Fusion Followup: Confusion Abounds

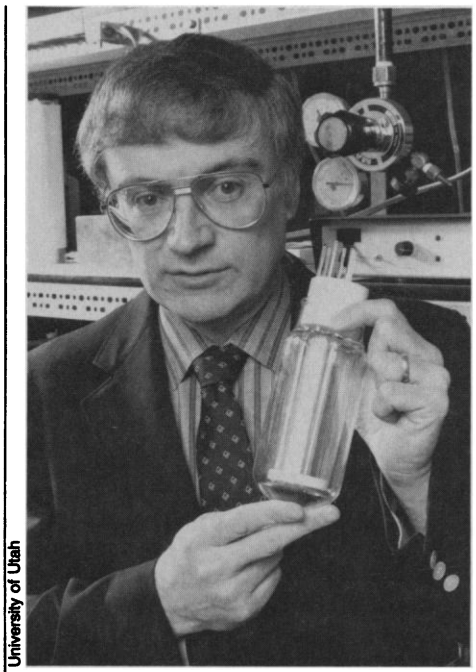
As researchers raced to reproduce a potentially revolutionary fusion result, a second group announced it too had seen fusion in metals in a series of independent experiments

A WEEK AFTER THE ANNOUNCEMENT of what appears to be a previously unsuspected form of fusion, scientists at laboratories around the world were scrambling to reproduce the results, aware that the new fusion process—if verified—could easily be the most important discovery of the decade. At the same time, rumors and innuendos were flying about the relationship between the researchers at the University of Utah who made the announcement and a team at nearby Brigham Young University that was working on a similar experiment.

On 23 March, Stanley Pons of the University of Utah and Martin Fleischmann of the University of Southampton, England, announced that they had created a sustained, room-temperature fusion reaction. Working alone and funding the experiment themselves because they thought it was too far fetched to attract a grant, the two electrochemists claimed to have done what hundreds of other scientists and hundreds of millions of dollars had not: They had found a way to keep a fusion reaction going in a laboratory, turning out more energy than it absorbed. Even better, they claimed they could do it at room temperature in glass beakers instead of at millions of degrees in million-dollar fusion reactors. If true, not only would Pons and Fleischmann be shoo-ins for a Nobel Prize, but their discovery could lead to a convenient, virtually limitless source of energy.

The first reaction by most fusion scientists was disbelief, followed by a dash for their laboratories. Because the fusion procedure outlined by Pons and Fleischmann is so simple, it can be repeated in almost any lab. Two electrodes, one palladium and the other platinum, are inserted into a beaker of heavy water—water in which hydrogen is replaced by the heavier isotope deuterium—and the electrodes are hooked up to a battery. The resulting current separates the water into oxygen and deuterium, and the deuterium is absorbed into the palladium electrode. After enough deuterium has suffused through the metal, the electrode starts to give off heat. In one cell, the researchers reported they measured a heat output 4.5 times as great as the electrical input.

The first attempts at verification did not work, but they had been done with only the information available from the press briefing on 23 March, when the researchers announced their findings. Fleischmann and Pons quickly started giving more details to researchers in other labs. Fleischmann, for instance, spent most of 28 March at Harwell Laboratory in England advising them on setting up the experiment. Spokesman Nick Hance said the lab had about ten fusion cells set up. The outcome from Harwell will not be publicly available for 2 or 3 months, Hance said, because the lab will go through the normal peer-review procedure for publishing its results.



University of Utah

Stanley Pons *of the University of Utah displays a cold fusion cell.*

American scientists are not likely to wait so long, however. Pons spoke with researchers at many laboratories throughout the week, passing out details of the experimental setup. He also began making preprints available of a paper detailing the results. One item that had been overlooked in the initial tests is that the palladium takes a long time to absorb enough deuterium that the fusion becomes measurable. If one uses small electrodes, Pons said, there is too little fusion taking place to measure. But if larger electrodes are used, it takes longer to charge them with deuterium. It takes 2 to 3 weeks, for example, to charge rods that are 4 millimeters in diameter, which is the size that has given the best results.

While researchers were trying to reproduce the results, they were also searching for a theoretical explanation of cold fusion in metals. A number of scientists have suggested that putting the deuterium ions into palladium reduces the ions' mutual repulsion, which is the basic obstacle to fusion. That still leaves the question of why measurements of the fusion cells detect many fewer neutrons than would be expected from the amount of heat they produce.

John Bockris of the Texas A&M chemistry department, who has spoken extensively with Pons and Fleischmann about the experiment, told *Science* he has a possible explanation for the lack of neutrons. Two deuteriums fuse to form either a helium-3 atom and a neutron, or a tritium atom (the isotope of hydrogen with one proton and two neutrons) and a proton. Normally, roughly half the fusions will take one branch and half the other. But in the interior of the palladium, Bockris suggests, the reaction that forms helium-3 may be much less likely than the one producing tritium. A large number of fusion reactions will take place—on the order of 10^{13} per second—but only a few of them will produce neutrons—about 10^4, Bockris said.

Many of the early attempts to reproduce the results concentrated on detecting neutrons, and researchers could detect few of the radioactive particles. Pons said, however, that his own results do not find a great number of neutrons coming from the fusion cells even when they are generating a measurable amount of heat. If Pons and Bockris are correct and the cold fusion reaction is one that turns out very few neutrons, then negative results from neutron counters are no indication of failure.

As the efforts to verify and explain the University of Utah results were going on, there was a great deal of confusion about the relationship between Pons and Fleischmann and a group of researchers at Brigham Young University headed by Steven Jones, who announced they had also seen evidence of cold fusion in metals. Rumors of attempts by one group to steal credit from the other were flying at both schools, which have a history of spirited and sometimes acrimonious rivalry.

The facts of the case, as told by the principals, are these:

Pons and Fleischmann began looking for cold fusion in metals after each had found unusual results in various of his own experiments, Fleischmann in the late 1960s and Pons a decade later. The two friends had discussed the findings, once on a drive through Texas and again as they hiked up Millcreek Canyon outside Salt Lake City. Deciding that something must be going on, they mapped out a strategy to test their suspicions. They funded the work themselves, eventually putting up about $100,000.

Jones and company were also looking for cold fusion in metals, but for totally different reasons. For many years, Jones had worked with muon-catalyzed fusion, in which muons—short-lived atomic particles similar to electrons but 200 times more massive—are used to bring hydrogen ions close enough together that they can fuse. The idea of dissolving deuterium in metals to overcome their mutual repulsion and bring them close enough to fuse was an extension of this work, Jones said. The group was also influenced, he said, by the work of Russian physicist B. A. Mamyrin, who in 1978 saw anomalous concentrations of helium-3 in various metals. Jones, whose work was supported by the Department of Energy, began to investigate fusion in metal in 1986, according to lab notebooks.

His team first saw signs of cold fusion in metals in 1986, Jones said, but his initial thought was that the neutron counter might be giving spurious data, so the group spent much time modifying and checking it. By the fall of 1988, Jones was sure enough of his data that he was ready to start putting together a paper.

Soon afterwards Jones discovered Pons and Fleischmann. Jones served as a reviewer for a grant proposal that the other two submitted to the Department of Energy, and he decided to approach them with an offer to cooperate. He wrote in his report to the DOE that the two teams had complementary techniques—Brigham Young had a state-of-the-art neutron spectrometer, while Utah was using calorimetric measurements—and he said he thought a collaboration was natural.

Pons said he was never interested in a collaboration because he did not believe the other group had much to offer. "We never needed his [Jones's] spectrometer, we never wanted his spectrometer," Pons said. On the other hand, Jones said that Pons did seem interested. "He asked for information on the spectrometer. We had quite a bit written up, and we sent it to them."

Pons also said that Jones initially argued with the proposal, pointing to problems in his and Fleischmann's theoretical calculations, but later became convinced by the proposal and only then offered to cooperate. Jones agreed that he had some problems with the grant proposal—"I have continuing reservations about the theoretical underpinnings"—but said that he never had questions about its merit. "I never disapproved of the proposal at any point." (Pons' application for a $322,000 grant from DOE was approved 2 March, although final funding authority has not yet been issued.)

At any rate, the two groups had continuing contact, and on 23 February Pons and Fleischmann came to Brigham Young to talk with Jones's group. At lunch, Jones said, he showed them his data and indicated that the team was preparing to publish an article. Pons and Fleischmann told him they would prefer to do their research quietly for another 18 months or so, Jones said, and the two asked him if he would put off reporting his results. Jones told them that he had already submitted an abstract for an invited paper on his work to be given at an American Physical Society meeting on 4 May, and he could not renege on that talk. He did cancel a planned colloquium 2 days later, however, and a graduate student of his canceled a talk at a research conference.

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During the same meeting, the three scientists also discussed submitting their results jointly, Jones said, but they reached no final agreement. On 6 March, Pons, Fleischmann, Jones, and the presidents of the two universities met at Brigham Young, where it was agreed that the teams would submit their papers jointly to *Nature* on 24 March.

Jones and Pons disagree on exactly what conditions were placed on the agreement, in particular concerning the question of publicity before the papers were submitted. "Our understanding was that we would not go public before the submission of the papers," Jones said. Pons differs: "There was never any agreement not to publicize."

Judging from the papers made available by both teams, although they were working with similar systems, their results were very different. (The preprint from the University of Utah is of a paper submitted to the *Journal of Electroanalytical Chemistry* on 11 March; the Brigham Young paper went to *Nature* on 23 March. The paper that Pons was planning to submit with Jones went to *Nature* on 24 March, but that manuscript has not yet been made available.) The main difference between the papers lies in the magnitude of the reported effects and the way in which they were measured. The Brigham Young group got about a dozen neutrons an hour from their cells—a rate so slow as to be useless for generating power. The Utah team measured heat instead of neutrons and reported an energy generation of about 26 watts per cubic centimeter of electrode—a fusion rate a billion times higher than the Jones team calculated. (This calculation was based, however, on the assumption that neutrons would be produced in about half of the fusion reactions which, if Bockris is correct, may be a fatally flawed assumption.)

One cell in the Utah experiment generated so much heat, that "a substantial portion of the cathode fused (melting point 1554°C), part of it vaporized, and the cell and contents and a part of the fume cupboard housing the experiment were destroyed." Certainly the Brigham Young team saw nothing like this.

The two papers also differ greatly in their implications. The amount of heat observed by the Utah team has obvious possible applications to fusion power, and although the preprint does not make the dramatic claims about fusion power that were made at the press conference, it is clear that fusion power is a main consideration in the work. The Brigham Young group, on the other hand, devotes much of its paper to geophysical considerations, where cold fusion may explain thermal effects in the earth as well as the distribution of helium-3 and tritium. The paper notes that the fusion rates observed so far are too small for generating power, but ends hopefully by saying that "the discovery of cold nuclear fusion in condensed matter opens the new possibility at least of a new path to fusion energy."

After the 6 March meeting, the two groups proceeded to write their papers and get ready for the agreed upon submission. At the University of Utah, however, worries began to appear that news of the breakthrough could not be kept quiet much longer. James Brophy, vice president for research, said too much was circulating about the experiment—rumors, leaks, questions, and false information. Although the university had filed for a patent on the cold-fusion process, there were also some concerns about patent rights if other groups learned of the results and reproduced them.

On approximately 21 March, the University of Utah decided to announce the fusion results in a press conference 2 days later. The university administration "agonized" over the decision, Brophy said, but they could wait no longer. "We decided to stop at the point where they could demonstrate fusion without explaining it," Brophy said.

Jones was not informed of the decision, although he spoke with Pons over the telephone 2 days before the planned news conference. When Jones heard Pons say at the press conference that his team had already submitted a paper to a journal, he decided that Pons had broken their agreement, and he submitted his paper to *Nature*.

At the University of Utah, the fact that Jones contacted Pons after seeing his grant application has generated widespread rumors and innuendos that Jones was attempting to steal some of the credit for Pons and Fleischmann's work. In addition to the uneasy relationship between the two schools, some of those suspicions may have come from Pons himself, who has hinted, a couple of observers said, that Jones had stolen ideas from his grant application.

But nonpartisan observers who are familiar with the situation say Jones almost certainly came up with the ideas for his work independently of and prior to seeing the University of Utah grant proposal. For his own part, Jones said he can prove the work in his paper came completely out of his own lab. "Our log books prove we have been studying this since 1986." In fact, Jones said, he had a page from his notebook dated 7 April 1986 notarized. That notarized page contains an outline of experiments his team planned to run, including explicit reference to looking for cold fusion in palladium electrodes, he said. A drawing done in May 1986 of a fusion cell looks very similar to what Pons and Fleischmann eventually used, he said, although that is not too surprising because "there are only so many ways [to design it] once you get the idea of doing electrolysis." None of his team's work was done because he saw Pons and Fleischmann's grant application. "I've stuck to my reviewer's agreement," he said. "We had our program outline and we've followed it."

Meanwhile, Utah governor Norm Bangerter has announced he will call a special session of the state legislature to provide $5 million for a fusion center at the University of Utah, and former NASA head James Fletcher has accepted the position of director. If the discovery pans out, said Bockris of Texas A&M, "the University of Utah will be the richest university in the country in 5 years." ■ Robert Pool

Telescope Collapse Unraveled

The fracture of a single highly stressed steel plate has been identified as the most likely cause of the spectacular collapse of the 300-foot radio telescope at the National Radio Astronomy Observatory at Green Bank, West Virginia, last November. An independent panel appointed by the National Science Foundation, which funds the facility, and Associated Universities, Inc., which manages it, reached that conclusion after examining the suspect plate and performing a computerized stress analysis.

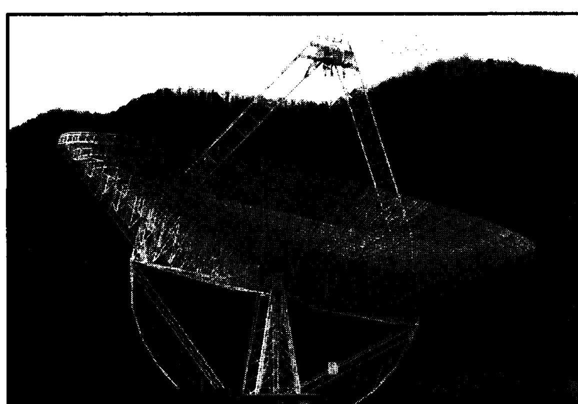
The panel found that parts of the telescope were under far higher stresses than would be permitted today, and that "from the beginning of its life, the structure was marginal with respect to structural failures of a minor or perhaps major nature." The plate that failed was a critical connection in the support structure of the instrument and it was subjected to high stresses when the telescope was moving. Half of the plate was recovered from the wreckage and a metallurgical analysis indicated that small cracks had been developing in it before it suddenly failed. The telescope was being swiveled when it collapsed around 10 p.m. on 15 November.

The panel absolves the managers of the facility from blame. It says there is no indication that the telescope was inadequately maintained—the plate itself was hidden from view and could not have been examined without disassembling the telescope—nor was it being operated inappropriately. The panel also notes that computerized stress analysis would identify potential failure points in telescopes built today, but these methods were not available when the instrument was built in 1962.

Now that the apparent cause of the collapse has been identified, attention is likely to focus on NSF's plans for replacing the instrument. In testimony before the House Appropriations Committee last month, NSF director Erich Bloch said that the foundation's top priority for its next major astronomy facility is an observatory to search for gravity waves. Known as the Laser Interferometer Gravity Wave Observatory, or LIGO, it would consist of a pair of facilities situated near the East and West coasts. LIGO has been in the planning and R&D stage for several years, and NSF was hoping to include funds in its 1991 budget to begin construction. The total cost would be about $100 million.

West Virginia Senators Robert Byrd (D) and Jay Rockefeller (D) have other ideas, however. In a statement released last month, they said that replacing the collapsed telescope with a modern instrument should have higher priority than LIGO. A replacement telescope, which would cost about $75 million, would be "the best promise for jobs, education, tourism, and scientific prestige," for their state, they said. Byrd, who is chairman of the Senate Appropriations Committee, said he "will aggressively pursue funding" for the telescope. Rockefeller is on the Senate Commerce committee, which oversees NSF.

■ Colin Norman



Before and after. *The cause of the collapse has been determined, but whether the instrument will be replaced may become a political question.*