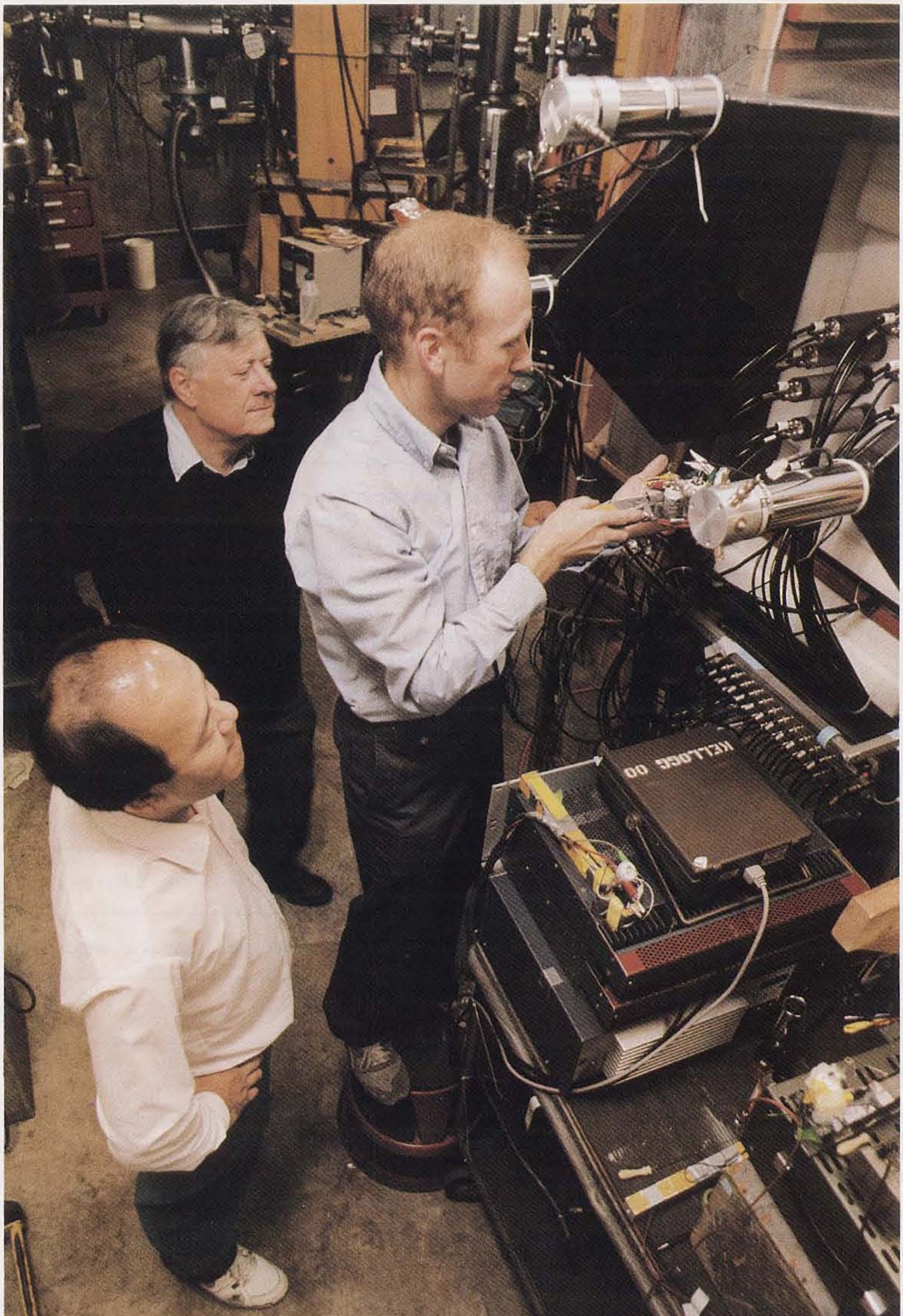

KELLOGG 00

Quest for Fusion

"I just wanted to be able to tell my grandkids that the day after cold fusion was discovered I went and did it in my lab."

Nuclear astrophysics takes a back seat to cold fusion. Kellogg inserts a fusion cell array into the neutron detector as (from left) Wang and Barnes look on.

Unless you've been adrift in a dinghy for the past four months, you've doubtless heard something about Drs. Fleischmann and Pons and their remarkable claim to have produced nuclear fusion at room temperature. The announcement, at a press conference at the University of Utah on March 23, sent scientists worldwide scurrying to try to duplicate the phenomenon. Caltech's effort began as a lark by a couple of postdocs in an electrochemistry lab and quickly grew to a multidisciplinary effort that monopolized a nuclear astrophysics research facility for months and at its peak involved some 20 people.

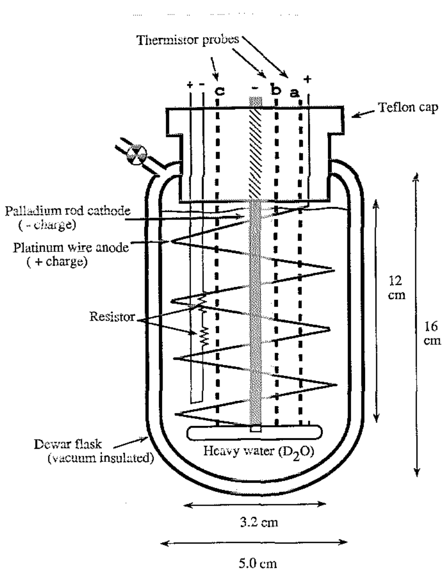
Things began innocently enough on Friday morning, March 24, when the *Los Angeles Times* ran a story on the previous day's announcement. Details were sketchy, but the key ingredients were palladium, heavy water (D_2O, water made with a form of hydrogen called deuterium, which has one proton and one neutron in its nucleus), and electricity. (See box, page 4.) In the lab of Associate Professor of Chemistry Nathan Lewis, postdocs Reginald Penner and Michael Sailor realized with mounting excitement that they could do the same experiment with material on hand. Sailor recalls, "I just wanted to be able to tell my grandkids that the day after cold fusion was discovered I went and did it in my lab." So they raided the evaporator lab for palladium wire and set off in search of heavy water. They came up empty-handed and soon wound up down in the chemistry stockroom faced with the prospect of actually having to *buy* a bottle of the stuff. It almost ended there. Says Penner, "It was 60 bucks for a 100-gram bottle [about half a cup]. So we hemmed and hawed for about ten minutes before Mike said he'd take the rap." "We were just doing it for fun," recalls Sailor. "I said, 'Well, shoot, we've gotta do this. And Nate [Lewis] can afford it.' So we bought it, and Reggie started setting up a fusion cell in a corner of the lab. Then Nate walked in with a prospective graduate student, and asked, 'What are you guys doing?' and Reggie answered, 'Cold fusion.' And Nate just sort of rolled his eyes and said, 'OK. I'll give you guys one day.' Which quickly turned into a month and a half."

The *Times* didn't describe the electrolyte—the liquid that conducts electricity between the cell's electrodes—except to say that it contained D_2O. So the two guessed, wrongly, about the other ingredients, choosing perchloric acid. Pons and Fleischmann's cells were also much larger, about the size of an iced-tea glass, while the Caltech cells were the size of prescription-drug vials.

"We did a lot of experiments that day," Penner recalls. "I know it seems like a dumb idea now, but we had an H_2O cell and a D_2O cell both running over Polaroid films to look for gamma rays. [The H_2O cell was a control cell. If both cells produced gamma rays, they couldn't be due to deuterium fusion.] Meanwhile, Nate had been talking to the people over in physics, and we started another cell in their neutron counter that afternoon. We were pretty excited about that, and we were just amazed by the large-scale physics going on down there; we don't work with that kind of hardware."

And there's plenty of hardware in the Kellogg Radiation Laboratory's windowless subbase-

What's All the Fuss About?



Scientists have spent the last 30 years and several billion dollars trying to harness nuclear fusion, the sun's power source, here on earth. (Deuterium, the key ingredient, occurs naturally as .015% of all hydrogen. Seawater could power the globe for 10,000 years without lowering ocean levels an inch, because huge amounts of energy are released when light atomic nuclei such as deuterium fuse. The trick lies in overcoming the positively charged nuclei's mutual repulsion.) These efforts use complex and very expensive machinery to mimic the hellish temperatures and crushing pressures that make fusion work in the sun, but they have yet to reach the "break-even" point—where the reaction produces as much energy as is consumed.

But one might imagine other ways to make atoms get intimate. Palladium and certain other metals can adsorb deuterium atoms, which fit neatly in the gaps between the much larger metal atoms. If enough deuterium could be forced into a piece of palladium to fill all the gaps and then some, reasoned Pons and Fleischmann, some of the deuterium might fuse into helium.

Their apparatus came straight from a freshman chemistry experiment, called *electrolysis*, in which an electric current passing through a beaker of water breaks it down into hydrogen and oxygen—or, in the case of heavy water, deuterium and oxygen. The negatively charged electrode, where deuterium gas appears, is called the *cathode*, while oxygen forms at the positive *anode*. An *electrolyte* is added to the water to make it conductive. Pons and Fleischmann hoped that the deuterium atoms would find the electrically charged palladium cathode irresistibly attractive, jamming themselves into it to the point where fusion would occur.

If fusion did occur, there would be several telltale signs that are impossible to miss. New nuclei would be produced: two deuterium atoms, with one proton and one neutron apiece, should produce helium-3—which contains two protons and one neutron—plus a free neutron; it should also produce hydrogen-3 (tritium)—consisting of one proton and two neutrons—plus a free proton (an ordinary hydrogen nucleus). Both reactions have roughly a 50 percent probability of occurring. A much less probable reaction (about one chance in ten million) would produce helium-4—which contains two protons and two neutrons per nucleus—and a whopping big gamma ray with an energy of 23.8 million electron volts (MeV). While most of the helium would probably remain trapped in the palladium, the neutrons and gamma rays would escape easily, and would be instantly detectable. Some of the tritium should diffuse out of the palladium, accumulating in the electrolyte. (The palladium and the electrolyte could be analyzed later.) While a fraction of the fusion energy would be carried away by neutrons and gamma rays, most of the energy would be expected to show up as heat in excess of that generated just by passing electricity through the cell. The heat would be measured by *calorimetry*, in which the fusion cell's temperature would be taken with a *thermistor*, a sensitive electronic thermometer. The neutrons and gamma rays, however, would be apparent even at very low fusion rates, where there would be no detectable calorimetric effects.

$$\mathrm{H} + \mathrm{H} \rightarrow \mathrm{D} + e^{+} + \nu_{e}(1.2\ \mathrm{MeV})$$
[solar fusion]

$$\mathrm{H} + \mathrm{D} \rightarrow {}^{3}\mathrm{He} + \gamma(5.5\mathrm{MeV})$$

$$\mathrm{D} + \mathrm{D} \rightarrow \mathrm{T}(1.01\ \mathrm{MeV}) + \mathrm{H}(3.02\ \mathrm{MeV})$$
[~50% probability]

$$\rightarrow {}^{3}\mathrm{He}(0.82\ \mathrm{MeV}) + \mathrm{n}(2.45\ \mathrm{MeV})$$
[~50% probability]

$$\rightarrow {}^{4}\mathrm{He} + \gamma(23.8\ \mathrm{MeV})$$
[10^{-7} probability]

Left: The fusion cell. Right: Fusion reactions. H = hydrogen, D = deuterium, e^{+} = positron, ν_{e} = electron neutrino, He = helium, γ = gamma ray, T = tritium, n = neutron. The energy of each emission is in parentheses.

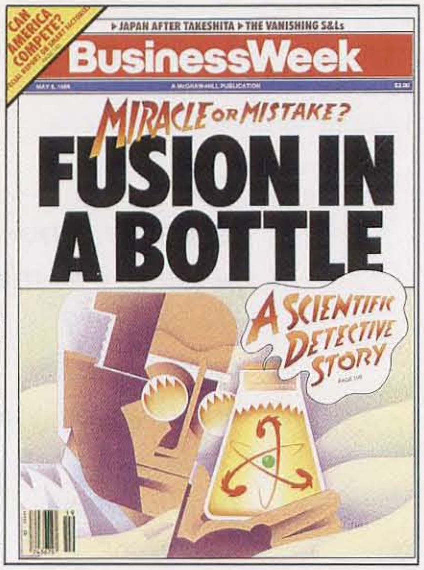
"Nuclear astrophysics is mostly the study of various fusion reactions, so you could say that fusion is our business."

ment. The lab's centerpiece is a mobile-home-sized, 3.5-million-volt particle accelerator used to study the exotic fusion reactions within stars. Not just the run-of-the-mill hydrogen-fusion reactions that make the sun shine, but also the rare fusions of other nuclei that forge all the heavier elements in our universe. These reactions are so slow that after 4.5 billion years our sun is still mostly hydrogen. Thus astrophysicists, in order to see anything useful in their lifetimes—say one event per hour—have to work at energies higher than those in the sun, and have to build detectors sensitive enough that the single neutron, gamma ray, or other product of that event doesn't go unnoticed in the welter of background radiation we live in.

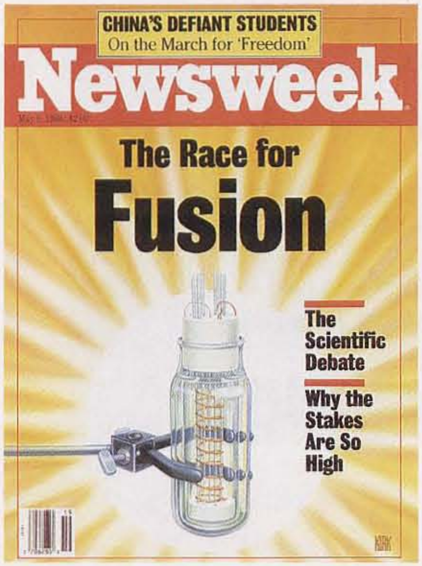
The neutron polycube—the "cube" for short—is one of those detectors. Designed and built by Professor of Physics Ralph Kavanagh and Research Fellow Stephen Kellogg to be incredibly sensitive to neutrons (100,000 times more so than the University of Utah's detector) and oblivious to other forms of radiation, the cube is a bit larger than a telephone booth and is mounted on railroad tracks. Buried within a ton of cosmic-ray-absorbing paraffin wax, polyethylene plastic, and graphite bricks, a 12-inch-diameter cylindrical array of 12 helium-3–filled "proportional counter" tubes—the actual neutron detectors—surrounds a 4- by 4-inch sample borehole. Just outside the detectors, inch-thick sheets of special plastic scintillate, emitting flashes of light when charged particles such as cosmic-ray muons pass through them. These flashes tell the computer that coincident signals from the tubes are probably due to an external source and should be rejected. It was a simple matter of rolling the cube back from the accelerator and modifying the software to set up for cold fusion. Until the number of cells got too large, the cube was switched back and forth routinely between fusion and the lab's regular work.

The physicists had actually gotten wind of the story several hours before the noon press conference the previous day. That morning Ryoichi Seki, a visiting associate in physics, had heard a Japanese-language radio station break the story, quoting the *Financial Times* of London as its source. (Fleischmann had given his brother, a reporter for the *Times*, the story a day early in deference to a British bank holiday, thus scooping his own press conference.) Seki passed the word around Kellogg. Recalls Professor of Physics Charles Barnes, "The *Financial Times* told of this new, boundless source of nuclear-fusion energy obtainable at room temperature. Bob Finn, in our Public Relations office, got me a copy of the AP press release based on the *Times* article almost immediately. It had no details, so we tuned into every newscast we could all day, trying to decipher what Pons and Fleischmann were really claiming to have done. And we speculated on how it might work—Research Fellows Steve Kellogg [no relation to the cereal magnate who funded the lab's construction] and T. R. Wang, Bruce Vogelaar, a graduate student waiting 'in limbo' for his final PhD examination, and I. We immediately realized that we could probably do a better job on the neutrons than almost anybody else in the world, and we could do as good a job as anyone in detecting gamma rays as well. But none of us knew very much about electro-

Reprinted from BUSINESS WEEK by special permission. ©1989 by McGraw-Hill, Inc.



©1989 Newsweek, Inc. All rights reserved. Reprinted by permission.



Copyright 1989 The Time Inc. Magazine Company. Reprinted by permission.

"Over the weekend he convinced me that this thing might fly."

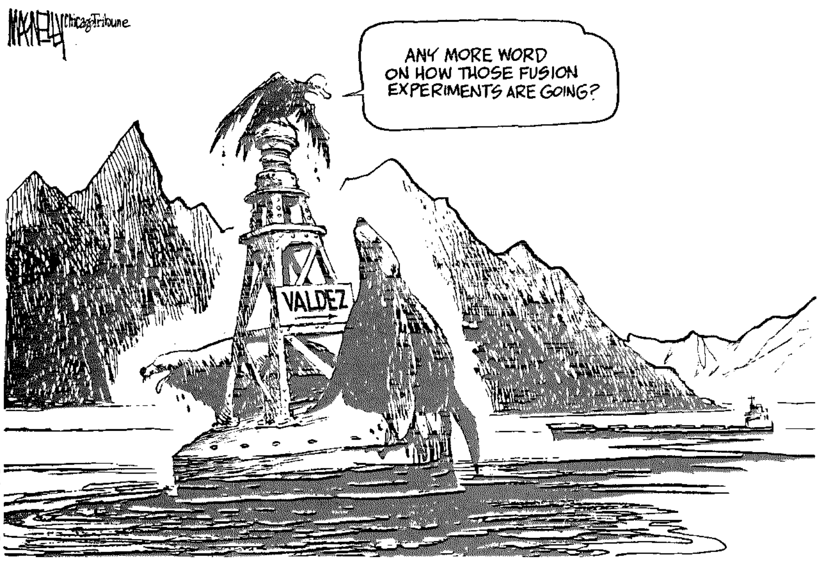
chemistry, and I was wondering whom to call in the chemistry department for help at about the same time that Nate Lewis's crowd decided they needed to find out how to measure neutrons. Anyway, Nate called me early Friday morning before I got around to calling anyone. It was natural that we teamed up. Nuclear astrophysics is mostly the study of various fusion reactions, so you could say that fusion is our business.

"We had a regular convention down there when Nate's group brought over the first cell just after lunch. We had most of Nate's people, all of our people—Steve, Bruce, T. R., and I—and a huge number of hangers-on and spectators, all watching this cell being put into the cube. There were no neutrons. My first feeling was immense disappointment, but my second was relief, because it occurred to me that we really hadn't stopped to take adequate precautions if this cell really was producing a large blast of neutrons. Of course, Pons and Fleischmann were still around to tell their tale, so we felt intuitively that the radiation level was probably not going to be high enough to be dangerous, but, in principle, the neutron flux could have been as high as 10^{12} neutrons per second per watt of output power, and that is really a very high flux. The lab's radiation alarms would have gone off instantly."

The radiation threat didn't elude the chemists' notice. Some jokingly considered lining their shorts with lead foil. (In fact, lead doesn't absorb neutrons all that well.) That afternoon, people clustered around another cell in the chem lab while graduate student Michael Youngquist (from the Baldeschwieler group down the hall) wielded a Geiger counter. The cell seemed to emit erratic bursts of radiation. Youngquist soon noticed that the signal increased every time Reggie Penner stepped forward for a better look, and faded when he stepped back. Youngquist turned to Penner, who stood with his hands in his pockets. "As the counter moved down from his chest to his stomach, the signal got louder and louder and Reggie's eyes got bigger and bigger," chuckled Sailor. "You should have seen his face when the noise maxed out below his belt!" The counter was picking up emissions from the radium-painted glow-in-the-dark numbers on Penner's watch.

The group had found nothing by Friday's end. The one-day limit had expired, so nothing much went on that weekend, although Sailor did try a crude calorimetry experiment in a styrofoam cooler, without result.

On Monday, all thought of a deadline had evaporated. Says Lewis, "I talked to [Professor of Theoretical Physics] Steve Koonin quite a bit. Friday morning he recommended that we work with Charlie Barnes on measuring neutrons and gamma rays, and over the weekend he convinced me that this thing might fly. [Koonin, on leave at the Institute for Theoretical Physics at UC Santa Barbara, had also heard from Seki.] That innate Caltech curiosity set in, and I got all excited." Lewis (BS '77, MS '77), Kellogg (BS '78), Koonin (BS '72), and Kavanagh (PhD '56) are all Caltech alumni. Meanwhile, more information had come in by phone, fax, and BITNET (a nationwide network of university and research



Reprinted by permission: Tribune Media Services.

institute computers). This day set the pattern for the next two weeks. A new fact would come in through the electronic grapevine, and a new batch of cells would be set up accordingly. Today's tidbit was that the electrolyte contained lithium in some form. Lewis's team guessed, again wrongly, that it was lithium perchlorate.

They also discovered a nonradioactive hazard: the power supply to their electrodes sparked when changed to another setting, causing one cell to explode. The cells were so small, however, that no damage was done.

"We also found out that Polaroid film wouldn't work," recalls Penner. "So we got together with the Baldeschwieler group, who work with radiation a lot, and [grad students] Mike Youngquist and Steve Novick prepared a very sensitive film experiment for us. We ran at least six cells on those films in two days, plus control cells, and we still didn't see anything."

> *"It was already at the point where you couldn't buy* any *palladium."*

Wednesday morning, the 29th, the group learned that the electrolyte was alkaline, ruling out their previous picks. The obvious choices now were lithium hydroxide in D_2O, which was on hand, or lithium deuteroxide in D_2O, which wasn't. The cells of the day were made up with lithium hydroxide.

Meanwhile, over in Kellogg, the physicists had pressed into service two very sensitive gamma-ray counters from the accelerator lab—a large-volume, state-of-the-art "intrinsic germanium" detector, and a sodium iodide scintillation counter. The two complement each other, as the germanium detector looks for low-energy gamma rays and the sodium iodide detector senses high-energy ones. Both detectors were cocooned in a ton of lead shielding.

The group also looked at claims by Steven Jones of Brigham Young University to have seen fusion—albeit at levels too low to be a useful power source—in a somewhat similar setup. Graduate students Amit Kumar and Sharon Lunt joined the rest of the group in an all-nighter to build a 48-cell array when Lewis's calculations showed that a single Jones cell wouldn't generate enough neutrons to give a good signal. They didn't see anything with the 48-cell array, either, but hopes remained high. (Work on the Jones cells is continuing.)

Sometime between March 29 and April 1, Lewis finally received an oft-faxed preprint of Pons and Fleischmann's paper, and learned that the magic electrolyte was, in fact, lithium deuteroxide. The group also learned that the effect increased with electrode size. Unfortunately, the only palladium to be had around Caltech was the 10-mil (.01-inch) wire they had been using, while Pons and Fleischmann were using rods .22 cm (about .09 inches) thick. So the hunt was on for more palladium.

Recalls Penner, "All we found were two palladium thimbles. The physicists had them; someone dismantled an old gas-purification apparatus from their accelerator for us and pulled them out. They were all greasy and dirty, but we cleaned them up. They were at least 99.9 percent palladium, and that's pretty good metal." (The 10-mil wire was ultrapure "five-nines" metal, 99.999 percent palladium.)

They calculated that fusion should start after about 10 hours, the length of time it would take for deuterium to saturate the palladium lattice. (This "charging time" is calculated from the electrode's radius and the diffusion constant of hydrogen in palladium, an extensively researched number.) By contrast, their little wires had been fully charged in 20 minutes. They gamma-counted and neutron-counted for two and a half days, and came up empty once again.

"We'd been calling all over the U.S. this whole time," says Penner, "trying to get thicker palladium wire. And it was already at the point where you couldn't buy *any* palladium. You just couldn't get it anywhere. That's what Nate was doing most of the time, calling people and finding out information and feeding it to us, and getting us raw materials."

Sailor adds, "He called up Engelhard [a national precious-metal supplier] on the weekend, and told the guard who answered the phone, 'Hi-I'm-Nate-Lewis-from-Caltech-we're-trying-to-do-cold-fusion-do-you-have-any-palladium?' all in one word. But they needed five guards to open their vaults, and they'd trip all these alarms, so they said call Monday. Monday it turned out they didn't have any anyway. But Nate found this place in Long Beach, the David H. Fell Company, and so he comes into my office saying, 'The last palladium in all of southern California is at this address, and they're holding it for us! Go!' I don't have a car, so he tossed me the keys to his RX-7, and I couldn't pass that up. I went by my money machine on the way down and took out a hundred bucks, but the price had gone up since the guy had talked to Nate and it was now $120—for an 18-inch rod a little thicker than hanger wire, less than an ounce. He gave it to me anyway and said, 'Don't worry about it. Send me a check for the difference.'" Says Lewis, "Fell was really interested in our work, and just incredibly cooperative. He gave us palladium at cost, and later he sent a bar over to a jeweler friend of his to cast, on credit, and we paid for it afterward."

"It was clear we were adsorbing all the deuterium into the palladium cathode. We thought that was the key."

Fusion fever had gripped the scientific community. Not only had palladium futures soared, but D_2O had vanished from campus stockrooms. Some of it resurfaced in unusual spots. "Nate was out at dinner with some old buddies of his that first week," recalls Sailor. "They were talking about fusion, and this guy at the next table said, 'Hey, are you from Caltech? Are you doing this cold-fusion stuff? We've got some D_2O if you need it.' Just out of the blue. Turned out they were Caltech undergrads. They were going to do the experiment in the dorm." (Kids, don't try this at home!) "At one point, we had to see if just putting D_2O in the cube would produce neutrons from interactions with cosmic rays," said postdoc Gordon Miskelly. "So we borrowed 15 bottles of D_2O to stick in the cube. Reg [Penner] put up this big sign saying, 'THESE BOTTLES ARE BORROWED! DO NOT OPEN!' At $60 a bottle, it would have been an expensive mistake."

"After a little while," recalls Lewis, "when we realized we were spending lots of money, I wrote to the provost and said, 'Caltech's a first-class place. We need first-class research bucks.' And they gave it to us. Unrestricted, to support the supplies. We couldn't have done nearly as thorough a job otherwise."

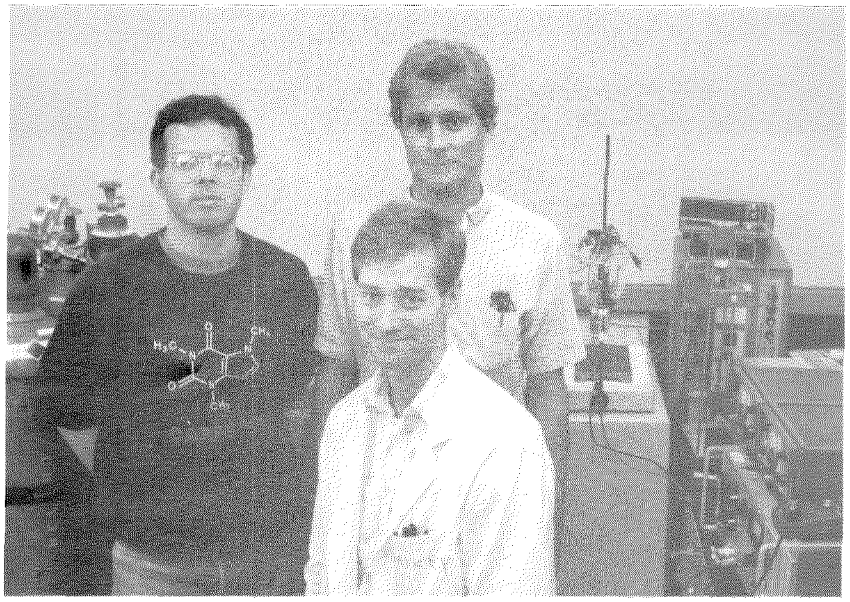
That same weekend the group discovered that the electrodes needed pretreatment. Graduate student Pat Santangelo had carefully read Fleischmann's old papers on hydrogen-deuterium separation, which used a setup similar to his fusion cell. These papers showed that he baked his palladium in a vacuum for several hours to drive out any adsorbed gas. Then, when operating a cell for the first time, he reversed the current direction for about half an hour. This grew a porous oxide on the palladium surface, which, when the cell was switched over to its operating mode, was reduced back to porous palladium metal. Explains Penner, "You get a very high surface area. And the weirdest thing happened. When we started to charge the cell, there was no gas evolved at the cathode. Normally, when you electrolyze D_2O, you get gas at both electrodes, deuterium at the cathode and oxygen at the anode. So it was clear that we were adsorbing all the deuterium into the palladium cathode. We thought that was the key.

"Everything was right with that experiment. We did the pretreatment, we had *exactly* the same amount of palladium, the same electrode geometry, the right electrolyte, and the right current density. And we ran this in excess of 450 hours—that's 18 days—alternately gamma-counting and neutron-counting in Kellogg, with identical cells running for calorimetry in the chem labs. We knew it would take 33 hours to charge, so we let it run for a long, long time."

The calorimetry runs, meanwhile, had moved from Sailor's styrofoam cooler to Professor of Chemistry Robert Grubbs's precision calorimeter, and into Miskelly's keeping. Miskelly says, "I scoured the model shops of Pasadena for boat motors and propellers I could use for stirrers. They didn't work too well, but I found good motors in an electronics store, and I became quite proficient at cutting my own propellers out of little plexiglas blocks."

Shuttling cells from one spot to another was a touchy business, because current had to be kept running through them at all times. The moment a cell was unplugged, deuterium began to boil out of the electrode. Thus cells were assembled in the chem labs and taken to their destination before the switch was thrown for the first time. Soon Kellogg's subbasement was festooned with extension cords between the neutron cube and the gamma counter 40 feet away, so that cells could be moved without disconnecting them. For the few occasions when a live cell had to be toted across campus, Santangelo rigged up a cart with an on-board power supply—two car batteries borrowed from the Baldeschwieler group, whose members had been using them as part of an ultra-low-noise power supply for their scanning tunneling microscope.

New cells were started continuously, exploring variations on the cold-fusion theme. The new .22-cm rods were only 99.9 percent pure, so to ensure that trace impurities weren't sabotaging



Left to right: Tufts, Miskelly, and Sailor (seated) with the precision calorimetry setup. *The constant-temperature bath is the fridge-sized cabinet behind them.*

"Every day we learned something that made us think everything we'd done so far was wrong. So we'd say, 'That's it! That's the thing!' and make a new cell. But we believed in cold fusion 100 percent."

the experiment, more cells were made up with brand-new, properly pretreated 99.999 percent pure 10-mil wire. Another set of cells was made from palladium supplied by Texas A&M, who had reported excess heat from cells made from the same material. Each cell required its own set of control cells, and all were running simultaneously. "It got really hard to find power supplies," says Penner. "We had everything Tom Dunn could find in the chemistry department's electronics shop, and all we could get from the physicists in Kellogg. It became the rate-limiting step in starting new cells."

But new cells had to be started. On April 19, nearly a month after the initial announcement, a rumor surfaced that the electrode had to be cast. A frantic group meeting was called that night. Since palladium melts at 1552° C—higher than the melting point of quartz glass—and molten palladium dissolves carbon, making graphite crucibles useless, casting palladium isn't trivial. Grad student Mike Heben, the group's materials scientist, was the designated caster. Says Heben, "We called [Professor of Geology] Ed Stolper at home about 8:00 p.m.—had no previous contact with the guy—and said, 'We hear you have a high-temperature furnace. Can we use it?' And he says, 'Sure.' And a research associate of his, John Beckett, came in at nine that night and stayed till about 3:00 a.m. helping us cast. Our first two tries failed when the plug in the bottom of the alumina mold melted, and the palladium wound up in the bottom of the furnace. We were turning palladium back into minerals at an incredible rate."

"We had cast palladium the next morning," says Lewis. "Way before anybody else. Other people were five and six days behind us." The next day, grad student Gary Shreve, along with Professor of Materials Science William Johnson and materials science postdoc Hans Fecht, used a powerful radio-frequency-field furnace to make a blob of molten palladium. "The Blob" joined Heben's cell in the rotation, followed shortly by "The Gang of Four"—four identical cells made from palladium cast by David Fell's jeweler friend. Each cell ran for over 300 hours.

"This casting business really bummed us out," says Penner. "It implied that everything we had done up to April 20 was wrong. Which wasn't true. We found out later that Pons and Fleischmann hadn't used cast palladium at all, but just regular extruded wire." "But we had to check all the possibilities," says Lewis.

"The first two weeks were an incredible emotional roller coaster," says Penner. "Every day we learned something that made us think everything we'd done so far was wrong. So we'd say, 'That's it! That's the thing!' and make a new cell. But we believed in cold fusion 100 percent. The high point was always after starting the electrolysis down in Kellogg and watching the counts on the neutron counter. You'd be really fired up, and the count would jump, 'Wow! Here they come!' And then it would go back down." Says Lewis, "There was a lot of that. It would start at six counts and jump to eight. Eight counts! Two extra counts! Then it would go down to seven, but it was still an extra count!" Physicist Steve Kellogg had a slightly

The days quickly settled into a routine. All the basic ingredients were known, and the group was just waiting for one of their cells to show signs of life.

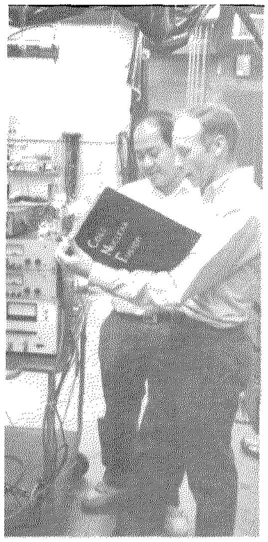
different view: "The chemists would get all excited as soon as we started a count. There's always some background—from traces of uranium in the detector tubes' steel walls, and other things that are well understood—so the counter always registered something. The screen updates the number of counts every three minutes, and so I started to bet pennies with the chemists on what the next total would be. I bet on the background level, and I always won."

Mike Heben remembers, "The fax machine was buzzing all the time, spitting out different pieces of data." "And there were rumors everywhere," adds Lewis. "Some rumor that Bell Labs did it. A rumor that Bell Labs didn't do it. Rumor was that *we* had done it, and wouldn't tell anyone. People would call us up and say, 'We know you've done it! We know you're lying about it, too!' We heard Florida had seen tritium. Ten minutes later I was on the phone with the guys in Florida to figure out what they did. I was on the phone 12, 15 hours a day over three time zones. It was fun. The intensity was amazing. The whole group was in on it. People were up all night—I'd be in at two or three in the morning and there'd be people in." Says Miskelly, "Every time there was a rumor that someone somewhere had found excess heat, I would get dragged back into the lab at midnight to recheck and recalibrate my cells and reassure everyone that we hadn't made a mistake and missed a heat gain."

Barnes notes, "I certainly wasn't excluding the possibility that the various changes the chemists were making might actually work. I really hoped Pons and Fleischmann were right on some level, and that we'd be able to see something." T. R. Wang concurs, "We all agreed, with our knowledge of nuclear reactions, that we couldn't see how fusion could happen at such low energies, but we were trying to prove them right. We spent lots of time trying to think what could be wrong with our setup . . ." Adds Barnes, "Sure. How come someone else sees neutrons and we don't? What are we doing wrong?"

Although the group wondered what they were doing wrong, they were also aware that others might be making mistakes. Lewis's and Barnes's marathon phone sessions with researchers who were rumored to have seen something included talking them through their experiments step by step, looking for sources of error. Elevated neutron counts could be traced to heat-sensitive neutron detectors. Helium results were consistent with the levels found in typical laboratory air, as Sailor discovered, especially if there were liquid-helium-cooled instruments nearby. Tritium, another fusion byproduct, was sought with a liquid scintillation counter, which graduate student Bruce Tufts found gave false readings when its scintillation "cocktail" was contaminated with the electrolyte. "We fixed more than one experiment," says Lewis.

The days quickly settled into a routine. All the basic ingredients in the recipe were known, and the group was just waiting for one of their cells to show signs of life. Says Barnes, "The neutron detector and the gamma detectors were running 24 hours a day at this point. A typical run would be 12 hours, in order to get as high



Keeping vigil in the control room. Wang watches the neutron counter's readout while (from left) Penner, Kumar, Lewis, Kellogg, Lunt, and Youngquist fight boredom. Is it 1:40 a.m. or p.m.?

a sensitivity as possible, and then 12 hours of background. We didn't have to be there all the time, though, especially running background, because the computer recorded everything." Penner agrees. "T. R. [Wang] and Steve [Kellogg] were really handling the counting with minimal help from us. We took turns going over and refilling the cells with D_2O twice a day."

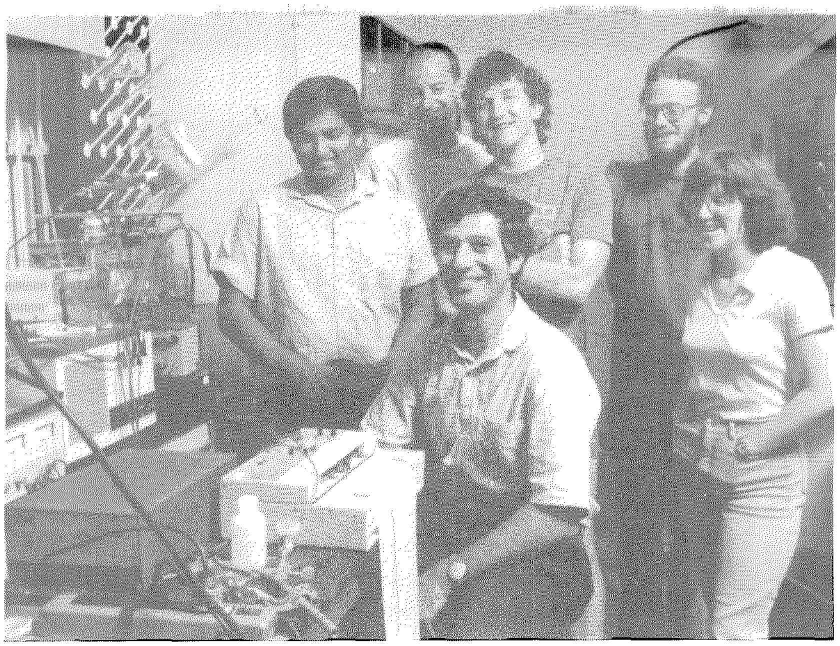
Refills were a bit tricky after hours, as the chemists' keys wouldn't open the physics building. Admits Penner, "We kept talking about getting a key to Kellogg, but we never got around to getting a requisition form. Once in the building, it usually wasn't a problem getting into the lab itself, because Steve was in there almost every night. But we had to find an open window to get into the building." Adds Tufts, "Mike Heben and I went over one night, while Steve was at the APS meeting in Baltimore, and we couldn't get into the lab. We called Security, and they couldn't get us into the lab, either. The guy tried every key he had. And we were really desperate, because we had to add D_2O to keep the cells running. I had noticed earlier that the lab abuts a machine shop, which had an emergency exit that went up through the ceiling to the ground floor. We realized it had to come out in the seminar room. After kicking around for a while, we found this floor plate. I got all the screws taken off the hinges, but it was double-latched by bars underneath, so I still couldn't get in. But it would have been really neat. We wound up calling T. R. at about 1:00 in the morning, and he's a really good guy, so he came in and let us in."

As time passed, and nothing worked, the mood shifted. Kellogg says, "When we didn't see anything, we knew we should set stringent upper limits on how much fusion could be occurring. And when you take that approach, you assume you won't see anything. Then if you see something, the first thing you ask yourself is, 'what am I doing wrong?' Once there was a neutron source behind a wall ten meters away, and my detector was so sensitive it registered." On another occasion, a "burst of neutrons" was traced to electronic noise from a faulty connector. Another time, a calorimetry cell that suddenly started to "run hot" signaled not fusion, but a dying battery in a thermistor.

The last week in April, after doing precision calorimetry on many cell designs, Lewis and Miskelly decided to test an exact replica, as far as possible, of the Pons-Fleischmann cell to see if quirks in its design would produce heat anomalies. "We used photographs from the *L.A. Times* of Pons holding the cell, and you could see pretty well how it was made," say Sailor. "You could tell it was double-walled glass, for example, because you could see the little nipple on one side where it had been sealed off; that also meant it was probably a vacuum vessel, like a Thermos bottle. We used Pons's finger for scale. Gordon figured his hand was about equal-sized, so he scaled it to his own finger." "It was like 'Columbo,'" Lewis says. "Eric [Kelson, a graduate student in the Bercaw group next door] was in Utah, and he had videotapes from the local TV station, and we looked at them to find out what the readings on their thermistors were, where the electrodes were, and how they were doing their calorimetry—whether the cell was in a constant-temperature bath, where were temperatures measured, how it was all hooked up."

Caltech's cell differed from Pons and Fleischmann's in two significant respects, however. Miskelly's design included a valve so that space between the double walls could be pumped to any degree of vacuum. "It wasn't certain how much theirs was evacuated," Sailor explains. "And isoperibolic calorimetry is based on the concept of a leaky Thermos. You contain some of the heat, and the rest leaks out at a known rate into the constant-temperature bath. If you have a really well-insulated flask with a good vacuum, the heat leak will be slower, and the contents of the Thermos will come to a steady state that is much hotter than outside, say 40 degrees. And that temperature difference is what you measure. But if your Thermos leaks—in the worst case if you do it in an aluminum can—it would equilibrate with the outside very quickly and your temperature rise would be maybe a couple of degrees or less." The other difference was that Miskelly's cell had several holes drilled in its cap so that thermistors could be placed in many locations to look for hot spots within the cell. Pons and Fleischmann's design relied on the bubbling gas at the electrodes to stir the cell's contents instead of having a mechanical stirrer, a design that Miskelly believed was inadequate to ensure uniform temperature throughout the cell. It was a crucial test, because small differences in the cell's observed temperature became large calculated heat outputs. And, indeed, the group found temperature variations within the cell, despite the furiously bubbling gas at the electrodes.

Says Miskelly, "At lunchtime I asked Gabor Faludi, our departmental glassblower, to make the cell, and I gave him the plans at 2:00. He had it done that afternoon, put it in the annealing oven overnight, and gave it to me the next



From left: Kumar, Penner, Heben, Kelson, Lunt, and Lewis (seated) with a fusion experiment in the chem lab.

morning. Which is as fast as you can get." Sailor says, "He basically threw everything else out the window for us."

Meanwhile, on April 26, Pons and Fleischmann appeared before a hearing of the House Committee on Science, Space, and Technology. Although scientists from several other institutions followed with negative evidence, the seductive promise of unlimited, cheap energy won out, and the committee appeared receptive to a proposal by Chase Peterson, the president of the University of Utah, that $25 million would be just about right to set up a cold-fusion center. When that happened, Caltech's plans to abide by the decorum of the scientific process—a peer-reviewed paper, published in the scientific literature, followed by a public announcement—went out the window. Bruce Tufts recalls, "Nate got really worked up. He never cusses, but he does get agitated. He didn't sleep that whole night. He had a tape of the hearing from C-SPAN, and every time he'd try to go to sleep, he couldn't, and he'd get back up and watch more of it. He eventually ended up watching all six hours of testimony." Graduate student Gary Shreve adds, "He came back to the lab, but everyone had gone home. He was so agitated he had to talk to somebody."

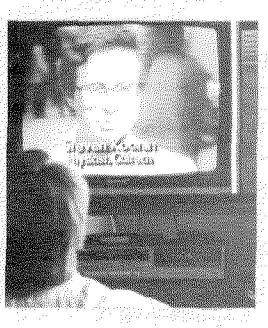
"Gordon was up late-late-late about three days straight getting data, right before the American Physical Society meeting in Baltimore," Sailor remembers. "Nate was going to speak there on May 1. We had almost all the figures, but he really wanted this data from the Pons-Fleischmann cell." (Lewis and Miskelly had already gone back through Pons and Fleischmann's paper and back-calculated what their raw data must have been, and discovered that all their really big heat outputs were based on calculations, not measurements. And Heben, Penner, and Tufts had run a set of power-conversion experiments that cast serious doubt on the assumptions underlying the calculations.) "Nate was going to talk at seven that evening, Baltimore time," says Miskelly. "We finally got all this ready at about 10:00 a.m. our time. We'd given him blank overhead transparencies before he left, so we faxed him the sheets and he photocopied them onto the transparencies at the hotel and used them that night—taking advantage of modern communications."

Says Barnes, "The calorimetry was crucial, so we decided that Nate should present our results to the APS. They had organized a special session on cold fusion at the last minute. We went through an especially hectic few days rechecking the neutron and gamma-ray data and putting them in the best form for presentation."

Recalls Kellogg, "Baltimore was the high-water mark. That Monday, *Time*, *Newsweek*, and *Business Week* all ran cover stories on cold fusion. The APS's special session had more people who wanted to talk than could be accommodated in one evening, so it ended up being divided into two. A total of 40 talks. The first four were invited. Steve Jones of Brigham Young University led off with his measurement of a very low rate of neutrons—he was at the conference to talk about his cold-fusion work anyway, but his session wasn't until Thursday. He was very careful to point out that he was working independently from Pons and Fleischmann, that they were reporting different-sized effects. [Jones would say later that if his energy output was the size of a dollar, then Pons and Fleischmann's was equal to the national debt.] Then Rafelski [a theorist from the University of Arizona who often collaborates with Jones] spoke. Then Steve Koonin's talk on the theoretical problems, in which he concluded with that assessment of 'incompetence and perhaps delusion.' A hush fell over the hall."

Says Koonin, "The week before Baltimore, serious doubts began to emerge. Nate turned up all these potential errors in the calorimetry; MIT showed the so-called gamma-ray measurement to be an artifact; all our neutron and gamma-ray measurements were nil, and the rest of the world's best experimental groups couldn't reproduce their results either; there was no theoretical way to explain them; and Pons and Fleischmann weren't answering questions. We were really

The Theorist's Tale



Professor of Theoretical Physics Steven Koonin is on leave at the Institute for Theoretical Physics at UC Santa Barbara this year, but he, too, got swept up in fusion frenzy. Koonin recalls, "We started thinking about how cold fusion could work, since there was no known process that would explain it. There was a great chase off to the library to see what people knew about hydrogen in palladium—we turned up a whole book on the subject—and we started talking. We had no idea quantitatively how hard it was to make cold fusion go, so Michael Nauenberg, who's a professor of physics at UC Santa Cruz, and I did a simple calculation to see what were the rates, how hard you had to squeeze the nuclei, which of the different possible reactions was fastest, and so forth. I was able to take a program out of the computational physics book I wrote for a course I teach, and modified it just a little bit and got it to do the calculations."

The calculations showed that the deuterium-deuterium fusion rate had been underestimated about 10-billion-fold. But the new rate was still incredibly slow—a sun-sized mass of deuterium would only undergo one fusion event per year. Hydrogen-deuterium fusion turned out to be 100 million times faster than deuterium-deuterium, but even that didn't help much. "In retrospect, they were the simplest calculations I think I've ever published, but they've certainly gotten the most notoriety. Michael and I wrote them up, and submitted them to *Nature*, now in press, and by Thursday the 30th or so we also published a preprint on BITNET, sort of like an electronic chain letter. This whole story is tied up with BITNET and faxes. Later on, I gave a talk at Cornell titled *Cold Fusion: Facts, Fantasies, and Fallacies.* Some wag changed it to '. . . *Faxed Fantasies.* . . .'

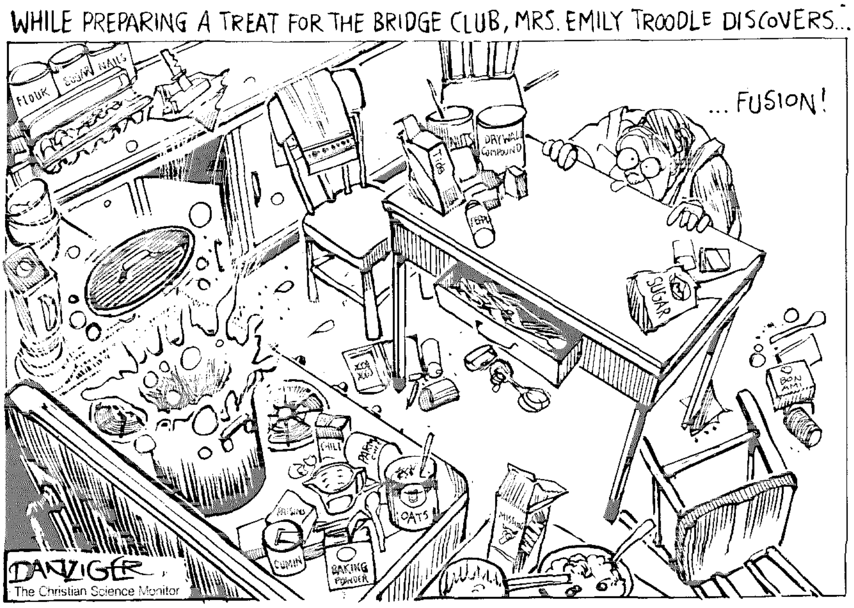
"One of the people who got the BITNET was Richard Garwin, a prominent physicist at IBM. He had been asked by the Italians to get some Americans together for a conference on cold fusion to be held in Erice, Sicily—a well-known physics center. He called me on Saturday, April 8, and early Monday morning I was driving to LAX to catch a plane for Rome. On the way, I heard that Texas A&M had announced confirmation, and I thought, "This could really change the world, if it's right."

"It was a one-day conference. There was a big banquet the night before—I sat next to the Governor of Sicily—but I was jet-lagged and couldn't eat much. The next morning at the conference hall, there was a huge crowd of press people, and so we had two hours of press grandstanding before we could settle down to science. Meanwhile, during the conference, there were all these reports of confirmation coming over the phone and on faxes. Fleischmann was there, and people grilled him pretty hard. Many people believed him, and even the skeptics couldn't find anything obviously wrong.

"On the way home there was a strike at the airport in Rome, and I missed my connection. I pulled into my hotel room, turned on the TV news, and saw Pons being cheered at the American Chemical Society meeting in Dallas. Then I opened the *International Herald Tribune* and there's Charlie Barnes being interviewed on the front page. After a quick phone call back to Caltech to check the latest results and rumors, I started thinking again about how things could work, and essentially wound up writing a paper in the hotel room that night. That's the nice thing about being a theorist. I was scribbling the whole flight back from Italy." This paper shows that fluctuations in the way palladium and deuterium atoms vibrate within the electrode might substantially enhance fusion rates. Fluctuations could be enhanced by cycling the electric current or the cell temperature up and down. Modest fluctuations, well within the realm of possibility, could enhance rates sufficiently to explain Jones's results, but implausibly large fluctuations would be needed to account for Pons and Fleischmann's.

"I got home Sunday, stopped at home to say hello to my wife, and headed to the lab—jet-lagged as I was—to type up the paper and put out a preprint on BITNET.

"A few days later Joe Redish calls me from Maryland—he had read the preprint—and says the American Physical Society wants to hold a fusion session at their Baltimore meeting, and would I be one of the speakers? I knew it was important to get our experimental group on the program as well—we were in touch by phone almost daily at this point—so I made a strong pitch to Joe to put Nate on, and when Fleischmann declined, there was an obvious opening."



Danziger in The Christian Science Monitor ©1989 TCSPS

starting to disbelieve the whole business. So I finally decided to hit really hard at Baltimore. I think we've done well, but there's always a chance—one in a trillion, or whatever you want to call it—that we're wrong. We would all be so happy—overjoyed—if we're wrong, but considering what's happened since, I think we're not. Theorists are allowed to float trial balloons. Experimentalists, never."

Lewis's talk followed Koonin's. Says Kellogg, "Nate made a very good presentation. He was able to explain the electrochemistry to the physicists, and all the possible sources of error and the questionable assumptions in the calculations. And, as the press reported, half of the audience—who I think had come skeptically but with open minds—didn't see the point of staying another two or three hours to listen to everybody else. Most of the subsequent talks were people getting up and saying, 'We did this, we did that, we didn't see anything.' And late into the night, somebody said, 'Well, I'm here to add my voice to the Greek chorus.'

"After the meeting broke up at about 12:30 a.m. or so, I went back to the hotel and turned on CNN. And already the emphasis had shifted to Nate and Steve, and the skeptics. And I remember saying to myself that I had just seen this whole deal peak, and now it's coming down the other side." Kellogg had been there to present his own work, work which had been dislocated when the neutron cube had been preempted for cold fusion. He says ruefully, "I was trying to finish up an experiment for my talk when this all started, so my days got a lot longer.

"The second evening, Douglas Morrison from CERN [the European Laboratory for Particle Physics] talked about the sociology of reporting results. I think any of us could have predicted that there would be early confirmations, with so much pressure from funding agencies and administrators who want it to be known that their people are doing something important and newsworthy. So Morrison pointed out where the yeses and noes were coming from. And apart from the U.S., which was mixed, he saw this regionalness. Brazil and India said yes. Western Europe said no. Anyone who said anything in the Soviet Union or China said yes. So the society you live in can dictate whether you step forward to present a negative result, or a questionable positive result. Surely the experiment had been done in other laboratories there, but we weren't hearing the negative results.

"After Jones gave his real talk Thursday morning, I stayed to talk with him afterwards, along with some other guy who introduced himself as being from Langley, Virginia. CIA. . . . It draws everybody's attention when you claim you can generate tritium in your kitchen. It was correctly assessed by the media as having tremendous potential impact on the world order. Jones does have interesting results, and I think the geophysical motivation is a compelling one. His work gives a very nice explanation for the unusual helium ratios in volcanoes."

While back East, Koonin and Lewis made a pass through the halls of Congress in an attempt to counterbalance Pons and Fleischmann's testimony. "That was an interesting experience," recalls Koonin. "We saw only two congressmen, but we saw staff people from three different ones. That's really unusual, to get to talk to a congressman at all. But the staff people had their heads screwed on right; many of them had degrees in science themselves, and they understood. But I don't know what kind of impression we made. Somebody once told me that when you get into that kind of situation the best you can do is prevent something bad from happening rather than do any good."

Adds Kellogg, "If the Utah legislature wants to fund Pons and Fleischmann, from where I'm sitting, that's fine. But when *my* Congress wishes to do so, then I want my work to be weighed against these other fellows' claims."

Media madness set in at Baltimore, and Lewis and Koonin were dragged into the spotlight. Said Koonin, "It's weird talking to reporters. They clearly know what they want to get out of you, and it's not necessarily what you want to say. Everybody gets their 15 minutes of fame, and I think I've had mine."

In the hall outside Lewis's lab, clippings and cartoons proliferated on the wall, along with a running tab on palladium futures. A blackboard (whiteboard, actually) listed the day's media appearances. One day read: 6:30, CBS News—Koonin . . . 7:00, ABC News—Lewis . . . 11:30, "Nightline"—Lewis. (The "Nightline" appearance was preempted by coverage of the first woman arrested under a new Los Angeles law designed to make parents responsible for alleged street-gang activity by their offspring. The charges were later dropped.) Other items on the board included a "Fusionbusters" T-shirt design contest, and graffiti such as "Fusion in a jar? We prefer peanut butter."

The experiments consumed D_2O as if it were water.

The next round came in Los Angeles, at the Electrochemical Society's annual meeting. As at the APS meeting, a cold-fusion session was hastily organized, followed by the inevitable press conference. (Lewis and company, being electrochemists in good standing, would have been there anyway. The group presented half a dozen papers on their normal work at the regularly scheduled sessions.) The press conference put Pons, Fleischmann, Jones, and Lewis together in the same room for the first time, along with other partisans of both sides, and was rather fractious. When it ended, Pons and Fleischmann made a quick escape through a side door, while Jones and Lewis, who were a shade slower out of the blocks, were mobbed by reporters. The two, backs pinned to the wall by the press, stood practically shoulder to shoulder as they adressed their respective seas of reporters. Lewis answered questions for half an hour or so until rescued by a cavalry charge of students from his Chem 1 class, led by Beckman Professor of Chemistry Harry Gray. Lewis lunged into their protective envelope, and the convoy headed for the exit in a manner reminiscent of the Beatles dashing for their limo during their touring days.

"We truly hoped that nature had provided yet one more surprise for us. We were disappointed that it didn't work out that way."

Things have quieted down since then. Most of the people have gone back to the research they put on hold. Some are still testing Jones's and others' results, including an Italian claim, confirmed by Los Alamos National Laboratory, of neutron production from titanium shavings in pressurized D_2 gas. Papers are being published, but the urgency is gone, as these low-yield claims do not appear to be of commercial import. Some are just now catching up on their sleep.

Says Penner, "It was emotionally trashing, but incredibly fun. I wouldn't trade it. It's been the only time that I've been working on something with the potential to revolutionize technology. And not to be blowing our own horn, but it became clear after about two weeks that we had done as many experiments as, or more different ones than, anybody else. People were really looking to us for guidance."

Notes Lewis, "Caltech's probably the only place where we could have done this much this fast. The physicists were all great, and the geologists and materials scientists who helped us cast palladium on fifteen minutes' notice, and Judy Campbell, the biology professor who taught us how to do scintillation counting; we met lots of nice people. Smart people."

Adds Barnes, "It was a most invigorating experience for all of us. We truly hoped that nature had provided yet one more surprise for us. We were disappointed that it didn't work out that way, although we'll always wonder if there isn't something else we should try. Meanwhile we're testing other recent claims of low-level neutron production. The most enjoyable part of the whole project was working alongside the chemists and theorists, with everyone so excited about the urgency of the work."

Concludes Koonin, "It's been fun. Great science—not in the way we usually think about it, because of the mix of politics, sociology, and psychology involved. It was brought home to me that we were in the midst of something that's at least socially important, if not scientifically, when I gave a seminar at Cornell on my way home from Baltimore. After the talk a historian of science came up to me and said, 'Save all your transparencies. Save your notes. You guys are moving a little too fast for us right now, but we'll catch up to you.'"□—*DS*