuterons in close proximity tend to orientate themselves so that their constituent neutrons approach each other and keep the protons apart, which enhances the fusion rate because it permits the neutrons to interact while minimizing the electrostatic repulsion of the protons.

Explanations of this sort illustrate a common feature of cold fusion theories. The discrepancy between the standard fusion rate and what was needed to generate the heat seen by Pons and Fleischmann amounts to some 60 orders of magnitude, and the Oppenheimer–Phillips enhancement is a matter of ten per cent or so; nevertheless the reader was invited to agree that things were moving in the right

direction, and accept that a bit of numerical fine-tuning would be needed to take care of the details.

A somewhat more sophisticated clutch of ideas emerged as people latched on to the idea of effective masses. Two deuterons bound together in a molecule have a tiny but calculable chance of fusing and if, as a thought-experiment, the mass of the orbiting electrons is increased, the molecule becomes more tightly bound and physically smaller, and the fusion rate rises. It was then noted that electrons in palladium can have effective masses several times greater than that of a free electron; if two deuterons could be bound together with one of these 'heavier' electrons, an interesting fusion rate would result.

But a little learning is a dangerous thing. Electrons in a metal obtain their effective masses because they are not bound to a single atomic site, but move in concert throughout the lattice: push against one, and you push against them all. But if one of these lattice electrons is detached from the system so it can bind two deuterons into a molecule, it is no longer part of the electron collective, and reverts to having its normal mass. In other words, the effective mass of an electron in a solid depends on what you are doing to it, and if you want it to bind an isolated molecule, it can no longer have a high effective mass.

The third broad category of cold fusion theories rested on more sophisticated uses of collective effects in solid-state physics.

The general drift was that deuterons, being particles of zero spin, could fall into a Bose-condensed state, in which their wavefunctions were all identical and periodic throughout the lattice. (This condensation can occur only if the temperature of the system is less than a few degrees above absolute zero but, as with the Oppenheimer–Phillips mechanism, it was the principle that mattered.) Once in this condensed state, one deuteron could interact with another at a different lattice site in the palladium, because all the identical periodic wavefunctions of all the deuterons would have their maxima at the same places.

These theories had the special attraction that they could easily be decorated with the jargon, at once forbidding and enticing, of solid-state physics: Bose-condensates, Bloch states, Wannier functions . . . like the Paris fashions, they outface mockery.

Nevertheless they were all wrong, and for another straightforward reason. The fusion rate for two deuterons is calculated from their two wavefunctions, multiplied by the nuclear interaction rate. The latter is a very short range force; only at separations of a few nuclear radii is the nuclear reaction rate significant. The only important contribution to the fusion rate, there-

fore, comes from the product of the wavefunctions when the deuterons are very close. But the wavefunctions in the Bose-condensed state are calculated explicitly by ignoring the nuclear interactions; they are valid everywhere except at close range. Where the assumptions that lead to Bose-condensation are correct, the fusion rate is negligible; where the fusion rate might be significant, the Bose-condensation model is wrong.

All cold fusion theories put forward so far can be demolished one way or another, but it takes some effort. Although cold fusion was, in terms of 'ordinary' physics, absurd, it was not obviously so; it contravened no fundamental laws of nature. This made it easy for advocates of cold fusion to insinuate that some arcane but genuine phenomenon of quantum-mechanical solid-state physics might provide a credible theoretical foundation, and likewise made it impossible for sceptical physicists to give any clear and general proof that cold fusion could not work; all they could do was rebut one daft idea after another, and even that required a good deal of patient explanation.

But it has to be said that one of the reasons that Pons and Fleischmann prospered early on was that few people were willing to stand up and say why they thought cold fusion was nonsense. (One or two physicists suggested in private that it was up to this journal to take on the task in its editorial pages). After the 23 March press announcement, the response of most experts consulted by science reporters was academically correct but journalistically weak. It's a very interesting idea, was the gist of the scientific community's opinion, and we really can't say what we think of the experiments until we've seen more details, or tried it for ourselves, or consulted our colleagues in the chemistry department. This measured scepticism, contrasted with the unhesitant declarations coming from Pons and Fleischmann, sounded like academic nose-holding, as if physicists knew they had been beaten but could not bring themselves to admit it.

Perhaps science has become too polite. Lord Kelvin dismissed the whole of geology because his calculations proved that the Sun could be no more than a few million years old; Ernest Rutherford is still remembered for his declaration that talk of practical atomic energy was "moonshine" — but the stature of neither man has been noticeably diminished by their errors, which were as magnificent as their achievements. Kelvin and Rutherford had a common-sense confidence in the robustness of their judgements which the critics of cold fusion conspicuously lacked. Would a measure of unrestrained mockery, even a little unqualified vituperation, have speeded cold fusion's demise? □

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