This file includes two papers that were published together in *New Scientist* magazine.

Close, F., *Cold Fusion I: The Discovery That Never Was*. New Scientist, 1991. **1752**: p. 46.

Bockris, J., *Cold fusion II: The Story Continues. New Scientist*, 1991. **1752**: p. 50.

They were introduced with the following paragraph:

WHATEVER HAPPENED TO COLD FUSION?

The cold fusion saga is one of the most extraordinary episodes in the history of scientific research. The sudden announcement of a potential source of virtually limitless energy turned its inventors into media stars, caused them to shortcircuit normal scientific procedure and wrap their research in a shroud of secrecy. Researchers have argued bitterly about the validity of experimental results, accusing each other of fraud and suppressing scientific evidence. It has proved to be a stern reminder that scientists are just as vulnerable to the human failings of greed, vanity and spite as anyone else.

Perhaps the most telling revelation has been the lack of mutual intellectual respect between scientists working in different disciplines. The chemists failed to consult nuclear physicists before making their claims, treating the nuclear evidence for fusion with extraordinary carelessness. Physicists adopted a dismissive, arrogant attitude to those some described as "mere chemists", without appraising the possible significance of the electrochemistry behind the reaction. The sneers that have accompanied claims and counterclaims in both camps are a poignant reminder of the fragmented specialism and tunnel vision that dogs much research today.

New Scientist presents two opposing views of what cold fusion was, or is, all about: one from a nuclear physicist, and well-known science writer, who insists it is time to bring down the curtain on a fake phenomenon; the other from an eminent chemist who does research on cold fusion and believes it is worth pursuing.

Cold fusion I: the discovery that never was

At last, the bubble of cold fusion has burst, leaving behind a sticky story of intrigue, false facts and wrong inferences

Frank Close

ON 23 MARCH 1989, two chemists working at the University of Utah, Stanley Pons and Martin Fleischmann, stunned the world with their claim to have harnessed nuclear fusion-the process that powers the Sun-in a test tube of water at room temperature. Pons and Fleischmann asserted that passing a current through heavy water-water containing deuterium, a heavy isotope of hydrogen – between palladium electrodes produced a huge amount of heat. They concluded that this excess energy could not have come from any electrochemical process but must have resulted from the fusion of deuterium nuclei.

Nuclear fusion holds the elusive promise of cheap, abundant and pollution-free energy. Sea water contains an effectively limitless supply of deuterium. So, coming within hours of the Exxon Valdez oil disaster, and with the nuclear catastrophe of Chernobyl still fresh in people's minds, the sudden possibility of "cold fusion" captured everyone's attention. Overnight, governments and scientists around the world redirected their research programmes in attempts to repeat and develop the extraordinary experiment for themselves. In a single week in April, anecdotal claims by small teams of one or two researchers of having achieved cold fusion flooded the media. This was despite the fact that major research laboratories, which had large teams of scientists with wide ranging expertise and far superior equipment, were seeing nothing and saying nothing.

Meanwhile, the state of Utah voted $5 million for research into cold fusion, established a National Cold Fusion Institute (NCFI) in Salt Lake City, and then in nationally televised proceedings, lobbied the US Congress for $25 million of federal funds. By this stage, enough suspicion about some of the claims had surfaced for Congress to reject the request. Today, most mainstream scientists have dismissed the episode and returned to their more regular research.

Yet there remain isolated groups of fervent believers who insist that cold fusion is a real phenomenon, that it is being developed in Japan and that we in the West are in danger of being left behind. Meanwhile a committee acting for the State of Utah has been questioning how the NCFI has used the $5 million it received.

The bizarre behaviour surrounding the cold fusion episode is more akin to that found in cloak-and-dagger novels than in scientific circles. Scientists have accused each other of pirating ideas. Pons's attorney has made threats against a research group in the physics department of the University of Utah whose results discredited the phenomenon. There have been persistent rumours that scientific data were altered. The University senate passed a vote of no confidence on the president of the University of Utah, particularly in connection with the NCFI. There were reports that Pons had put his house up for sale and left the country in November as the work at the NCFI was about to be evaluated. And people have slowly become aware that several claims coming out of Utah misrepresent reality.

Added to this, my own researches during the past 18 months show that some of the so-called evidence for fusion was not obtained and presented to the world in the accepted scientific way. To be concerned is not simply being pedantic. Society relies on science and makes substantial investment in it. The general public assumes that when researchers claim to have made a major discovery it has been thoroughly and carefully researched. Parts of the test-tube fusion episode failed sadly on this score.

I became involved in cold fusion in 1989 when I was distinguished scientist at Oak Ridge National Laboratory, which is operated by the US Department of Energy (DOE). The DOE had ordered its laboratories to make a concerted effort to discover if the claims were right, and had given us autumn 1989 as the deadline. The funding of billions of dollars of research into hot fusion was in the balance; if the claims of cold fusion turned out to be true, the DOE would have to reallocate funding.

Every Wednesday at noon, all the scientists and engineers involved in cold fusion experiments at Oak Ridge met resident experts in various fields of physics, chemistry and metallurgy to share news of progress. From these meetings a weekly report was prepared for the Secretary for Energy in Washington. I was consulted as a theoretical physicist. I felt that the claims of cold fusion ran contrary to the paradigms of physics, indeed, they were incredible. According to standard theory of nuclear physics, a hypothetical mass of cold deuterium the size of the Sun would yield only a few fusions per year, yet some of the claims from Utah required this rate to be many thousands per second in a beaker of water. This seemed so far-fetched that first I tried to find out how well the documented reports and claims matched what had really happened.

The news reports and the experimental paper that Pons and Fleischmann had published early on were not much help. The paper must have been written in haste because it contained several obvious errors. The most bizarre was that the name of a co-researcher, Marvin Hawkins (who it transpired had done much of the work but whose existence is still not widely known) had been omitted from the paper. The normal scientific procedure would now be to question Pons and Fleischmann in the hope of learning essential details. But trying to reach them by phone was like trying to get through to British Rail enquiries – the phone line was perpetually busy.

Instead, I first spoke to many scientists who were attempting experiments of their own (with little success). When I talked to people who had earlier gone on record as having replicated the phenomenon, I learnt that they had later discovered shortcomings in their experiments and had withdrawn their original claims. This was something that the media had not widely reported.

The most direct insights into what had really been done in Utah came when I carefully studied tapes, videos and transcripts of public presentations by Fleischmann and Pons. They had become such celebrities that every public utterance was recorded. Gradually, I became aware of a credibility gap. The tapes showed that Fleischmann and Pons were giving a standard lecture which said little more than was in their paper. Probing questions from the audiences, however, began to reveal deviations from normal scientific procedure and expose inconsistencies. An individual lecture did not always reveal these problems; it was when I compared the responses to questions asked at different lectures that discrepancies and even contradictions began to emerge. Regrettably, I realised that some of the data supporting the claims of cold fusion could not have been measured in the form that Fleischmann and Pons had presented in their paper: the data had been mysteriously altered. What had started as an attempt to understand the scientific basis of the cold fusion claims became instead an investigation into the true nature of the evidence, its presentation and the cause of the fiasco.

Nuclear fusion involves joining together atomic nuclei of light elements, particularly isotopes of hydrogen, deuterium and tritium. Potentially, it is a relatively clean source of energy compared with chemical sources, such as burning coal, and nuclear power. The technical problems of achieving fusion are many. This is because all nuclei are positively charged and so repel each other, making it difficult to force them together – a first step in achieving fusion. Attempts to overcome this and produce a viable energy source has already cost billions of dollars. Modern fusion research requires arrays of magnets as big as houses in order to contain the fuel – plasma – at temperatures 10 times hotter even than those in the centre of the Sun. The technology needed to make the plasma dense enough and stable enough to sustain fusion is formidable. Nuclear physicists and engineers have made a lot of progress in recent years but thermonuclear fusion as an economic energy source is still decades away and the research is becoming extremely expensive.

It is not surprising that researchers were both excited and taken aback when Pons and Fleischmann claimed to have found an easy way of fusing nuclei. Chemists are well aware that palladium soaks up deuterium like a sponge soaks up water. In a cell containing an electrolyte of heavy water and palladium electrodes, an electric current from a battery transports the deuterium ions, or nuclei, from the heavy water to the negative cathode and into the spaces between the palladium atoms. The two chemists believed that once the deuterium was crammed inside the palladium, the deuterium nuclei would get close enough to fuse and form new elements such as helium or tritium, neutrons and heat. If fusion were really taking place, then the chemists should have been able to detect the products and, from the amount of heat measured, determine the amount of fusion products produced. In the case of Pons and Fleischmann's experiment, the reported heat released indicated that a staggering thousand billion neutrons and tritium atoms per second should have been produced.

In fact, many scientists and government departments around the world were concerned about the potential strategic military implications of a test-tube-device capable of producing such copious amounts of neutrons and tritium. These are essential ingredients for preparing thermonuclear devices. Cold fusion could become available to any tinpot dictator with some heavy water. I learnt that this was what drove some of the keen interest in India, where there was a suspicion that the West would classify the work as secret.

Desperately seeking fusion

Fleischmann had the idea of test-tube fusion several years ago and made some exploratory measurements of the heat. The portrayal in the media of a concentrated five-year research effort into test-tube fusion which culminated in the announcement on March 1989 is far from the reality. Between 1985 and 1988, Pons wrote more than 100 papers, 30 jointly with Fleischmann, none of which had any bearing on the cold fusion research.

The work at the University of Utah moved into the front line only in 1988, becoming urgent towards the end of that year when they learned of a rival group Jed by Steve Jones at Brigham Young University (BYU), 50 miles away in Provo, Utah. Jones thought that the fusion of hydrogen isotopes under pressure could be the source of the Earth's heat. He designed a similar experiment using an electrochemical cell and heavy water. Being a nuclear physicist, however, Jones was looking for the neutrons as evidence of fusion, and so his approach was complementary to that of the heat-seeking chemists.

Jones saw, at most, only a few neutrons (and even today there is controversy as to whether his neutron signal is significant). What made him confident, however, was that he could measure not just the number of neutrons but also their energy. His small signal had the energy expected for neutrons coming from fusion though at an infinitesimal rate compared with that implied by the claims of Fleischmann and Pons. For Fleischmann and Pons, Jones's neutrons were the missing link, complementing their measurement of heat (even though the heat claimed did not correspond to the number of neutrons detected). Early in 1989, the chemists thought they had every reason to believe that they were on the right track. After a meeting with Jones on 23 February 1989, they learnt that he was preparing to publish his results, far sooner than they could publish theirs. It was from that moment that their problems began.

The possible commercial benefits of the "discovery" were already apparent to the University of Utah, as were the dangers. Although Jones's work was rather different from that of the chemists – because he was making measurements of neutrons not heat – there was concern that if Jones even mentioned heat, he could undermine the university's patent claims.

Fleischmann and Pons took urgent steps to measure neutrons in order to obtain the whole evidence for their case and go public without delay, even though their research was incomplete. They needed to detect neutrons coming from their own apparatus and to measure the energies of the neutrons. They hoped to confirm that the neutrons were, indeed, evidence for fusion and were not produced by background radioactivity or cosmic rays.

The chemists did not have a suitable neutron detector but Fleischmann knew that the Harwell Laboratory near Oxford did, so he thought of transporting the "fusion cell" to Britain. This proved to be impossible: you can imagine the bureaucratic problems associated with flying a "fusion" device around the globe. The next idea was to fly Harwell's detector out to Utah, but this too was out of the question because it weighed several tonnes. Thus in mid-March, Fleischmann was reduced to giving Harwell details of the procedures so that a Harwell team could perform the measurements themselves to try to confirm the phenomenon.

While this was going on, Fleischmann and Pons had come up with what they believed to be a way of detecting neutrons for themselves. Any neutrons produced in the fusion would shoot out of the cell and into a surrounding water bath. As the neutrons slowed down, they would be captured by protons in the water and emit gamma rays with a characteristic wave-length. These could be detected by crystals of sodium iodide. A radiologist at the university, Bob Hoffman, took the data and found what appeared to be a gamma-ray peak at an energy of about 2500 kiloelectronvolts. Neutrons produced by deuterium fusion have an energy similar to this. On 11 March, Pons submitted a "preliminary note" for publication in the *Journal of Electroanalytical Chemistry* entitled "Evidence for Fusion ... ". This paper gives the impression that the gamma-ray signal is a proof of the neutrons and an indicator of their energies.

At the time of the press conference on 23 March 1989, the scientific community had not seen any of the data on which the claims were based. The first display of these was five days later, following the Easter weekend, on Tuesday 28 March, when Fleischmann spoke to a select audience at Harwell. It was here that the first problems began to emerge.

A crucial piece of evidence for their claim for fusion rested on the gamma-ray signal at 2500 keV. The experts at Harwell, however, told Fleischmann that real gamma rays from neutron capture should have shown up near 2200 keV (actually 2224 keV), not at 2500 keV. The official report of the meeting noted that: "It was hard to see how the calibration [of the gamma-ray spectrum] could be so wrong." Fleischmann transmitted this information to Pons who was in Utah with all the details of the experiment. By 31 March, the axes had been redrawn, generating the peak on the graph as presented in the publicly circulated paper. The data are essentially the same as that measured at 2500 keV but the total number of events has mysteriously altered, the energy scale has been stretched out and the central value changed from 2500 to 2200 keV (see Figure 1). I am told that neither Hoffman nor Hawkins, who had made the original measurements, had done any further experimental work to recalibrate the position of the peak.

Indeed, when I and Richard Garwin – a member of the official panel investigating cold fusion-asked for information on the calibrations made to relocate the peak, we were unsuccessful. Even at 2200 keV, the peak is wrongly positioned as it should centre on 2224 keV, a fact that many researchers noticed and thought that the Utah team had misidentified the gamma radiation that occurs naturally at this energy.

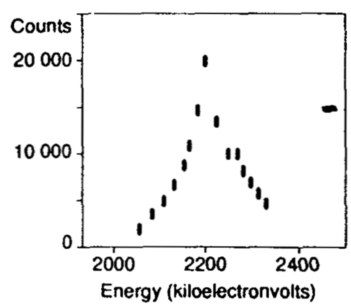

Figure 1 The case of the shifting data. In Fleischmann and Pons's paper, the gamma-ray emission peaked at more or less the right energy (2200 keV) to indicate fusion. Originally, the peak was located a further 300 keV up the scale but moved before final publication had made the original measurements, had done any further experimental work to recalibrate the position of the peak.

This is but the first of several idiosyncratic approaches to the gathering and analysis of data that makes it essential to look at the original logbooks if we are to evaluate what happened. A second set of dubious claims concerns the heat – whether or not control experiments were made with ordinary water in place of heavy water. If heat was due to the fusion of two deuterium nuclei in the heavy water, then there should be no such heat when ordinary water is used. At lectures, researchers repeatedly asked Fleischmann and Pons whether they had carried out a control experiment and with what results. The general impression is that they had not made this rudimentary test before going public (even though in a recent paper they claim that they did). Yet their own record seemingly denies this most recent claim and reveals how the psychology underlying belief can override observation and deduction.

At Harwell on 28 March and again at CERN, the European Laboratory for Particle Physics, on 31 March, Fleischmann intimated that no control experiments wit ordinary water had been done, stating that "these experiments are now in progress". Two weeks later at a special meeting in Erice, Sicily, he said that he was "not prepared to answer" and maintained this in the face of repeated requests from the audience. However, in Dallas that same day, 12 April, Pons was less reticent and admitted to the media that he had made an experiment with plain water and that it also produced heat.

The conclusion should have been that there had been a miscalibration or that the heat source was not deuterium fusion. The chemists' belief in the phenomenon was so strong, however, that they decided that the heat in plain water must be due to fusion between the copious protons and the trace of deuterium that occurs naturally (about I part in 6000). Indeed, Pons convinced Charles Martin of Texas A & M University to go public on 10 April with the first claims of replicating cold fusion, even though his research group found heat with plain water too and were concerned that this implied some error in the experiment. In fact, there was. A bad earth connection had caused electric current to enter the cell and heat the contents. Being unaware of this at the time, the team interpreted the heat as "excess".

So began a remarkable week, by the end of which the DOE were in some panic. The claim from Texas A & M was soon followed by claims by Georgia Institute of Technology of finding neutrons and by researchers at the University of Washington in Seattle of detecting tritium. This seemed to put all of the ingredients of fusion in place. But each result was subsequently withdrawn as the researchers found errors in their experiments. Nevertheless, some people are still claiming the results are confirmations.

Pons was feted as a hero in Dallas and the DOE called in one of its past chairmen, Glenn Seaborg, a Nobel Laureate for discoveries in nuclear chemistry, for advice. Seaborg was sceptical but advised that the DOE set up a special investigative panel. He also briefed President Bush on the subject. Seaborg contacted Pons who told him about the heat in ordinary water and his belief that it was due to fusion between protons and deuterium nuclei. But in addition, Pons claimed that the key to the heat and lack of radiation in the heavy water experiments was that the fusion produced helium-4 and that experiments to test this idea were currently under way in Utah.

On the Sunday evening, just two days later, Pons announced that the helium-4 had been found and in quantities commensurate with those expected given the amount of heat that he was claiming. The reaction at the DOE was immediate: the heads of nuclear and fusion programmes at all of its laboratories were ordered to drop whatever they were doing and come to Washington for an emergency meeting on the morning of Fleischmann (left) and Pons, with the test-tube reaction that Tuesday 18 April. Directors from the West Coast came by launched a thousand experiments overnight flight and, bleary eyed, met with their eastern colleagues to be asked, "Why are you doing nothing?"

It rapidly transpired that roughly five teams per institution were already doing experiments, working night and day with state-of-the-art equipment. It was too soon for them to go on the record and write scientifically reputable papers, but the trend was already clear-there was no sign of test-tube fusion. This did not mean that they saw nothing. In fact, they were seeing things, the same sort of things that others had seen and had called press conferences about. The difference was that the major laboratories contained a wide range of expertise, knew many of the pitfalls that the unwary could fall into and had already pointed out to some groups where they had gone wrong. Later, a research group at the California Institute of Technology showed that Pons's claim to have produced helium by fusion was impossible to support, given his apparatus, and that the helium that had been detected was almost certainly from the atmosphere.

In addition, researchers at Los Alamos reported that they were already negotiating with Utah to have access to a working cell in order to test it for radiation, but this collaboration was stillborn, the excuse from Utah being that patent attorneys were uneasy. Following this meeting, the DOE laboratories were ordered to redouble their efforts, report weekly – which is when I became involved. An official national panel of some two dozen experts was charged to examine the claims. By the end of the year the experts concluded that there was no evidence for fusion, had identified flaws in several "positive" experiments and advised against any special funding of the research.

The cost had been tens of millions of dollars in human effort and equipment, not to mention other research programmes that were curtailed in the interim. In Britain, the Harwell Laboratories, which had been advised early on by Fleischmann, performed what many regard as among the most complete and definitive range of experiments in the world. They found no evidence for fusion, nor even for heat. This negative result came from a team of experts spanning a wide range of physics, chemistry and materials science aided by first-rate equipment, far superior to that used by the groups who claimed positive results. Harwell's reputation and the received opinion among experts of the quality of their work effectively killed off the episode in Britain, though it is still being pursued in isolated pockets around the globe.

Supporters of cold fusion will cite anecdotal confirmations but they fail to mention the many groups who have reported negative results. The research groups for cold fusion tend to be small and tend to be relatively "amateur" compared with the full-time, large-scale teams of laboratory scientists who have, almost universally, seen nothing.

The fluidity of the evidence from Fleischmann and Pons made many nervous. Some supporters have responded by suggesting that there is some establishment conspiracy wishing to suppress these-dramatic discoveries. This we can surely dismiss. Scientists, especially theoretical physicists, are all awaiting the next revolutionary breakthrough that overthrows the paradigms. When a radical new result arrives, see how they all drop what they were doing and pursue the new promise. That is what happened when the news of cold fusion erupted. We crave new discoveries and the attendant excitement and promise, that is what drives many of us into science in the first place. The art of research is to judge when to pursue an outlandish notion and when to ignore it. As students, we read the textbooks, attend the lectures and are guided into research. But I have never forgotten the advice that my research supervisor gave me the first time that I was pursuing a dead end: "It is important to recognise when to quit." As far as cold fusion in a test-tube is concerned, we can safely say that it is time to move on to pursue other routes for harnessing fusion. □

Frank Close is senior principal scientist at the Rutherford Appleton Laboratory and distinguished scientist at Oak Ridge National Laboratory, Tennessee, US. His first-hand exposé of the race for cold fusion called Too Hot to Handle is published this week by W.H. Allen at £14.99.

Cold fusion II: the story continues

Many scientists think that fusion carried out in a test tube is nonsense. But a good look at recent results reveals that the reaction is real

John Bockris

IT IS extremely unusual for scientists to announce their discoveries at a televised press conference. In fact, it may never have been done before, but that was the path taken by Martin Fleischmann and Stanley Pons on 23 March, 1989. They claimed something so breathtaking that most scientists who heard the announcement, simply suspended disbelief until more was known.

The chemists, from the University of Utah, said that they had achieved nuclear fusion, not in a gigantic machine that aims to simulate conditions in the Sun, nor at the end of eight laser guns needing a four-storey building to hold them. It happened, they said, in a simple electrochemical cell, the size of a jam jar, costing about $100 and with instruments costing maybe $5000. They said that the experiment was so simple it could be done by any secondary school student.

What most scientists who heard the amazing claim of Fleischmann and Pons did not realise was that there was a valid scientific idea behind it. The chemists had been extremely surprised to observe that passing a current through heavy water (containing the hydrogen isotope deuterium) between palladium electrodes produced a burst of heat great enough to melt the electrodes. As a competent electrochemist, Fleischmann was sure that the energy produced could not have come from a chemical reaction. But it was possible to formulate a mechanism whereby the high electric field at the electrodes, combined with the rapacious appetite of palladium for taking in hydrogen and its isotopes, could bring about conditions for nuclear fusion. At high enough current densities, the deuterium nuclei would be forced into the palladium crystal lattice and could be compressed to as much as 10^{24} atmospheres. This could provide enough energy to overcome the repulsive electrical force between deuterium nuclei and allow fusion to take place.

Unfortunately, this idea can readily be appreciated only by those who have a detailed knowledge of electrode kinetics and surface chemistry.

Physicists were not familiar with these concepts, but they knew that they had been presented with an absurdly simple apparatus and a claim that fusion took place inside a metal in the cold. For them, there was no way to see any connection with reality. They had adopted their traditional approach to calculating the probability of nuclear fusion. This was to use equations that apply to simple collisions between deuterium ions, and assuming that the distance between deuterium nuclei in palladium was the same as that of dissolved deuterium in palladium at normal temperatures and pressure. Under these conditions, fusion seemed impossible.

Nevertheless, shortly after the announcement, Admiral James Watkins, the chairman of the Department of Energy, issued a stern order to all American national laboratories. They were to find out whether the claims were true. Within two weeks, hundreds of confused fusion physicists turned away from their hundred-million-dollar magnetic confinement systems and the four-storey high giant lasers, and set up the $100 jam-jar cells to do what Watkins had demanded – to repeat the chemists' experiments.

Only a few research groups, mainly chemists, could confirm the observations of Fleischmann and Pons. Most physicists failed. Looking for heat with supersensitive calorimeters, they obtained only doubtful deviations from zero. But more important, the physicists could not detect what they regarded as the confirmatory signature of nuclear fusion-the emission of neutrons.

A few weeks later, there was a massive reaction against the two infuriating Utah scientists. Publicly, several physicists railed against what they called the incredible stupidity and manifest ignorance of two "unknown chemists" in not making accurate measurements of neutrons and gamma rays. Privately, the inventors were painted as fraudulent hucksters out to pull in risk capital before their absurd claims were blown to pieces.

There was also confusion in news reports as to who Fleischmann and Pons were. At this time (April to July of 1989) articles appeared in the New York Times which told of two "unknown chemists", one British, one American. This amused those in the large field of electrochemistry where Fleischmann's name counted as one of the foremost in the world, with the much younger Pons often described as "the smartest younger one around". Many defamatory statements made by younger American physicists and chemists – particularly from prestigious universities on the West Coast – are best ignored.

The reason why most researchers who first tried the cold fusion experiment failed was that they were not aware of the essential conditions for recording the anomalous results. We now have a much better understanding of what these conditions are. First, you need to carry out the electrolysis at high current densities (1 amp per square centimetre). Secondly, you have to be patient. It may take at least four weeks, and sometimes as long as 12 weeks before anything happens. Then, one may see bursts of heat or neutrons, or detect tritium-the radioactive isotope of hydrogen, which is one of the products of fusion. It is not surprising that little of the early work revealed the effects claimed by Fleischmann and Pons.

Physicists did not reject cold fusion only because they could not make it work straightaway. There is another less attractive reason: the chemists had undermined the fusion establishment, which had already spent billions of dollars on research. In certain American government laboratories, there was a campaign of suppression reminiscent of religious attitudes and conflicts in the 16th century. One research group described how they had boarded a plane to go to a national meeting and present some positive results when they were told by their boss to cancel their presentation.

In another institution, a visiting government commission came to investigate claims of tritium found in a cold fusion experiment. The commission's man, an expert on detecting tritium, said he did not believe any story of tritium being produced during electrolysis. It would have to be due to contamination. The researchers brought out their laboratory records, which showed results taken daily over several weeks. Tritium levels remained at zero most of the time but then there was a sudden increase, which we now know is typical. There it was in the notebook – a graph showing tritium levels that increased to 100 times the background value in a few hours.

The visitor was so unnerved that he pushed the notebook away and said he refused to look at such nonsense. Visibly angry, he got up and left the room.

There has also been a campaign of attacks launched by the two main research journals, *Nature* in Britain and Science in the US. *Nature* has published leaders asking other scientists to ridicule work on cold fusion. *Science* has done more. It accepted a news article by a journalist who claimed that some of the reports of tritium being detected were, indeed, fraudulent and that a graduate student made these claims to try to hasten the award of his PhD degree. The article, which is full of innuendo, alleges that the reported appearance of tritium correlated only too well with the visits of sponsors, and so on.

There is no doubt that the cold fusion experiment of Fleischmann and Pons has been difficult to reproduce. In one of our laboratories at Texas A & M University, we carried out 58 experiments to measure tritium, using several cells running for many weeks. Only 15 yielded tritium at a level above what would normally be detected when electrolysing heavy water. We carried out corresponding experiments to measure heat produced but in fewer cells, about 25. We found excess heat in only five cells.

The positive side

Fleischmann and Pons claim much better reproducibility. But until they divulge their method and results, kept secret so as not to invalidate patent applications. and tell the rest of us how to do it, it will remain difficult to counter the widespread rejection, which is often given to phenomena that cannot be reproduced at will by independent colleagues.

Nevertheless, work has continued in many countries. There have been reports from about 100 labs in 12 countries confirming at least one of the phenomena associated with cold fusion-heat, tritium, gamma rays or helium. Fritz Will, director at the National Cold Fusion Institute in Utah, has prepared a list of all the reports. But an increasing number have come from refereed journals. The list includes five of America's laboratories, but omits most of the Japanese research groups who have been very active on the cold fusion front.

When physicists suspect a nuclear reaction is taking place, they usually look first for neutrons. In the case of the cold fusion experiments, the numbers of neutrons measured are very low-about 1 to 10 neutrons per second per square centimetre, above the natural background value. These neutrons are produced occur in bursts lasting about an hour. At first, it seemed reasonable to suspect that the bursts could be due to a fault on the neutron counter or to sudden solar flare activity which can cause bursts of neutrons in the atmosphere.

But these reservations have been laid decisively to rest. Antonio Bertin at the University of Bologna in Italy, and more recently Kevin Wolf at Texas A & M, have taken their apparatus and counters deep underground where there is sufficient shielding (in a lead mine, for example) to reduce the background signal considerably. The .researchers still see the same bursts of neutrons. What is more, parallel experiments with ordinary water do not produce any neutrons.

Franco Scaramuzzi's group at Frascati in Italy, and other groups, have carried out complementary experiments underground by cooling palladium or titanium electrodes to about -100 °C. At this temperature, much more deuterium dissolves in the metal than at room temperature. The temperature is then raised so that the metal becomes supersaturated with the gas. In some of this kind of experiment, extraordinary phenomena show up – there are "mega-bursts" of neutrons, about a million times larger than the usual bursts. Recently, Eiichi Nishioka and Takahashi Yamaguchi at the Nippon Telegraph Company have seen such bursts. No one obtains a steady emission of neutrons. The bursts continue, but with decreased activity.

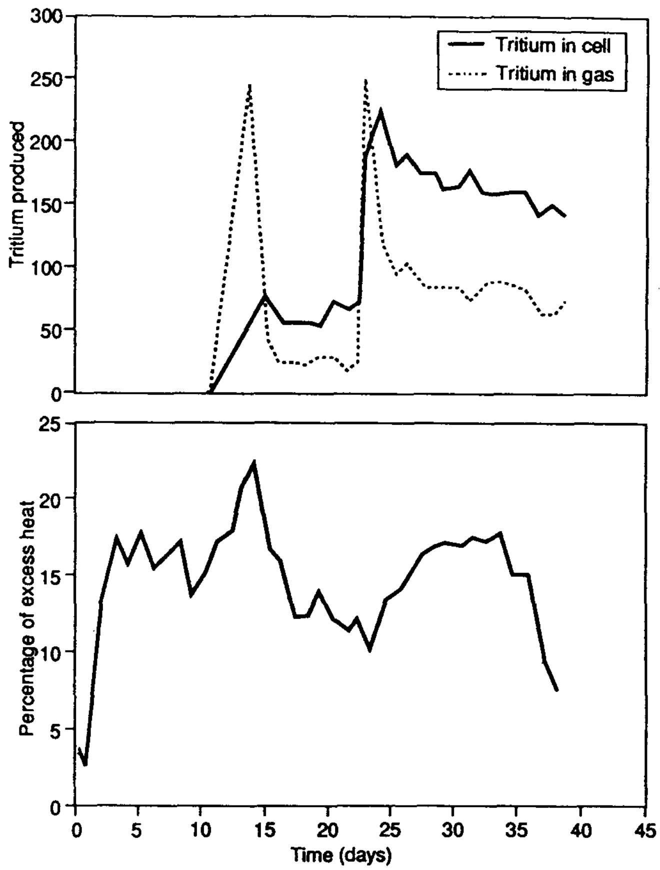

Figure 1 shows the simultaneous production of tritium and heat in a cold fusion experiment at Texas A&M

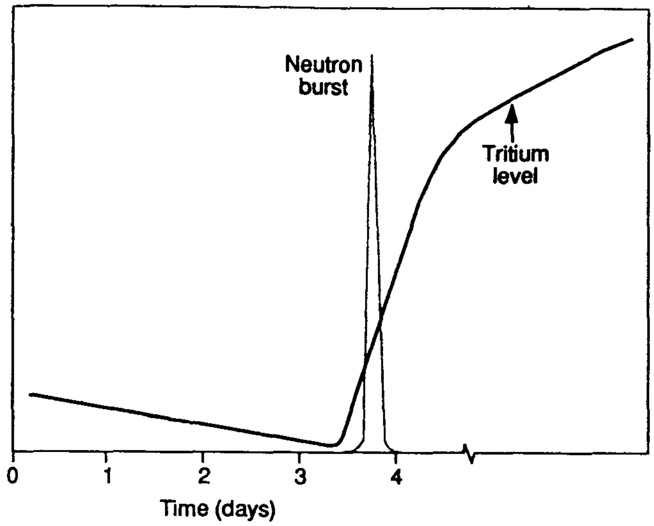

Figure 2 shows the formation of tritium accompanied by a burst of neutrons observed at Bhabha Atomic Research Centre (the tritium yield is on a logarithmic scale)

When it comes to measuring levels of tritium, most physicists blame the contamination, such as from other laboratories through ventilation systems. Some of the scientists who have found excess tritium in their experiments do, however, work in laboratories where measuring tritium levels is routine. This applies particularly to the gigantic effort made at the Bhabha Atomic Research Centre in Bombay, India, where 11 separate groups worked for about nine months on cold fusion. Eight groups reported finding copious amounts of tritium after about one week when palladium was electrolysed in heavy water. Tritium has been a particular subject of study there for 25 years so it is difficult to believe that gross errors would be made by so many groups.

In the same way, groups at Los Alamos National Laboratory in the US, led by Edward Storms and Thomas Clayton respectively, have reported finding tritium after passing a current through palladium which is in contact with deuterium. Researchers at Los Alamos are experienced in working with tritium so surely the problem of contamination is minimal.

One of the more impressive things about several of the tritium observations is that they can be correlated in time with other nuclear events. At Texas A&M, we observed tritium and excess heat levels suddenly rise simultaneously and then die down (see Figure 1). Daniello Gozzi at the University of Rome observed, early on, a huge burst of heat accompanied by the formation of tritium. At Bhabha Atomic Research Centre, the production of tritium has been frequently associated with neutron bursts (see Figure 2).

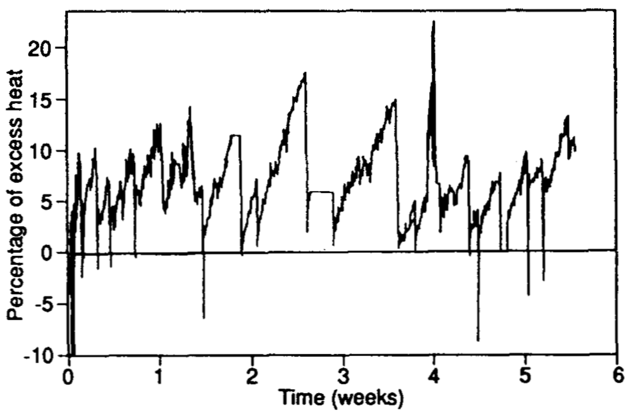

Figure 3 shows the typical bursts of excess heat we have seen in experiments at Texas A&M

There are, of course, many – perhaps hundreds – of laboratories where the experiments were tried and "nothing happened". At Texas A & M too, most of our attempts to find tritium have failed. But it would be foolish to dismiss the positive results, especially as their characteristics – the periods between bursts, the way the bursts die away and quantities of tritium found – are comparable in different laboratories.

Of all the results from Fleischmann and Pons, the ones most disputed are certainly the heat measurements. It is difficult to understand why this is so because it is relatively easy, with calorimeters similar to those used by Fleischmann and Pons, to measure the excess heat. Electrochemical theory tells us what amount should normally be given out by a cell undergoing electrolysis. This heat can be measured to an accuracy of about 1 per cent.

Figure 3 shows some results that we obtained. The burst of heat shown lasted for 32 days, and was an average of about 18 per cent higher than would be expected from a classical electrochemical reaction. On the basis of these results, I conclude that if you set up 20 cells, you will have a 90 per cent chance of seeing a burst of excess heat within three months.

There seem to be two types of burst. About 40 laboratories have reported bursts of 10 to 30 per cent excess heat. They sometimes correlate impressively with bursts of nuclear phenomena. Fleischmann and Pons originally showed. that, at a certain current density (0.1 amps per square centimetre), the excess heat turns on and increases with the current density.

Cold fusion 1990 – the plus and the minus

The plus

1. About 100 laboratories have reported anomalous effects similar to those claimed in March 1989 by Fleischmann and Pons.
2. Many of those who have tried and failed to replicate the phenomena have not accounted for the fact that prolonged electrolysis is necessary, and that even then the phenomena are burst-like.
3. The Japanese have made cold fusion one of their national priorities.
4. The theoretical side is not so dark as it seems: some theorists believe they can see a mechanism for cold fusion and one of them is Julian Schwinger, Nobel Laureate.
5. The famous irreproducibility is not universal. A few examples of reproducible heat, neutron and tritium measurements are now available.
6. In accounting for the lack of balance between the nuclear particles and the heat, we must remember that it is difficult to find some of the particles. For example, if the phenomena occur at or near the surface, most of the helium-4 will escape with the deuterium and then the analysis of, say, 10^4 helium atoms per second mixed with 10^{19} deuterium molecules will be a considerable challenge. But six laboratories have found helium inside electrodes.

The minus

1. More than 100 laboratories have failed to reproduce the phenomena.
2. Most nuclear physicists (using the theory of dilute high temperature plasmas) see the reported phenomena as "impossible".
3. The nuclear particles which are reported in bursts do not tie up numerically with the heat output (discrepancy 100 to 1000 times).
4. Until the anomalous effects can be tuned in at will using instructions that can be followed by other researchers, cold fusion is not part of established science.

The evidence for the second kind of burst – an occasional mega-burst of heat – is far less solid. Fleischmann and Pons have claimed to see bursts 200 to 300 per cent more than would be expected from the normal electrolysis of heavy water. But recently, Bruce Liebert and B.Y. Liaw from the University of Hawaii have made a much more remarkable claim. They carried out experiments employing a palladium anode in a molten salt containing lithium deuteride at about 400 °C. Under these conditions, deuterium is evolved at the anode rather than the cathode. The researchers obtained between 600 and 1500 per cent excess heat.

Researchers will find it difficult to accept this claim until other laboratories reproduce it, but it is interesting to note that the electrodes used have been analysed for helium-4 (the heavier isotope of helium, and a possible product of an exotic kind of nuclear reaction). The electrodes contained between 6 and 20 times background levels. If these mega-bursts were to be confirmed, it would change the outlook for cold fusion, and a practical fusion heat generator would become a possibility. Theoretical physicists would also have to take another look at the phenomenon.

Some countries have taken cold fusion more seriously than has the US and Britain. In particular, the attitude in Japan is utterly different from that so publicly shown in America. Instead of attempting to stamp it out like a dreadful contagion, the Japanese have put cold fusion on the list of national research priorities and devote 2 per cent of the hot fusion budget to cold fusion research. This implies an investment of about 25 million dollars a year for government funded groups (about 250 researchers). There is also cold fusion research going on in Japanese companies, although it is difficult to estimate how much. Because of the more relaxed, Jess hostile attitude in Japan, it may be that the decisive work on reproducibility, for example, will be done there.

Reproducibility is the key to scientific respectability. Scientists do not accept a phenomenon unless they can demonstrate it at will. It is good to be able to report, then, that people are beginning to claim that they can reproduce results in cold fusion. Wolf in our laboratories can obtain the emission of neutrons from a bank of electrodes more or less on call. Thomas Claytor, who works at Los Alamos, can make tritium reproducibly from a 10-layer sandwich of palladium, saturated with deuterium, and alternating with layers of silicon. At Stanford Research Institute, Michael McKubre and his colleagues have found two electrodes from which they can produce excess heat in their cells at will. Glen Schoessow at the University of Florida claims he also can do it at will.

It seems likely that reproducibility will spread during the coming year, given suitable funding. The detailed conditions particularly the ratio of deuterium to palladium, under which the experiments can be made to work may become clearer.

If the work of Fleischmann and Pons is confirmed, then theoreticians will have to modify their ideas about fusion physics. Who can say where this change would lead? Let me remind you of Ernest Rutherford's researches. When he first produced neutrons with James Chadwick in 1919, he told colleagues that he wasn't sure that there was any value in the result and that it was just an academic curiosity. His results led, of course, to nuclear energy.

There is, therefore, a good precedent for pursuing the ideas of Fleischmann and Pons. There is already enough evidence (see Box) to dismiss the widely held view that the original claims had no value. It seems now established that nuclear particles are, under some circumstances, produced in bursts at electrodes in the cold. As to the heat, there is no proof that it originates in a nuclear process, though when it coincides with nuclear emissions, it is difficult to think that it doesn't. No matter what the science journals and what many physicists say, "something is going on".

John Bockris is a distinguished professor of chemistry at Texas A & M University.