California Institute
of Technology

Engineering & Science

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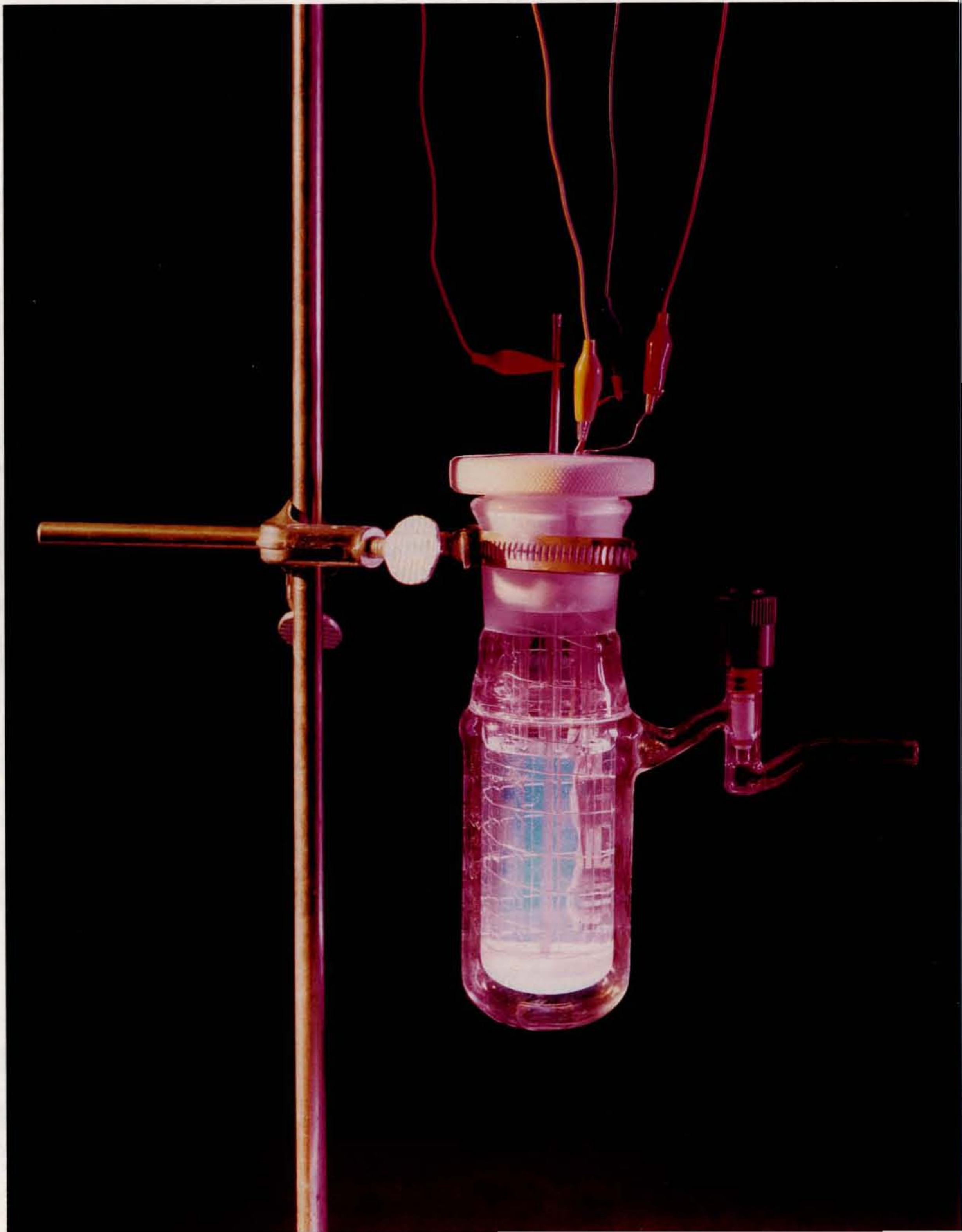

SCIENCE/SCOPE®

Voice and data communication to and from vehicles virtually anywhere in North America will soon be possible through a satellite system under development by Hughes Aircraft Company and seven other companies that form the American Mobile Satellite Consortium. The system would allow drivers unrestricted contact with any telephone anywhere in the world. Current cellular telephone systems require drivers to be within range of special two-way radio towers, leaving about 15 percent of the United States population without service. Initial customers for the new system will be trucking companies, fire fighters, search and rescue teams, and personnel working in remote areas. The service will also be available to aviators and mariners.

New primers help protect electronic circuits in neural prosthesis devices. The primers, plasma polymerized hydrocarbon films developed by Hughes, are used to bond biologically inert protective coatings to the devices for periods of over 10 years. The prosthesis devices help victims of neural trauma, such as stroke, to regain some of their lost neural functions by electrically stimulating proper areas of the brain. Plasma polymerized films are also useful as protective coating in many other applications such as infrared optics that are exposed to extreme conditions of sand and salt water.

An integrated security management system that can monitor and display security and fire alarms will help security forces operate more efficiently. The system, designed by Hughes for General Motors' Regional Personnel Administration, will integrate new and existing systems in 180 GM plants throughout the United States. GM will establish 12 Regional Personnel Centers (RPCs) to serve the plant sites. Each RPC will perform central monitoring and control, rather than each plant site performing its own, as is presently the case. The new system has the potential to save GM millions of dollars each year. A similar Hughes-designed system is currently installed in the Smithsonian Institution in Washington, D.C.

A computerized inventory system that can locate equipment companywide by performance parameters and availability may control costs on new programs. Hughes' new Equipment Screening by Performance Specifications System can identify, locate and re-deploy existing equipment such as test instruments and oscilloscopes. Inventories can be searched based on performance parameters or substitute equipment—for example, plug in modules that will work with an existing base unit—can be located. The system has the potential to reduce capital equipment expenditures at Hughes 2 to 10 percent annually.

Hughes Research Laboratories seeks highly-qualified scientists for advanced research in physics, chemistry and electronics. Disciplines include: Information sciences and artificial intelligence; space plasma sources; pulsed power switches; free electron lasers; electron beam testing; advanced IR detectors; liquid-crystal materials and displays; nonlinear optics and phase conjugation; computer architectures for image and signal processors; GaAs microwave devices and IC technology; and optoelectronic materials and devices. Send your resume to: Professional Staffing, Hughes Aircraft Company-Research Laboratories, Dept. S5, 3011 Malibu Canyon Road, Malibu, CA 90265. Equal opportunity employer. Proof of U.S. citizenship required for some positions.

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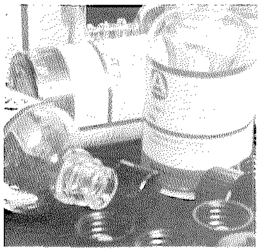

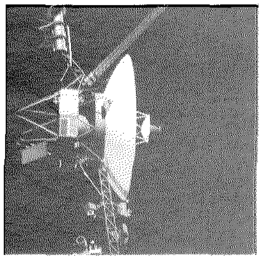

Departments

On the cover: Caltech's replica of the Pons-Fleishmann cold-fusion cell. Its glowing contents, however, are due to fluorescing dye, not fusing nuclei. Although the media blitz has died down, the science continues.

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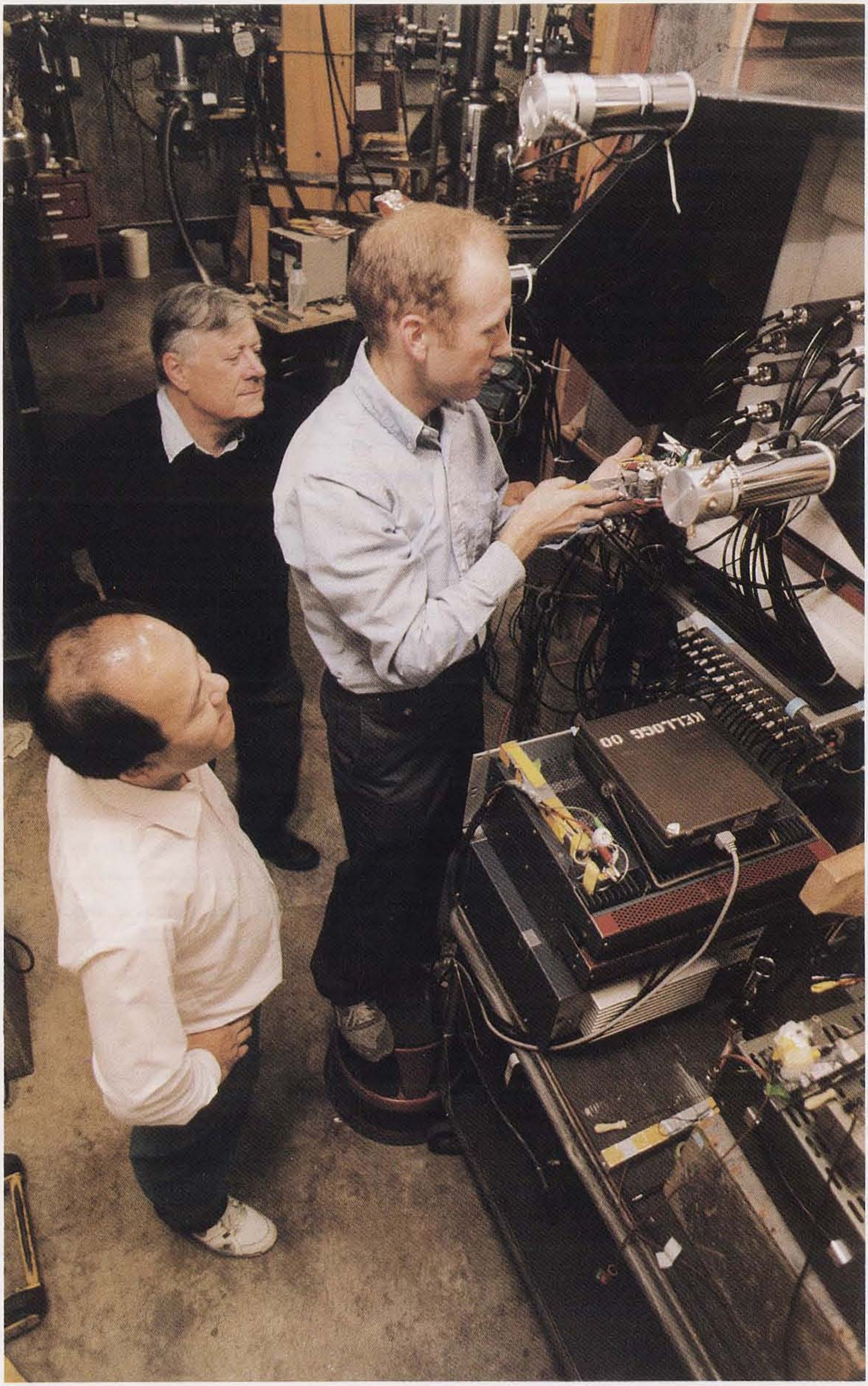

KELLOGG 00

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.

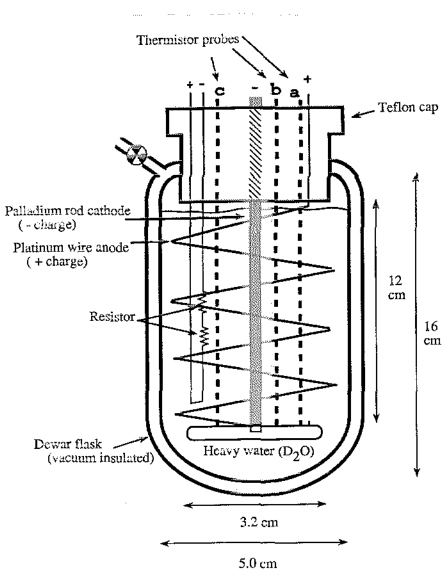
Things began innocently enough on Friday morning, March 24, when the *Los Angeles Times* ran a story on the previous day's announcement. Details were sketchy, but the key ingredients were palladium, heavy water (D_2O, water made with a form of hydrogen called deuterium, which has one proton and one neutron 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.

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

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

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

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

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

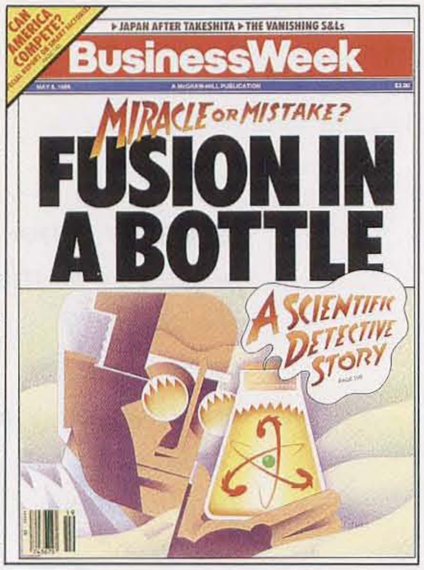
Left: The fusion cell. Right: Fusion reactions. H = hydrogen, D = deuterium, e^{+} = positron, ν_e = electron neutrino, He = helium, γ = gamma ray, T = tritium, n = neutron. The energy of each emission is in parentheses.

> *"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-



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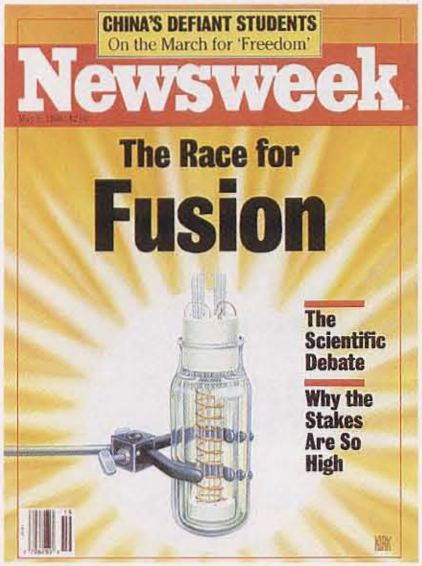

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"Over the weekend he convinced me that this thing might fly."

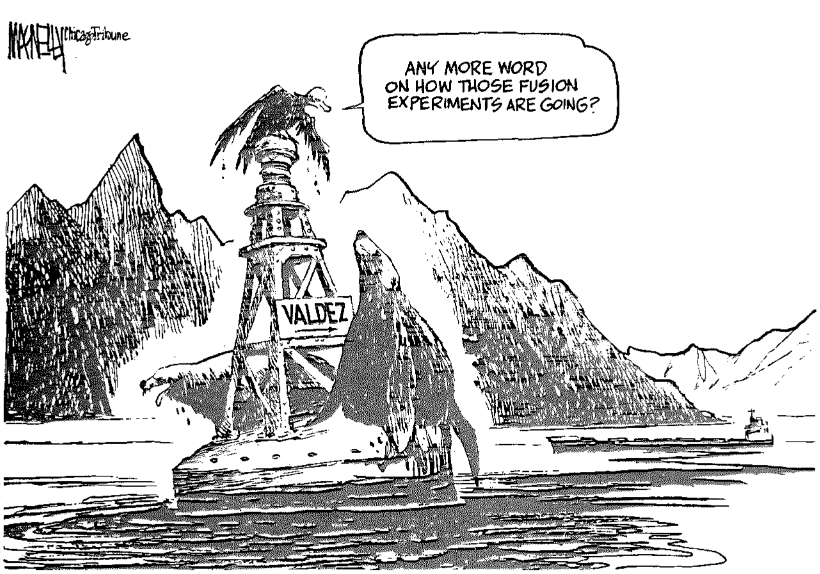
chemistry, and I was wondering whom to call in the chemistry department for help at about the same time that Nate Lewis's crowd decided they needed to find out how to measure neutrons. Anyway, Nate called me early Friday morning before I got around to calling anyone. It was natural that we teamed up. 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



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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.

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

"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."

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."

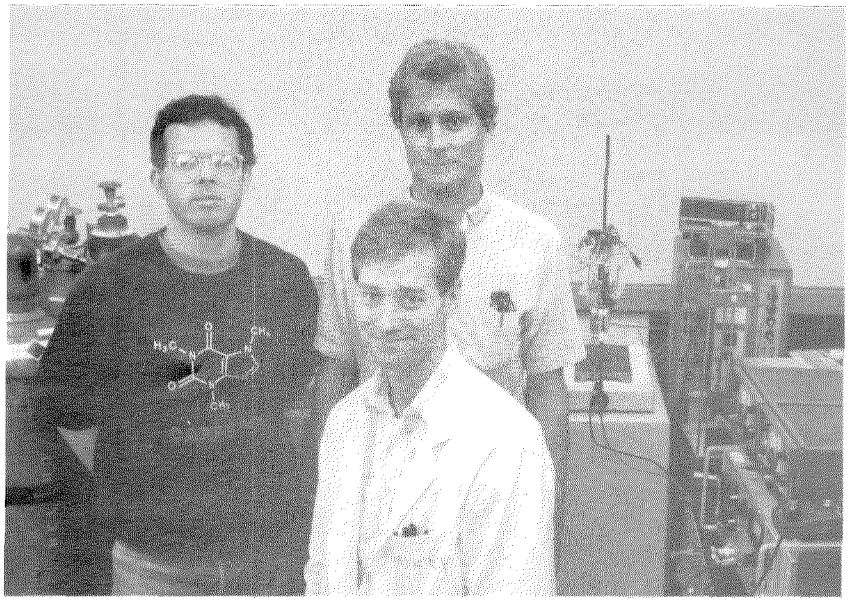
That same weekend the group discovered that the electrodes needed pretreatment. Graduate student Pat Santangelo had carefully read Fleischmann's old papers on hydrogen-deuterium separation, which used a setup similar to his fusion cell. These papers showed that he baked his palladium in a vacuum for 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.

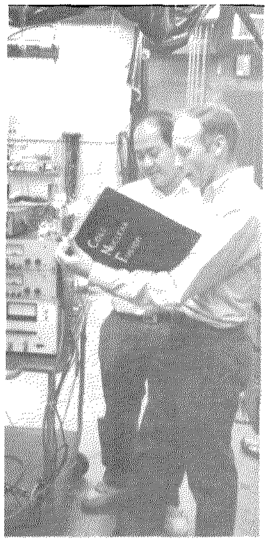
different view: "The chemists would get all excited as soon as we started a count. There's always some background—from traces of uranium in the detector tubes' steel walls, and other things that are well understood—so the counter always registered *something*. The screen updates the number of counts 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."

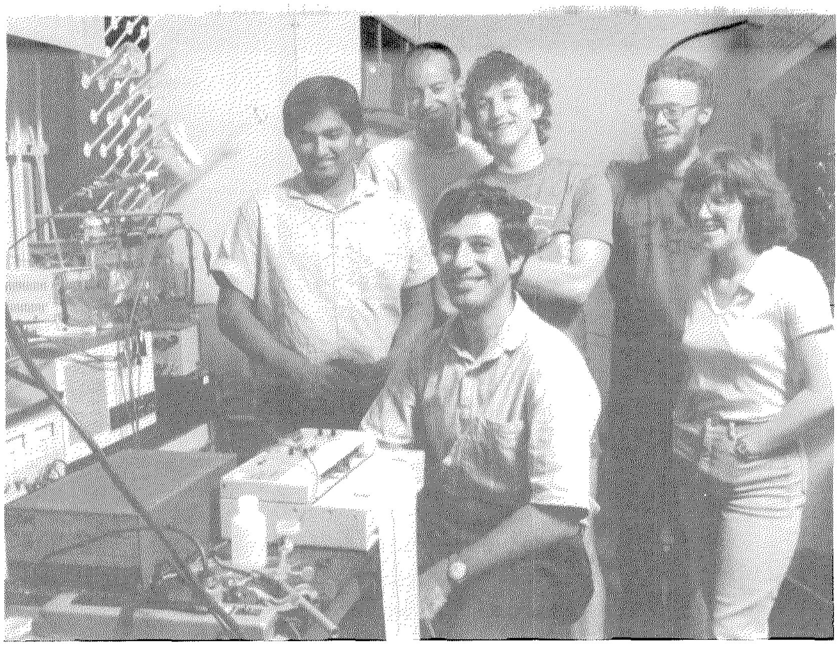
Refills were a bit tricky after hours, as the chemists' keys wouldn't open the physics building. Admits Penner, "We kept talking about getting a key to Kellogg, but we never got around to getting a requisition form. Once in the building, it usually wasn't a problem getting into the lab itself, because 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."

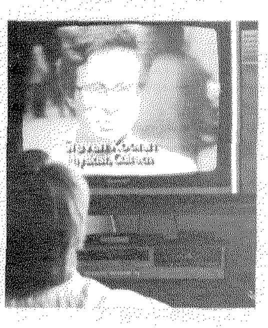
"Gordon was up late-late-late about three days straight getting data, right before the American Physical Society meeting in Baltimore," Sailor remembers. "Nate was going to speak there on May 1. We had almost all the figures, but he really wanted this data from the Pons-Fleischmann cell." (Lewis and 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*. . . .'

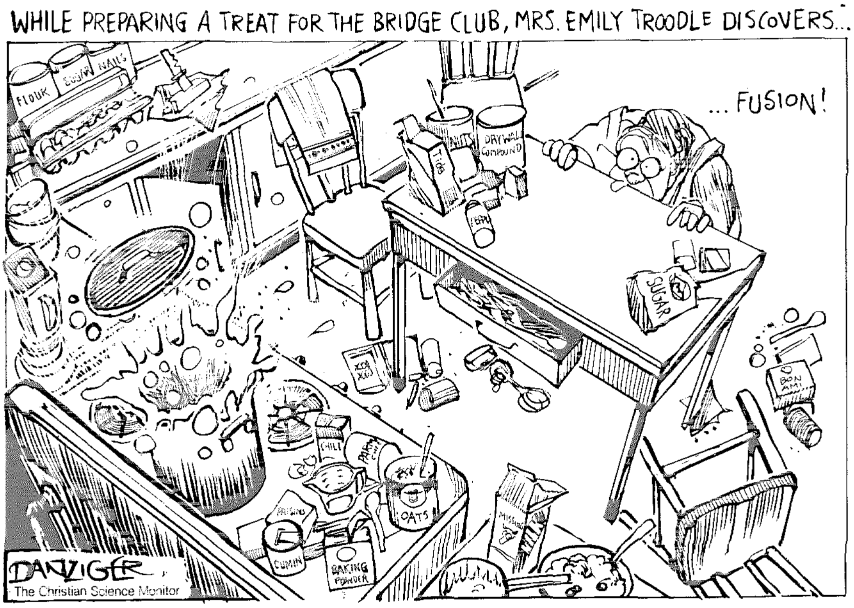
"One of the people who got the BITNET was Richard Garwin, a prominent physicist at IBM. He had been asked by the Italians to get some Americans together for a conference on cold fusion to be held in Erice, Sicily—a well-known physics center. He called me on Saturday, April 8, and early Monday morning 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."

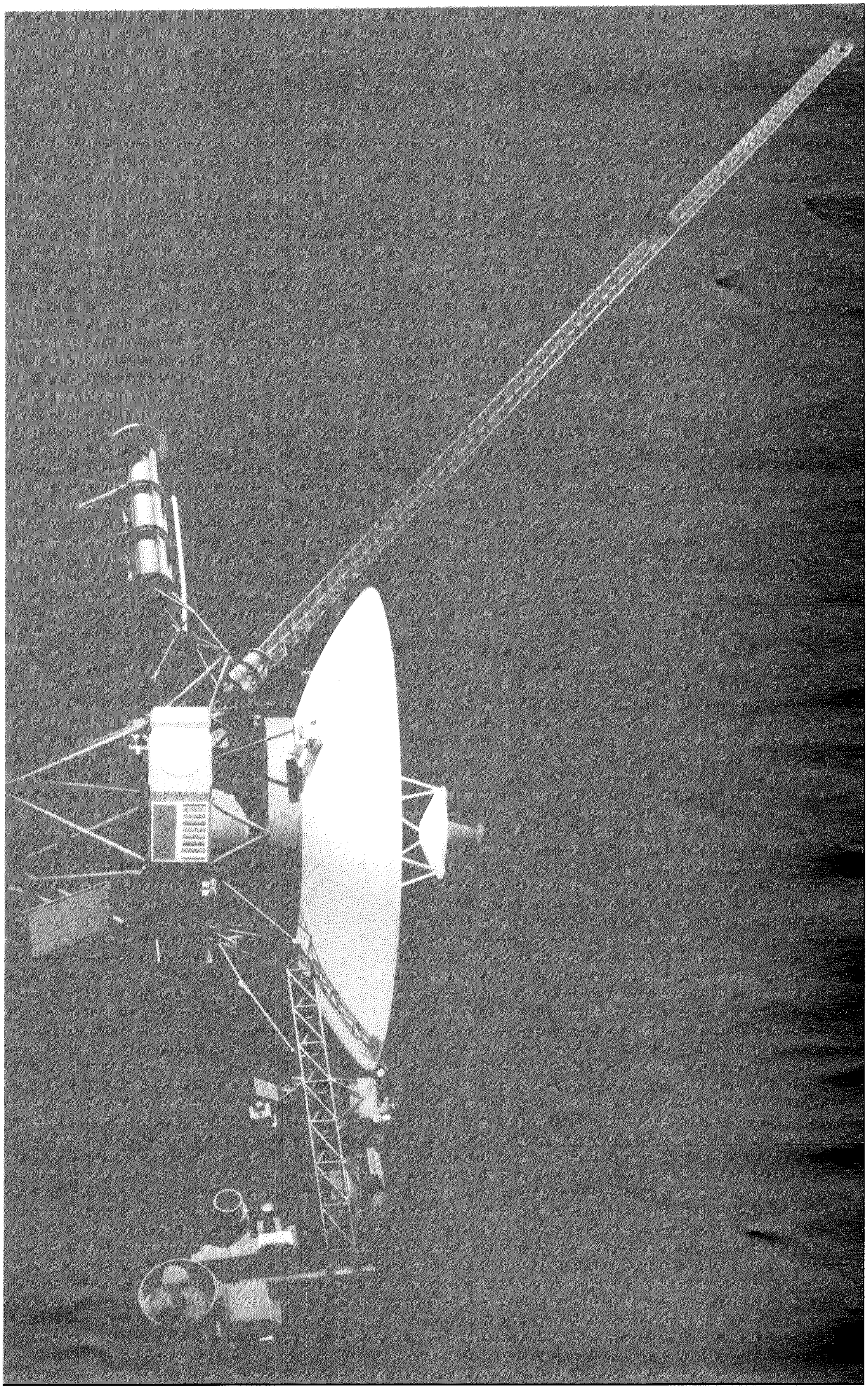
Things have quieted down since then. Most of the people have gone back to the research they put on hold. Some are still testing Jones's and others' results, including an Italian claim, confirmed by Los Alamos National Laboratory, of neutron production from titanium shavings in pressurized D_2 gas. Papers 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*



Voyager and the Grandest Tour Ever: Catching the Wave of the Century

by Bruce Murray

The Voyager spacecraft, the most productive robot explorer ever built, is now approaching Neptune, its final encounter in a spectacular tour of the solar system. But it had to survive political and technical frustrations before even getting off the ground a dozen years ago.

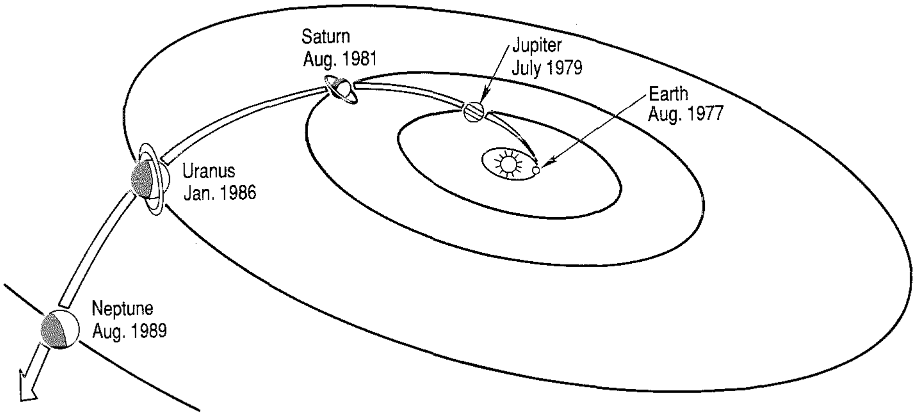
This chapter is excerpted from Journey into Space: The First Three Decades of Space Exploration *by Bruce Murray, published in July 1989. (Copyright © 1989 Bruce C. Murray) It's reprinted here with permission of the publisher, W. W. Norton & Company, Inc. Murray, a member of the Caltech faculty since 1960 and currently professor of planetary science, was director of the Jet Propulsion Laboratory from 1976 to 1982, a tenure that included the Viking landings on Mars and the Voyager encounters with Jupiter and Saturn.*

Imagine "Star Trek." An alien starship exploring our portion of the Milky Way trains high-powered sensors on a still-distant star of no apparent distinction. As the starship moves nearer, four cold, uninhabited planets come into view, orbiting majestically about the ball of burning hydrogen we call the sun. A few hours later (the starship is moving at nearly the speed of light), four rocky spheres are sighted orbiting much nearer the star than the four big planets. One of those is our home, the water-covered ball where we live, dream, and die. The starship aliens (or, more likely, their robotic surrogates) might soon streak on in search of a more interesting stellar environment, rather than expend fuel and time in a close-up inspection of insignificant rocky debris, orbiting a rather average star.

Those four conspicuous planets—Jupiter, Saturn, Uranus, and Neptune—are very large indeed. They constitute 99.8 percent of the aggregate planetary mass orbiting the sun. Viewed from interstellar space, these gas giants *are* the planetary system. Earth, however, is luckier. Tucked into a narrow zone of optimal solar warming, it has a moist surface, and because of that moisture life has evolved on it.

In ancient and medieval times it was fashionable to speak of the harmony of the spheres. The users of that phrase could not know how true that was. Jupiter, Saturn, Uranus, and Neptune *are* harmonious. Every century and three-quarters a magical alignment occurs among them. During this short-lived alignment a properly designed spacecraft launched to Jupiter from Earth can, with the right choice of close trajectory at Jupiter, break the sun's gravitational pull and fly on to Saturn, Uranus, and Neptune—the Grand Tour—and then, past Neptune, disappear forever into interstellar space. When that once-in-this-century opportunity occurred, in 1977, the United States was ready to ride that cosmic "wave" and make stunning discoveries about our giant planetary cousins.

James Van Allen, of the University of Iowa, was one of the world's first space scientists. In 1958 he discovered invisible, globe-girdling belts of lethal charged particles, belts that now bear his name. In the spring of 1970 he sat at the head of our conference table, chairing a scientific group brought together by NASA to explore the scientific opportunities offered by the Grand Tour trajectory discovered in the early 1960s. Of the 15 specialists meeting that day at JPL, I was the lone geologist, and the only member of the team especially interested in rocky worlds. My role rapidly became clear—to evangelize not just for good imaging but also for serious and



The alignments and motions of Earth and the outer planets very rarely coincide such that a spacecraft can be launched from Earth to Jupiter, be perturbed by Jupiter's gravity to move on to Saturn, and so forth all the way to Neptune. The last time the planets were aligned similarly for this trajectory, Thomas Jefferson was president of the United States.

On opposite page: Bruce Murray holds a model of Voyager. (Photo by Eric Myer)

During this short-lived alignment a properly designed spacecraft launched to Jupiter from Earth can, with the right choice of close trajectory at Jupiter, break the sun's gravitational pull and fly on to Saturn, Uranus, and Neptune.

close observation of the many satellites, the moons of these gas giants.

From Earth the moons of Jupiter and Saturn *look* insignificant, being no more than smudges on the photographic plates. Jupiter and Saturn dominate the telescopic views, physically and intellectually, presenting enormous challenges to the practitioners of physics. Jupiter's magnetic field challenges even the sun's. Intense radiation trapped within huge belts by that field generates the solar system's strongest radio signals. Jupiter's Great Red Spot, a giant vortex visible even from Earth, has persisted mysteriously over three centuries. Larger than Earth, that oval eye rules a colorful kaleidoscope of atmospheric swirls and bands, begging for theoretical interpretation. To be able to fathom the rings of Saturn would alone have justified the voyage. We were only a little wiser about those rings than Galileo was when he first described them.

In 1970, as we met at JPL, the first spaceships intended to scout Jupiter were already under construction only forty miles away at TRW. Called Pioneers 10 and 11, they were launched in 1972 and 1973, respectively, to sample Jupiter's environment—its electrical currents, magnetism, and energetic particles. Completely controlled by ground commands, the Pioneers spun passively in space like giant tops. These two spinning spacecraft were sturdily built to scout the gateway to the outer planets. They were mechanically and electrically simple and therefore relatively inexpensive. However, as every amateur photographer knows, imaging (and other remote sensing) is best carried out from a rock-steady and carefully pointed platform. Pioneers 10 and 11 did pack a small light-scanning sensor, from which a few images of Jupiter and Saturn were built up line by line by the spacecraft's spinning motion during the flyby. But no useful photography of Jupiter's or Saturn's moons was possible with such rudimentary equipment.

Pioneer 10 blazed a historic trail through the unknown hazards of the asteroid belt and extended humankind's reach to Jupiter on December 3, 1973, encountering far more damaging radiation than had been expected. Pioneer 11 reached Jupiter a year later, then blazed the trail past Saturn. In September 1979 it certified the safety of the narrow zone adjacent to Saturn's rings that leads on to Uranus and Neptune (just as Mariner/Venus/Mercury had found the tiny target adjacent to Venus that led on to Mercury). The success of Pioneer 11 silenced fears that unseen ring particles lurking there would pepper the unwary robotic explorer like a natural antisatellite weapon.

In 1970 those accomplishments of Pioneers 10 and 11 were still years away. Intense planning was taking place for the Grand Tour, for visiting all four giant planets in one flight. The Pioneer flights were overtures to the big show. For that next, giant step, new plutonium cells would be used to power the most intelligent robot ever conceived. A breakthrough in artificial intelligence (as it is now called) was essential for sophisticated exploration light hours from Earth. The state-of-the-art brain used on the Grand Tour would have to be as robust as the

control mechanism of a nuclear weapon in order to survive Jupiter's murderous radiation. That was the biggest problem, the hidden surcharge for the free ride to Saturn and on to Uranus and to Neptune.

As the months of 1970 passed, meetings proliferated. By October the small, dreary JPL conference room, crammed with engineering diagrams and plans, had become all too familiar. I was running one of Van Allen's subcommittees—the Data Handling Subcommittee for the Grand Tour Mission Definition Phase. Jupiter is half a billion miles away. Radioing back thousands of pictures and other data to Earth would challenge every link in the communications chain—the spacecraft, the Deep Space net, and the ground computers. But it could be done. The Grand Tour was no economy-class mission like the flights to Venus and Mercury. Expensive new technology was to be its hallmark. I experienced déjà vu, recalling nearly a decade of previous meetings for Mars and Mercury missions at which we had labored over the same technical issues.

Suddenly, a revelation hit me. Three successive missions to Mars, plus the upcoming flyby of Venus and Mercury, were enough for me. Other people could convert the promise of the Grand Tour into reality. After MVM, I would not return again to the "trenches" but would move on to something new.

Meanwhile, the Grand Tour seemed to have a sound basis for governmental approval, for it offered the following:

- Great scientific promise for countless physicists, astronomers, meteorologists, and geologists (although official scientific support still reflected a concern that the Grand Tour might suck up a disproportionate share of space resources).
- The cutting edge of space technology, to challenge JPL once again.
- The prospect of historic American achievement, clearly beyond Soviet capability, certain to elicit worldwide acclaim.
- A billion-dollar, high-profile program for NASA, well suited to pick up the budget slack after Viking.

NASA promoted the Grand Tour concept strongly. In April 1971 it charged nearly a hundred diverse scientists to put scientific flesh on the concept Van Allen's group had laid out.

Over the next seven months two optimal trajectories were identified. The first involved a blast-off in August 1977 toward Jupiter, where the spacecraft was to catch the free trip to Saturn, Uranus, and Neptune. A few weeks later, the second spacecraft would be launched, also to Jupiter and Saturn. But the Grand Tour gateway to Uranus and Neptune would already have closed. Tiny Pluto would therefore become Number 2's final destination. Fifty-four separate scientific objectives for the Grand Tour were spelled out, but only eight even mentioned the satellites, and only two did so uniquely. For most of the scientists involved, those dozens of little worlds orbiting the gas giants remained insignificant smudges on photographic plates. Geographical discovery—the progenitor of so much natural science on Earth—was not fashionable with the laboratory physicists, chemists, and biologists who dominated American (and Soviet) science.

But Bud Schurmeier, JPL's new project manager, understood the importance of photographic exploration. Schurmeier had put JPL and America into the lunar race in 1964 when his Ranger lunar-impact probes radioed back the first close-up pictures of the moon. This was a crucial preparatory step for Apollo. In 1969 he led the work on Mariners 6 and 7 that achieved a hundredfold gain over tiny Mariner 4 in the return of pictures from Mars. With Schurmeier, JPL was clearly giving the Grand Tour its best effort. The exploration of even those unknown moons would now receive a sympathetic hearing.

Events moved rapidly in late 1971 and early 1972.

- On August 3, 1971, Dave Scott, the test pilot Chuck Yeager's protégé, and his Apollo 15 crew took viewers all over the world on a roving television ride across the rocky and forbidding lunar landscape.

- On November 12, 1971, Mariner 9 (launched in March of that year) eased into Mars orbit to wait out the dust storm before astounding us with images of mountains, canyons, and valleys larger than any on Earth.
- On January 5, 1972, President Nixon announced the beginning of a new era in space with the development of the Space Shuttle.
- On January 11, 1972, NASA notified JPL that the Grand Tour project had been killed for budgetary reasons.

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The Apollo momentum that had powered a generation of virtuoso American planetary first looks was fast dissipating. The Grand Tour simply reached the starting gate too late for a new planetary endeavor of such cost.

JPL responded to this situation by formulating a cheaper, less ambitious alternative. Within days Bud Schurmeier and his engineering team transfused a decade of hard-won experience in space exploration into an attractive new engineering concept for a rich comparative examination of Jupiter and Saturn and of their moons. It was modestly termed Mariner/Jupiter/Saturn. MJS matched the most promising scientific objectives at Jupiter and Saturn with capabilities already largely proven by previous Mariners, by Pioneers 10 and 11, and by JPL's large new Viking orbiter development then under way.

A greatly enhanced electronic brain, however, remained to be developed because MJS needed an exceptionally good memory and great intelligence. But Schurmeier and his project were more than five years away from launch, almost twice as long a lead time as earlier Mariner and Pioneer developments had enjoyed.

An immediate challenge loomed. MJS would have to be sold to the Nixon administration practically overnight. Just how fast, I learned in February 1972, in a telephone call from the Old Executive Office Building, adjacent to the White House.

"There are questions around here about Mariner/Jupiter/Saturn, Bruce. Any comments?" was the cryptic opening from an old friend, Russell Drew.

He was alerting me from within Nixon's secretive staff deliberations that MJS needed more justification in Washington. Drew, a naval aviator with a PhD in electrical engineering, and an active-duty captain in the Navy, had joined the science adviser's staff in 1966, swelling the civilian-garbed brigade of military officers that inconspicuously strengthens the White House's staff.

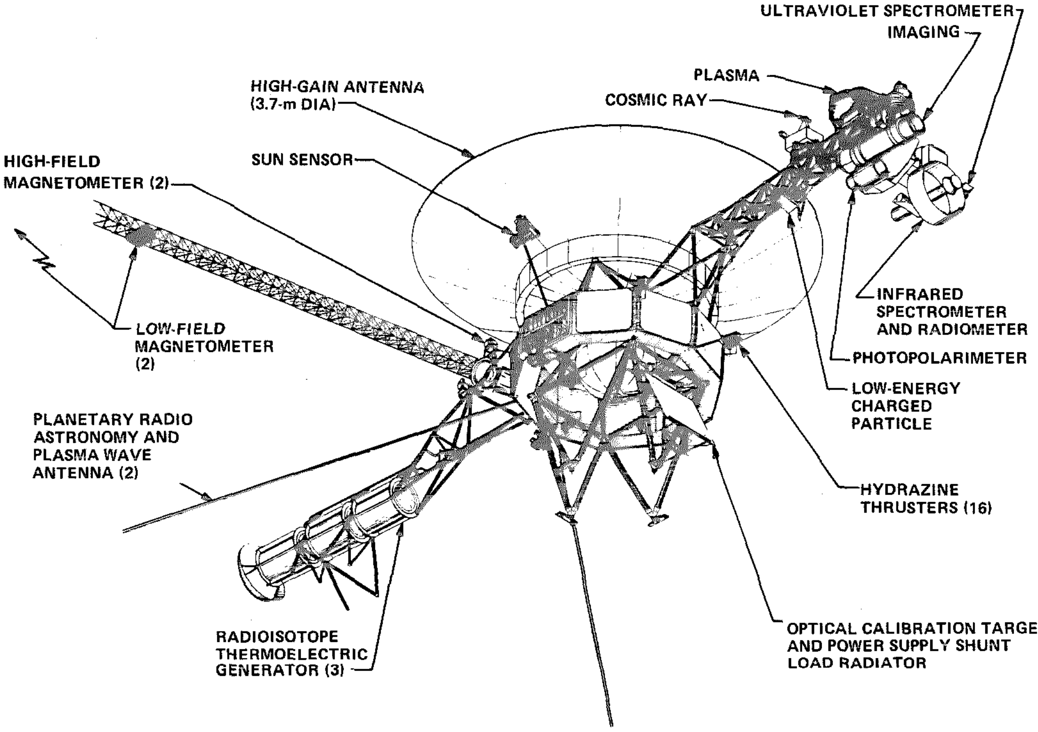
By February 1972 and our off-the-record phone conversation, the President's Science Advisory Council (PSAC), which Russ had ably served, had been reined in. (It would be eliminated altogether in January 1973.) But Drew still cultivated his old PSAC network, quietly funneling outside expert opinion into the budgetary deliberations of the executive branch. Trying to neutralize the damaging scientific opposition that had helped kill the Grand Tour, I told him that the Mariner-class investigation of Jupiter and Saturn was more promising scientifically than any new mission I could think of. The Space Science Board (SSB) of the National Academy of Sciences had sensed another expensive Viking-scale mission that would threaten other space science projects.

"It could be incredibly visual and popular," I said. "And once the Uranus, Neptune, and Pluto requirements are dropped, the mission is more within reach technically, and much cheaper." Then I pointed Russ toward what I hoped was still a key factor at the Nixon White House, despite the national ebbing of interest following the first Apollo landings: "It's certainly the most cost-effective space competition with the Soviets imaginable."

He knew from the secret PSAC briefings, as I did, that, despite the massive Mars and Venus efforts of the Soviets, Jupiter—let alone Saturn—was way beyond their reach. "Very interesting," chuckled the always cheerful Drew as the telephone clicked off.

Broad support for MJS coalesced rapidly. On February 22, 1972, the Space Science Board endorsed MJS as first-rate science (and much more affordable than the Grand Tour). NASA, Congress, and the Office of Management and Budget (OMB) waived their usual procedures in order to consider MJS right in the middle of the annual budget cycle. NASA approved JPL's MJS proposal officially on May 18, just a month before brief press reports mentioned that five men had been arrested for breaking into Democratic National Headquarters in the Watergate Office Building.

In fact, congressional and OMB support for MJS was so strong that an additional $7 million was added to the MJS appropriation for scientific and technological enhancements. The new autonomous electronic brain would now be made reprogrammable to an unprecedented degree. Moreover, some flashy new computer tricks would be built into the spacecraft to assure deep-space communications even under drastic conditions. A new, more efficient computer-driven attitude-control system could also be counted on. No one imagined then that these extra "mental" capacities, intended to assure



Voyagers 1 and 2 had identical structure and instrumentation.

that MJS got safely to Jupiter and Saturn, would first almost disable the spacecraft at launch and subsequently enable it to explore Uranus and Neptune.

At that time neither Uranus nor Neptune figured seriously in MJS's plans. Far from the sun, Uranus receives only 1/400 as much sunlight as Earth. It is *really* cold and dark on Uranus. Few of the remote-sensing instruments designed for Voyager, as MJS was called after 1975, were expected to yield much at Uranus, let alone at Neptune, a billion miles farther yet. In addition, more immediate problems were showing up in tests of that brilliant, autonomous electronic brain. JPL was learning the hard way that it is easier to build complex computer programs and systems than to test them adequately for the unforgiving space environment. But technical challenges are the stuff good engineers thrive on. Most important, MJS had gotten started and was on the way to becoming the most productive robot explorer ever built.

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Things were going less well for JPL as an institution, however.

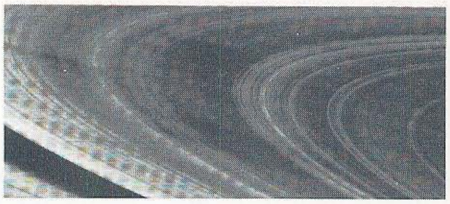
NASA, JPL's sole sponsor, lacked the presidential political support that it had enjoyed before the Apollo moon landings. Since then the agency had steadily declined in national clout and technical capacity. The challenging and uplifting Apollo project had become a glorious, almost mythic, memory for NASA, rather than a gateway to the future. JPL was NASA's only center not staffed by government employees. That made it a natural target for elimination in tough times, a circumstance that added to the "creative tension" inherent in the three-sided relationship between NASA, JPL, and Caltech. Furthermore, JPL had no important role in the post-Apollo focus on the Shuttle and on related uses of astronauts in low-Earth orbit. In fact, JPL seemed to be on everybody's rumored "hit list," awaiting the next big NASA cut. Russ Drew sometimes dropped alarming hints to me of draconian measures under discussion at OMB that would have eliminated JPL.

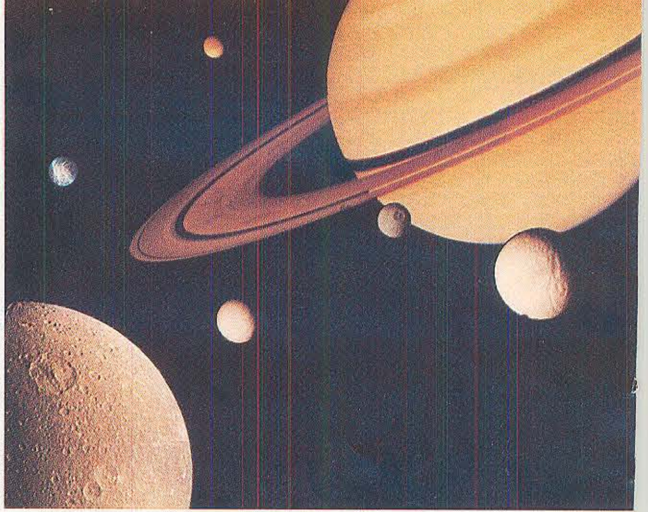
Nevertheless, such alarmism had seemed to me unwarranted in June of 1975, when I flew to Washington for my first official visit with NASA Administrator James Fletcher. Harold Brown, Caltech's president (and soon to be Jimmy Carter's secretary of defense), was waiting for Fletcher's approval before he would announce publicly that I was to succeed William Pickering as director of JPL.

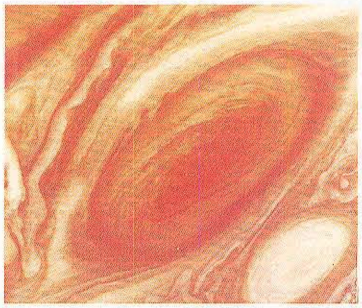
Affable as always, Fletcher had only one substantive question for me. "Bruce," he said, "How do you feel about JPL working for the Defense Department?"

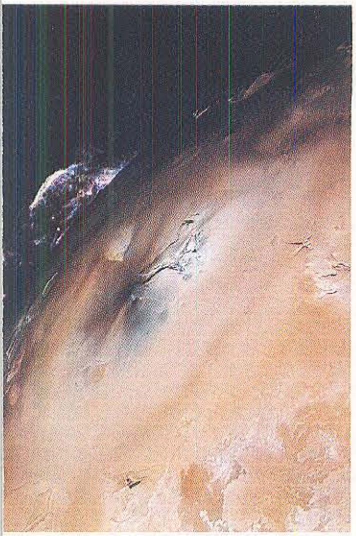
JPL had done no significant military work since 1958, when it became a charter member of the NASA-led civilian space program. I had no personal problem with future JPL defense work, but what a strange question for the head of the American civilian space program to raise! And Fletcher had nothing at all to say to me about planetary exploration, even though he kept prophesying publicly that the 1980s would be "a golden age of planetary exploration."

The message was clear: NASA, a declining institution, felt it could not support JPL as it









What Voyager saw—from left: geyser plume on Io, one of Jupiter's moons (1979); Jupiter's Great Red Spot (1979); the rotating spokes on Saturn's B-ring (1980); a montage of Voyager 1 images of Saturn and its moons (1980); the rings of Uranus (1986); and Ariel, a Uranian moon (1986). Also on this page: Voyager scientists receive the first polarimeter data on Saturn's rings; and on the opposite page, project scientist Ed Stone talks to the media.

had during the halcyon days of the building of America's first satellite, first moon probes, and first planetary explorers.

Unlike my predecessor Pickering, I would thus have a twofold task as director of JPL—to push U.S. planetary exploration to the hilt and, at the same time, to develop a second governmental role for JPL that would be independent of NASA. In 1976, following the oil-price shocks of 1973, government-supported energy research and development was the best (really the *only*) practical alternative for a high-tech, nonprofit, civilian-oriented place like JPL.

So, on April 1, 1976, my first day as director of JPL, I promoted Bud Schurmeier with great fanfare and with instructions to carve out a meaningful role for JPL in energy R & D. Neither Bud nor I, as it turned out, would find deep satisfaction in these new strategic responsibilities. The ebbing of the old Apollo spirit was merging with a broader retreat of American self-expectations in the face of failures in Vietnam and at home. America would not blaze new paths in either alternative energy or planetary exploration. But in 1976 we had to try to create the kind of future that JPL, and America, deserved.

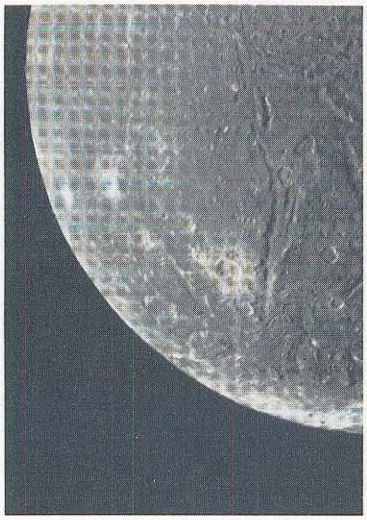
Schurmeier's replacement to head Voyager was John Casani, who at last had his own project. He loved every minute of it—at least until August 20, 1977, the date of the first Voyager launch at Cape Canaveral.

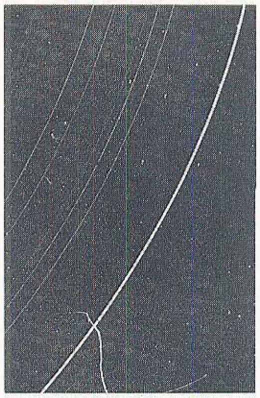
The day before that launch Suzanne and I stood on top of Pad 41 at Cape Canaveral, 160 feet up. The great red gantry tower clutched Titan/Centaur Number 5. Flickers of Florida lightning punctuated the sun-and-rain-washed panorama. The lightning was still far away, but if those scattered thunderstorms came within a mile and a half of us, we and everyone else would have to get out of there fast. We were standing on nearly 700 tons of high explosives.

Beneath us, twin 250-ton, solid-fuel boosters stood ready to power the Titan/Centaur, the largest existing U.S. launch vehicle, off the pad the next morning. The boosters supported a 70-foot-long, two-stage Titan rocket originally designed to fling a city-destroying nuclear bomb. Tomorrow's purpose was sublime in contrast. Two liquid propellants tanked separately within the Titan would mix precisely in a delicately sustained spontaneous explosion lasting six and a half minutes. These rocket firings of the Titan would thrust the graceful Centaur stage and its heavy Voyager cargo through Earth's atmosphere and part way into Earth-circling orbit. Then the intelligent Centaur stage would take charge.

As we gingerly descended the winding stairway of the gantry tower on that glorious Florida afternoon, a quick glimpse of a busy engineer working *inside* the Centaur surprised me. It was a reminder that unmanned rockets do indeed fly by the skill and dedication of humans—on the ground. The engineer was checking this giant's intricate computer brain.

At launch, delicate instruments at the very tip of the Centaur stage would sense precisely the rumbling flight motion produced by the noisy solids and the storable-liquid Titan rocket. As soon as the second Titan stage burned out and





fell away, Centaur's electronic brain would ignite the liquid hydrogen–liquid oxygen mixture of the Centaur high-performance engine.

Then would come the brilliant part. The Centaur's electronic brain would automatically compensate for any shortfall in the propulsion of the earlier stage. It would "fly" the Centaur and its payload precisely into a predetermined low-Earth orbit. The Centaur brain would then shut down the engine temporarily while the rocket, with its remaining fuel and the attached planetary spacecraft, would coast along in that orbit for the tens of minutes required to reach a precise location for that day's planetary departure. At that point, out over the Atlantic, liquid hydrogen–liquid oxygen would be reignited by the brain in the cold silence of space to enable the craft to break free of Earth's gravity and to build up most of the final velocity that would propel it to another planet. Then the final burst of velocity to reach Jupiter would be supplied by yet another rocket stage, fastened directly to the Voyager spacecraft.

Throughout these eventful minutes, Centaur's brain would measure thrust and calculate flight path. At the exact second when the correct velocity was reached, the brain would shut off the Centaur rocket engine forever. Explosives would then separate the precious spacecraft from the now useless Centaur.

This is what is meant by a smart machine. Inside it, working intently, was the even smarter engineer who understood it all.

We continued down the gradually widening gantry tower in the yellowing afternoon, past dormant liquid and solid explosives ready to send Voyager 2 into history the next morning. The lightning drifted harmlessly seaward into a darkening sky.

Why launch Voyager 2 before Voyager 1? The decision had to do with keeping open the possibility of carrying out part of the old Grand Tour dream. Voyager 2, if boosted by the maximum performance from the Titan/Centaur, could just barely catch the old Grand Tour trajectory. In that way the "Uranus option" from Grand Tour days could be maintained. Two weeks later Voyager 1 would leave on an easier and much faster trajectory, to visit Jupiter and Saturn only. Voyager 1 would make up time and reach Jupiter four months ahead of Voyager 2 and then go on to arrive at Saturn nine months earlier. (That's why it was called Voyager 1.) The nine-month separation between the arrivals at Saturn ensured that if Voyager 1 failed in its Saturn objectives, Voyager 2 could still be retargeted to achieve them—but at the expense of any subsequent Uranus or Neptune encounter.

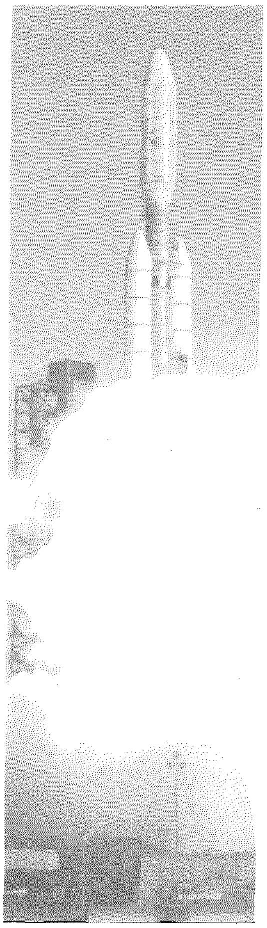
At 10:29 a.m., August 20, 1977, the blast from the twin solids and the Titan core shattered the clear blue Florida morning. Exactly as planned, the Titan/Centaur powered Voyager 2 into Earth orbit, and thence on a path toward the outer planets.

Voyager 2's gyroscopes and electronic brain were alive during the Titan/Centaur launch, monitoring the sequence of events in order to take control upon separation. But here the unexpected happened: Voyager 2's brain experienced robotic "vertigo." In its confusion, it helplessly switched to backup sensors, presuming its "senses" to be defective. Still no relief from its disorientation. Mercifully, the panicky robot brain remained disconnected from Voyager's powerful thrusters, so it did not cause damage to the launch. The Centaur attitude-control system—under its normally behaving brain—stayed in charge, suffering no "vertigo" and, as planned, electronically correcting the disequilibrium of Voyager's brain just before separation.

From the control center John Casani and his tense engineers helplessly watched (though mostly they listened, because there were not enough monitors available to us in Florida) the antics of Voyager 2's disoriented brain. One hour and 11 minutes after lift-off, Voyager 2 fired for 45 seconds its own special solid rocket to provide the final push it needed to get to Jupiter.

One and a half minutes after Voyager's key rocket burn ended, a ten-foot arm holding the

If the two Titan/Centaur rockets for Voyager had been fired in reverse order . . . Voyager 2 would not have received enough velocity to catch the Grand Tour trajectory.



A Titan/Centaur rocket launches Voyager 2 on August 20, 1977. Shortly after launch Voyager's electronic brain became disoriented, and the spacecraft almost didn't even begin its historic journey.

television camera and other remote-sensing instruments was unlatched and deployed as planned. Then, more trouble. Voyager's anxious brain once again sensed an emergency. This time it switched thrusters and actuated valves to control the tiny bursts of gas used to stabilize its orientation. Voyager's robotic "alter ego" (its executive program) then challenged portions of its own brain in a frantic attempt to correct the orientation failure it sensed. Next, Voyager followed the procedures JPL engineers had installed to cope with the most dreaded emergency for a robot in deep space—spacecraft attitude disorientation. (In August 1988 the Phobos 1 spacecraft of the Soviet Union succumbed to such an emergency after receiving an erroneous ground command, and in March 1989 Phobos 2 evidently met a similar fate.) Voyager shut down most communications with Earth in order to begin its reorientation.

Seventy-nine minutes passed while Voyager 2 struggled alone and unaided to find the sun and establish a known orientation. Finally, it radioed confirming data. For the moment, Voyager 2 was stable.

It was all work and no celebration that afternoon in the dimly lit High Bay Conference Room, where, just days earlier, a seemingly healthy Voyager 2 had checked out perfectly. Were the redundant sensors malfunctioning? Was the state-of-the-art brain defective?

The technical discussion in the room was poorly illuminated too. All the new, super-sophisticated fault protection in Voyager's electronic brain operated on the now-painful presumption that it would be triggered *only* by a hardware failure billions of miles from Earth. In that event Voyager would be unable to establish even emergency communications with its human handlers, who could not help it much at that distance in any case. As a consequence Voyager had been programmed virtually to shut off communications with Earth during such emergencies and to fix itself. But, somehow, these deep-space procedures had been triggered right after the launch.

Now, because of those disrupted communications, we were not receiving the useful flow of engineering-status measurements. We simply lacked enough information to figure out the causes of Voyager's mysterious behavior, even though the spacecraft was so close to Earth that communications normally would have been feasible under any emergency.

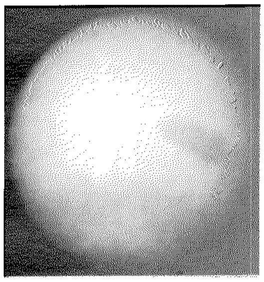
Voyager was proving to be far more autonomous than anyone had foreseen or wished.

Casani's frustrated specialists substituted coffee for sleep in that dark and dreary High Bay area. They crowded around the solitary speaker phone while reviewing the skimpy facts with their equally puzzled colleagues back at JPL. Even as they spoke, Voyager experienced yet another spasm of thruster firing, accompanied by frenzied switching of its redundant brain components. What was going on out there? Had Voyager's massive propulsion module, so gently jettisoned after launch, continued as a ghostly companion on Voyager's trajectory? Was it actually bumping into Voyager from time to time?

Ted Kopf, an attitude-control specialist at JPL, had designed some of that intricate computer logic now running rampant on Voyager 2. He had come to the Florida launch on his own hook for pleasure, not work. But that was before the emergency. I listened now as Kopf and a brilliant JPL systems engineer, Chris Jones, served up precise clues afforded by Voyager 2's scattered radio transmissions. So detailed were their mental images of that complex Voyager brain that they could reconstruct each stage of its successive anxiety attacks since the launch. There had been no hardware problems in the brain—just a slight but serious missetting of computer parameters.

Subsequent detective work shed light on the computer problems first encountered after rocket separation. These were complicated by unexpectedly large vibrations from the unlatching of the instrument boom. There had been no "bumps in the night" from a ghostly companion. Those spacecraft disturbances were simply a complex and overly sensitive reaction by the autonomous Voyager to a familiar space-engineering problem. Small dust particles released by the vibrations from rocket propulsion sometimes drift near a spacecraft. Being close, and being lit by the sun, they are much brighter than the star images that the spacecraft's optical detector normally tracks. In trying to follow the dust particles, the tracker reorients the spacecraft.

A lot of robotic technology and human expertise had accumulated over the years, ensuring that situations like this would be recognized and compensated for. But this new, superautonomous robot did not have that repertoire of human judgment and experience built into it. So it mistrusted itself but was finally righted by its own logic.



Coming soon: Neptune, August 25, 1989.

This could not be allowed to happen again. Desperately, corrections were patched onto the computer programs in Voyager 1, now waiting to be entombed for launch, even as the analysis of Voyager 2 stumbled along. A new mechanism to reduce the excessive vibrations from the instrument boom was designed, tested, and installed. Time was running out.

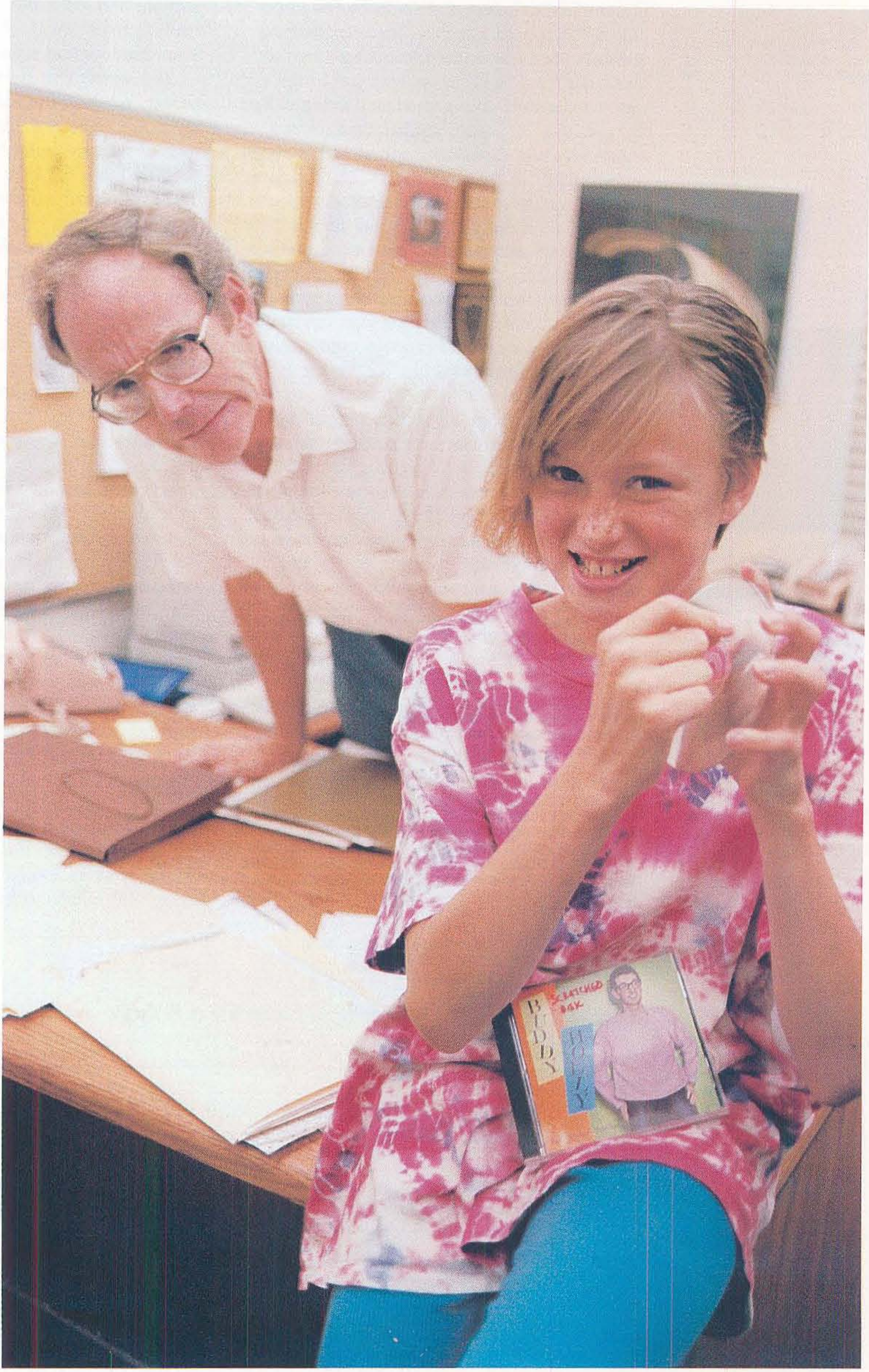
Sixteen days after Voyager 2's heart-stopping launch, on September 5, 1977, Voyager 1 flew. The last Titan/Centaur in the world shook the ground for miles around Pad 41. NASA had phased out America's most powerful rocket long before a replacement capability, the Shuttle, would be ready. (Indeed, not until the early 1990s will comparable rocket capability be available for American space endeavors, civilian or military—a gap of nearly 15 years.)

Our attention in the control room was riveted on the continued radio updating describing the execution of key rocket sequences. "Solid booster burnout and separation," a voice over the control room intercom said. "Titan core ignition . . . Titan burnout and separation."

But wasn't that sooner than shown on the projection? Yes, there definitely had been an anomaly. In fact, a slight error in the mixture ratio of the two liquid fuels in the Titan had left 1,200 pounds unburned. This was serious. Titan had underperformed, not propelling Centaur and Voyager fast enough. "Centaur ignition," came the words from the public address system. The Centaur's brain, recognizing the Titan performance deficiency, smoothly extended its burn just enough to compensate. After coasting, and then restarting, the Centaur put Voyager and its propulsion module precisely on course—with only a squeaky 3.4 seconds of propulsion left.

"Wow, that was pretty close," I thought at the time. It was only later, over a beer in Cocoa Beach, that I realized that if the two Titan/Centaur rockets for Voyager had been fired in reverse order, if the underperforming one had been used for the more demanding trajectory, Voyager 2 would not have received enough velocity to catch the Grand Tour trajectory. It would have reached only Jupiter and Saturn. The opportunity of the century would have passed us by (even though the rich Jupiter/Saturn comparison would still have taken place). Pure chance had assigned the unexpectedly low-performing Titan to Voyager 1 rather than to the more demanding launch of Voyager 2.

But in September of 1977 it was hard to take seriously any hypothetical extension to Uranus, 2 billion miles from Earth. We were not even off to a good start toward Jupiter, a mere 500 million miles and 18 months away. ☐


BUDDY
HOLLY
SCRATCHED
BAK

Safety in Numbers: Protecting Data Mathemagically

by Robert J. McEliece

Lizzy McEliece scratches a favorite compact disk with a paper clip as her father winces in mock horror—although he knows that error correction will save his music. Unfortunately, this time excessive scratching zeal finally did in this CD, which had, however, performed beautifully after a similar demonstration at the Watson Lecture.

They say that people with emotional problems become psychologists, sick children grow up to become doctors, people with bad teeth become dentists, and so on. Well, when I was little, I made a lot of careless mistakes: I could not read my own handwriting; I made spelling mistakes; I forgot to carry when I multiplied; when I tried to play chess, I always overlooked something. But after my junior year at Caltech, I got a summer job at JPL, where I found out that there was a way to use mathematics to correct errors automatically! For some reason, this subject fascinated me immediately. I learned all about it, and, as it turned out, the theory of error correction became a big part of my life's work. I still make a lot of errors, but now I get paid for correcting them. (And I get paid very nicely, thank you.) So, in this article I'd like to explain some things about the theory of error correction—why I think it's a lot of fun, and how it plays a big part in the design of a lot of today's communication and data-storage systems.

Everyone is already pretty good at error correction, in English at least. Below is a five-letter English word, in which one of the letters has been erased and replaced with a question mark:

Q?EEN

That's not so hard, is it? Okay, here's a slightly harder one—a five-letter word in which there's an error. One of the letters has been changed to another:

TRADF

Well, maybe that wasn't too hard either. But you should congratulate yourself anyway, because a computer has a much harder time than you do solving problems like this. In fact, no one knows exactly how humans correct errors in English. As a rule, though, it's about twice as hard to correct an error as an erasure.

Here are five more words to try. Change one letter and make a common English word. (Don't spend too much time on this or you'll never get on with this story. The answers are on page 36.)

THARA
ADAST
TRENA
NEMVE
SPLIR

If you got a couple or all of them, you can see that error correction in English is possible, even if it's not always so easy. It's possible because there is a subtle and complex pattern to the way words and sentences are constructed in English. Everybody knows that Q is always followed by U. That's a pattern, and even if the pattern is partially destroyed (by erasing the U, for example), the word is still recognizable. Many words end with the pattern: *vowel, consonant, silent E.* Such words as *date, stalactite, remote,* or *cute*—or *trade*—illustrate this pattern. And even if the silent E is changed to F or something, it's still pretty easy to see the pattern and tell what the word is. In the last five words, however, the pattern isn't as strong, and that's why it's harder.

Sometimes the patterns in English aren't good enough. Here's another five-letter word with one erased letter:

S?OUT

There is no one right answer to this one, because there are at least five possible answers (shown at left).

SCOUT
SHOUT
SNOUT
SPOUT
STOUT

The problem is that these five words are too close to each other; they differ from each other by only one letter, and if the second letter is erased, the word is lost. You can probably think of lots of other examples. Can you find a word of five or more letters such that if you erase one letter, there are six or more possible completions? (One suggestion appears on page 36.) With errors instead of erasures, the situation can be even more complex. Here's a five-letter word in which one of the letters has been changed:

GLADE

Well, GLADE is already a word, and there are (at least) five more words (shown at left) that we can get from it by changing just one letter.

BLADE
GRADE
GLIDE
GLARE
GLADS

You can see that GLADE is rather sensitive to possible typos. In fact, in 1879 Lewis Carroll invented a word game, called "doublets," based on the fact that many pairs of words differ by only one letter. For example, we can change BLACK into WHITE by making a sequence of one-letter changes. If you want to play "doublets," you might try turning LEAD into GOLD. (See page 36.)

BLACK
SLACK
STACK
STALK
STALE
SHALE
WHALE
WHILE
WHITE

So English has quite a lot of built-in error-correcting ability because of its natural patterns. But it wasn't designed systematically to correct errors, and, as we have seen, sometimes changing just one letter in a word can dramatically change its meaning. Of course, it is just this wonderful ambiguity that lets us play word games, commit horrible puns, write complex poetry, and invent pseudo-words like "mathemagically." I don't advocate changing the English language.

But suppose we did want to design a very precise language for absolutely reliable communication of important information (air-traffic control, military commands, deep-space communication, and so on). It turns out that we can do much better than English—no fun with word games, no clever poetry, but a much better ability to recover from errors.

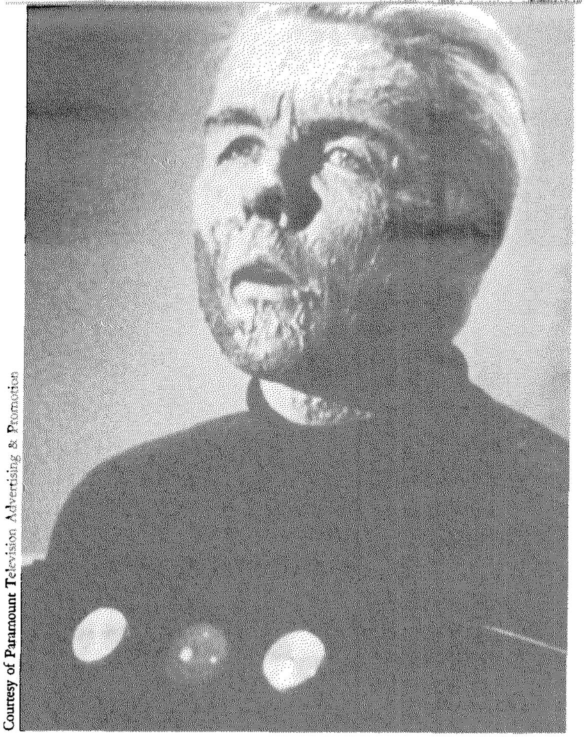
The first thing to know, if you want to design a new and improved language for communicating reliably despite errors, is that you don't have to use a 26-letter alphabet. (It's like Wilbur and Orville Wright designing the airplane: they looked at the birds for ideas, but they didn't have to copy slavishly. Birds have wings; that's a good idea. The wings flap; that's not such a good idea. Airplanes have things that spin around on their noses; they don't have feathers, and so on. We can steal ideas from nature if we want to, but only if we want to.) In fact, an alphabet with only two letters is sufficient, and, as you may know, many modern communication and data-storage systems use a two-letter alphabet. That's what a digital communication system is, really—a two-letter system.

The letters in these two-letter alphabets are usually called *zero* and *one*, but these names are arbitrary, and any other pair of names would do as well, for example, *yes/no, up/down, on/off, high/low,* or *trivial* and *obvious* (Caltech students' favorite two words). However, it's not trivial or obvious that with an alphabet of only two letters you can communicate any possible message. In fact, there is a famous episode of "Star Trek" that hinges on just this point.

This episode, called "The Menagerie," tells the story of the unfortunate Captain Christopher Pike, who has been exposed to a near-fatal dose of delta rays and is confined to a sort of tin can. He is able to see and hear and think normally, but he cannot speak. His tin can, however, has a light on the front that Captain Pike can use to communicate—one flash for yes and two for no. He has a two-letter alphabet. Captain Kirk tries for hours to figure out what's wrong with poor Captain Pike, who seems to be upset about something. He is joined by Dr. McCoy, who is not much help, though he gets quite philosophical about it. The stardate is 3012.4, and the dialog runs as follows:

> *Capt. Kirk:* He keeps blinking "no"—no to what?
>
> *Dr. McCoy:* They've tried questioning him. He's almost agitating himself into a coma.
>
> *Capt. Kirk:* How long will he live?
>
> *Dr. McCoy:* As long as any of us. Blast medicine anyway; we've learned to tie into every human organ in the body except one—the brain. The brain is what life is all about. Now, that man can think any thought that we can, and love, hope, dream as much as we can. But he can't reach out and no one can reach in.
>
> *Capt. Kirk:* He keeps blinking "no"—no to what?
>
> *Dr. McCoy:* They can question him for days, weeks, before they stumble on the right thing.
>
> *Capt. Kirk:* Could this have anything to do with Spock?

Yes, of course it has something to do with Spock. What's wrong with these guys? Captain Pike's mind isn't really trapped in there! They should study digital communications. There are many ways they could find out what's wrong. For example, they could have said to Captain Pike, "Think of a sentence describing your problem. Is the first letter A?" and so on, and in five



Courtesy of Paramount Television Advertising & Promotion

In a vintage Star Trek episode poor Captain Pike was exposed to a near-fatal dose of delta rays and locked up incommunicado except for a light that can flash "yes" or "no." He should have remembered how to play 20 questions.

But even the logical Mr. Spock may not have known that if Captain Pike's little light had malfunctioned occasionally . . . it would still have been possible to communicate reliably with him.

minutes they could have found out that Spock was planning to kidnap Captain Pike and take him to the forbidden planet Talos IV. In fact, any conversation that could be carried out between ordinary folks could also be carried out with Captain Pike, with a little patience and ingenuity. If you've ever played 20 questions you already know this.

In the game of 20 questions there are two players. Player 1 thinks of some object, and Player 2 tries to guess what the object is by asking Player 1 a series of questions that can be answered *yes* or *no*. In the traditional version of the game, Player 2 is allowed to ask up to 20 questions. As a simplified illustration, suppose I select one of the letters A, B, C, D, E, F, G, or H, and ask you to try to guess it by asking a series of *yes/no* questions. Our dialog might proceed as follows:

Question: Is it A, B, C, or D?	*Answer:* NO
Question: Is it A, B, E, or F?	*Answer:* YES
Question: Is it A, C, E, or G?	*Answer:* NO

After these three questions you will know what the letter is (in this case, it's F), since every one of the eight possible patterns of *yes/no* answers corresponds to exactly one of the eight letters. (The correspondence is listed on page 36.)

So Captain Kirk could have communicated reliably with Captain Pike by playing 20 questions. But even the logical Mr. Spock may not have known that if Captain Pike's little light had malfunctioned occasionally (by flashing *yes* when he meant *no* or vice-versa), it would *still* have been possible to communicate reliably with him. This fact is far from trivial and obvious, but it's nevertheless true. It can be done by playing "20 questions with lies," a game invented in the 1964 PhD thesis of Elwyn Berlekamp at MIT.

To illustrate how 20 questions with lies works, suppose I think of one of the above eight letters again, and you try to guess it asking *yes/no* questions. This time, however, I am not required to answer truthfully; I can lie sometimes. This time I'll allow you to ask nine questions, and in return you must allow me to lie up to twice in my nine answers. Now our dialog might go as follows:

Question 1: Is it A, B, C, or D?	*Answer:* NO
Question 2: Is it A, B, E, or F?	*Answer:* YES
Question 3: Is it A, B, C, or E?	*Answer:* NO
Question 4: Is it A, E, or F?	*Answer:* YES
Question 5: Is it A or F?	*Answer:* NO
Question 6: Is it F or G?	*Answer:* YES
Question 7: Is it F or G?	*Answer:* YES
Question 8: Is it F?	*Answer:* NO
Question 9: Is it F?	*Answer:* NO

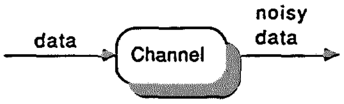
After these nine questions, even though I may have lied to you twice, you will know for sure what the letter is, and which answers were lies. (The solution appears on page 36.) Indeed, Berlekamp's results imply that it is always possible to determine one of eight possibilities with no more than nine questions in the presence of two lies, although the details of the questioning strategy are a little complicated.

The game of 20 questions with lies makes a nice parlor trick, but it also illustrates an important fact: it is possible to communicate reliably even though the communication medium itself is unreliable. This fact, and its remarkable consequences, was discovered in 1948 by a young

Members of the Claude Shannon fan club pose with their idol (left) at the International Information Theory Symposium in Brighton, England, in 1985. McEliece is second from right. At right is Paddy Farrell of the University of Manchester (co-chairman, with McEliece, of the symposium). Shannon showed that any communication process (left) can be rendered reliable by adding redundancy (on opposite page) that creates a strong pattern with the original data.

mathematician named Claude Shannon, one of the finest minds of this or any other century. (Berlekamp was a student of Shannon's.) Before I describe his scientific accomplishments as the inventor of information theory, let me tell two Shannon stories.

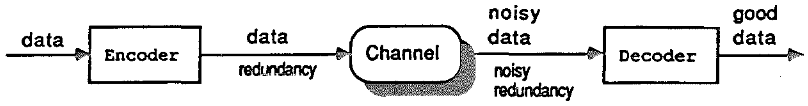
In 1985 the Japanese government decided to institute a prize for scientific and humanistic achievement, called the Kyoto Prize, which the Japanese hoped would rival the Nobel Prizes. (In monetary value a Kyoto Prize is worth slightly more than a Nobel.) The first Kyoto Prize in Basic Sciences was given to Claude Shannon. I expect there was very little trouble deciding whom to give it to. He would have won a Nobel Prize years ago, except that his achievements are in engineering and mathematics, and there aren't any Nobel Prizes in those subjects. The Kyoto Prizes cover all of science, so he was immediately eligible.



The second story is more personal. Shannon's main work was all done before 1965. Since then he has been semiretired, and a whole generation of researchers (including me) had never met him until June of 1985, when he unexpectedly showed up in Brighton, England, at an International Information Theory Symposium. All of us Shannon fans (which included everyone at the symposium, I can assure you) were thrilled to see him, and cameras were clicking all week. At the closing banquet, Shannon was, of course, seated at the head table. About halfway through the banquet, Lee Davisson, who was at that time head of the electrical engineering department at the University of Maryland, did what we had all secretly wanted to do all week: he asked Shannon for his autograph. That opened the floodgates. For the rest of the meal, there was a long line of autograph hounds (including me) waiting for Shannon's autograph. If you know how large scientific egos tend to be, you'll understand how really astonishing this scene was. It was as if Newton had showed up at a physics conference.

That's enough hagiography. What exactly did Shannon do?

Claude Shannon has a great feeling for generalities. He saw that any communication process—talking to another person either face-to-face or on the phone, watching TV, sending photographs of Neptune to Earth—can be modeled by the simple picture at left. The information that must be communicated is transmitted over a channel—the air that separates two people conversing, a telephone wire, the complicated stuff between the television studio and your house, or the 2.8 billion miles of empty space between Neptune and Earth. All communication channels are to a greater or lesser degree "noisy," which means that what comes out of the channel isn't always exactly what goes in. On many channels, the noise is intolerable—think of a bad phone connection or trying to talk to someone near the airport when a 747 flies over. Until Shannon, everybody thought that the only way to communicate reliably over an unreliable channel was to physically make the channel more reliable—yelling to overcome the 747 noise, or, more generally, building more powerful transmitters or more sensitive receivers, and so on. In



Shannon showed that every channel has an ultimate capability to transmit information . . . and that this limit can be reached only if the channel is making lots of errors.

1948, however, Shannon showed that this wasn't necessary. It is, in fact, possible to communicate perfectly reliably over essentially any channel, however noisy it may be. I already showed you an example of this: I was able to communicate to you one letter of the alphabet over a channel that caused errors (lies).

The illustration above shows Shannon's solution for communicating reliably over an unreliable channel. The idea is to send the data over the channel as shown in the previous diagram; but before the data is sent over the channel, it's processed by a man-made device called an encoder. The job of the encoder is to take the data and use it to calculate something called redundancy. The redundancy is then combined with the data before it's transmitted. Roughly speaking, the redundancy is added so that when the data and redundancy are combined, a strong pattern appears. It's a little like adding a U after every Q. Then the data and the corresponding redundancy (the combination is usually called a codeword) are sent over the noisy channel. Of course, the channel may cause errors in the redundancy as well as in the data. Shannon showed, however, that if the redundancy is computed in just the right way, the resulting pattern in the codeword will be so strong that it will almost always still be recognizable despite the channel noise. The pattern-recognizing device is called the decoder, and its job is to reconstruct the original data, using the noisy data and the noisy redundancy as clues.

There's a corollary to this, which is perhaps the most important thing about Shannon's work and which is not fully appreciated, even by many professionals today. It's that Shannon's theory applies to all channels, even ones that *aren't noisy*. In fact, Shannon tells us that if the channel isn't making a lot of errors, you're not using the channel to its fullest capacity. For example, if you're communicating photographs from Neptune to Earth, say at the rate of one picture per second, and everything *seems* to be going as well as it could, you're fooling yourself. You should be pushing the channel harder, maybe 10 pictures per second, right to the ragged edge of failure, forcing the channel to make lots of errors, and then correcting the errors, using redundancy and pattern recognition. In this way Shannon showed that every channel has an ultimate capability to transmit information, called the channel's *capacity* or Shannon limit, and that this limit can be reached only if the channel is making lots of errors, which are being corrected by the decoder like crazy. Shannon proved that channel capacity is like the speed of light: with a lot of work (building fancy encoders and decoders) you can get as close to it as you like, but you can never quite get there.

What Shannon did not do, however, was to say exactly how the encoders and decoders should be designed; he only showed that it must be possible. The actual design task he left for later generations, who have accepted that challenge. (Thank goodness he left something for us to do!) Today there are dozens of different error-correction systems in practical use in a wide variety of applications. I can't begin to describe even a few of these error-correction systems in the space of this article, so I'll content myself with just one, which was invented in 1960 by two MIT researchers, Irv Reed and Gus Solomon. Reed is now professor of electrical engineering at USC, and Solomon is a senior scientist at Hughes Aircraft Company (and also a well-known teacher of integrated voice and movement).

Reed-Solomon codes, as they're now called, began as only a theoretical curiosity, but today they're probably the most widespread and generally useful error-correcting code system. Reed-Solomon codes work, ultimately, with zeros and ones (bits), but it's easier to understand what's going on if you think not in terms of bits but of *bytes*—a group of eight bits. There are 256 possible bytes, numbered from 0 to 255. Reed-Solomon codes protect data that has been grouped into bytes; the data is in bytes, and the redundancy is in bytes, but since the bytes are in correspondence with the numbers 0 through 255, let's just pretend that they work on ordinary numbers.

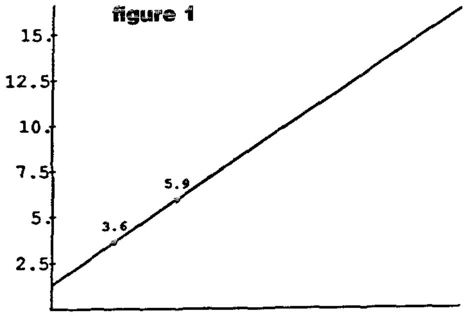


figure 1

So now let's see how Dr. Reed and Dr. Solomon filled Shannon's prescription. We shall see that the basic ideas are geometric: the information to be transmitted is encoded as part of a strong geometric pattern that can be recognized even if it is partly garbled by the channel.

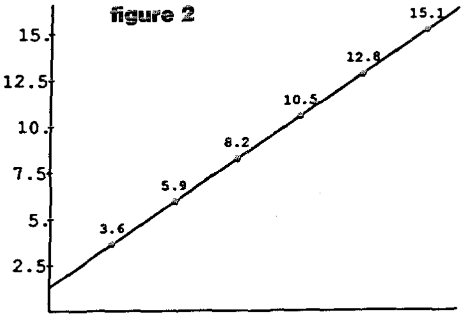


figure 2

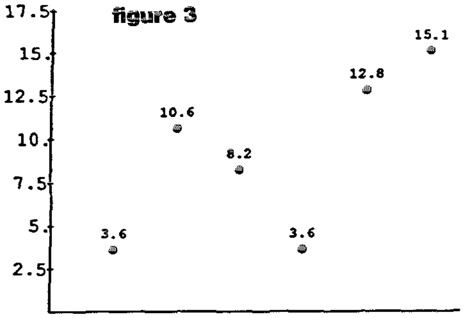


figure 3

Let's suppose, for example, that we want to transmit just two numbers, say 3.6 and 5.9, from one point to another over a noisy channel, and that we want to *encode* these numbers before transmitting them, using Reed and Solomon's ideas. To do this, we first plot the two numbers geometrically, and then join them with a straight line (figure 1). This straight line began with only two points, but of course now there are lots of other points on it. Let's pick four more of these points, spaced equally along the line, which as you can see from the figure are 8.2, 10.5, 12.8, and 15.1 (figure 2). We then use these four extra numbers as the redundancy and transmit the data plus the redundancy as the codeword [3.6, 5.9, 8.2, 10.5, 12.8, 15.1]. This codeword has a very strong pattern: each number is exactly 2.3 more than the previous one.

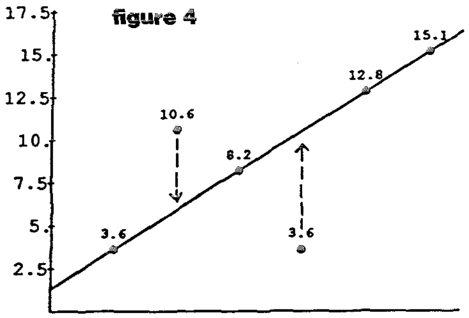


figure 4

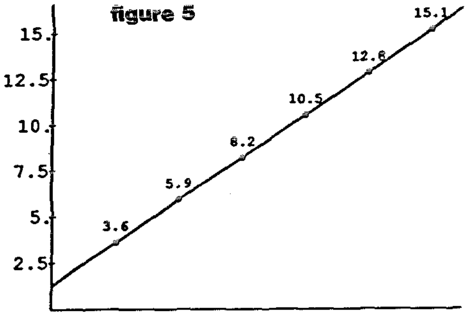


figure 5

Now let's send our codeword over the channel, and let's say that two errors occur, in the second and fourth positions, so that [3.6, 10.6, 8.2, 3.6, 12.8, 15.1] is received (figure 3). You can see that the straight-line pattern has been spoiled, but not entirely obliterated, since four of the six points still lie on a straight line. If we draw a straight line through these four points, we see that two of the points aren't on the line (figure 4). But since we know that the transmitted points all began on a straight line, in order to recover from the errors, all we have to do is move the wayward points back onto the line (moving vertically, as shown), and presto! the original codeword [3.6, 5.9, 8.2, 10.5, 12.8, 15.1] appears (figure 5). Now the original information [3.6 and 5.9] can be read off, and we have communicated these numbers reliably despite the channel noise.

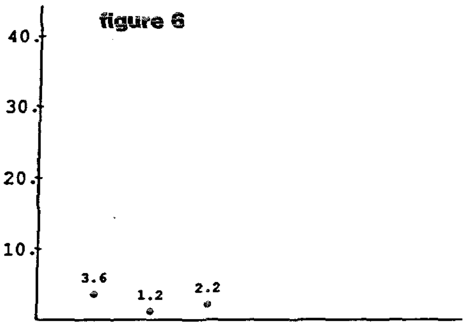


figure 6

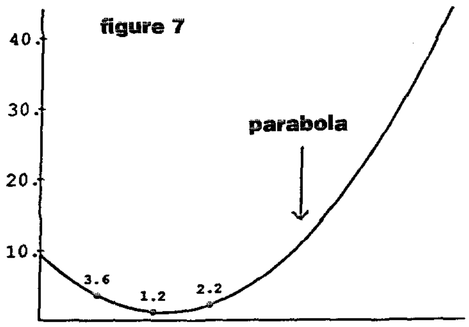


figure 7

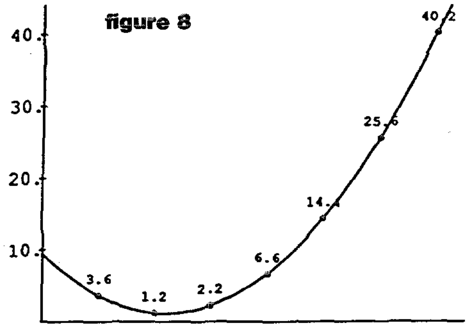


figure 8

Although this was just one example, the same process will always work. If the six-number codeword is subjected to two or fewer errors, no matter where the errors occur, at least four of the received numbers will still lie on a straight line, and if the decoder draws that straight line, the erroneous numbers can be moved onto the line and corrected. If the channel is in an especially bad mood, however, and three or more errors occur, the system can fail. Can you see why? (The answer is on page 36.) If you want your codeword to correct three errors, you'll need six redundant numbers; and to correct four errors, you'll need eight extra numbers, and so on.

I've just explained how Reed-Solomon coding is used to protect pairs of numbers: you connect the two points with a straight line, add extra points on the line, etc. In practice, of course, you might want to send three or more numbers at once. How do Reed-Solomon codes do this? To see how, let's consider another example. Suppose we wanted to send the three numbers [3.6, 1.2, 2.2] over a noisy channel. We again plot the three points geometrically (figure 6). Unfortunately, these three points don't lie on a straight line. But any three points will determine a *parabola*, which is a second-degree curve, so let's draw a parabola through these points (figure 7). Now that we have the parabola, it's easy to guess what to do next. We locate some more, say four more, equally spaced points (equally spaced horizontally) on the parabola, and use these points as the redundancy, thereby producing the codeword [3.6, 1.2, 2.2, 6.6, 14.4, 25.6, 40.2] (figure 8). The original data had no particular pattern, but this seven-number codeword has a very strong pattern, because the seven points all lie on the same parabola. (It's extremely unlikely that seven numbers chosen at random would lie on a parabola.)

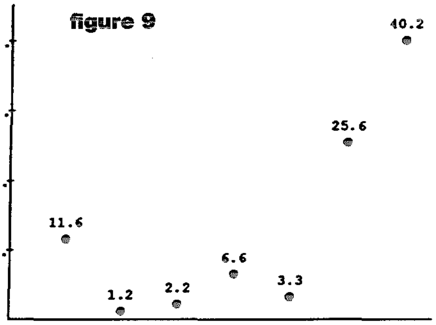


figure 9

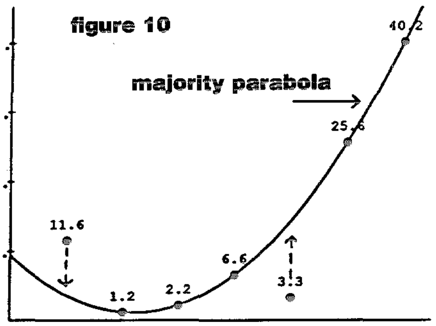


figure 10

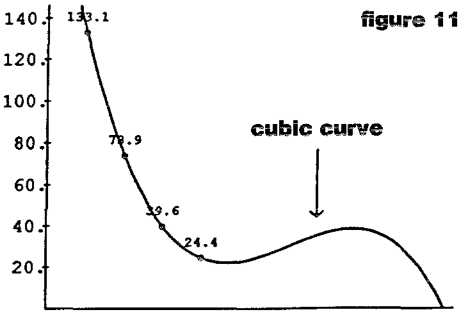


figure 11

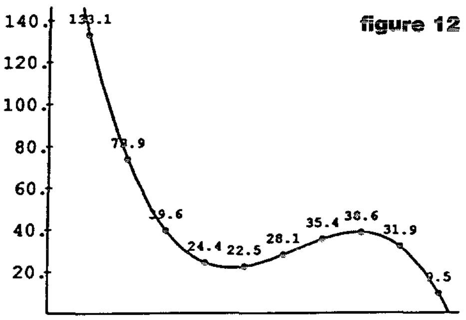
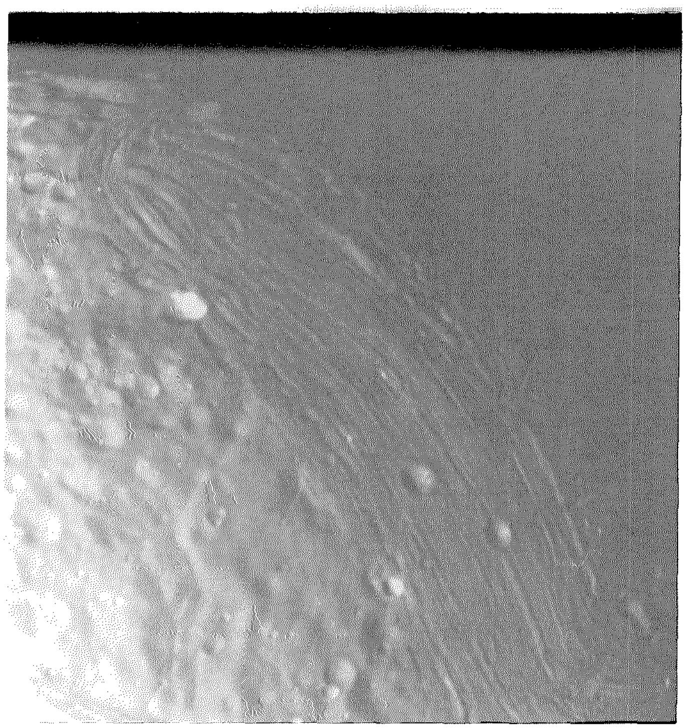


figure 12

With four redundant numbers protecting three pieces of data, we can again correct any pattern of two errors. For example, suppose the parabolic codeword [3.6, 1.2, 2.2, 6.6, 14.4, 25.6, 40.2] were received as [11.6, 1.2, 2.2, 6.6, 3.3, 25.6, 40.2], with errors in the first and fifth positions. To correct the errors, we'd plot the seven received numbers and look for a parabola connecting five of them (figure 9). In this case, it's a little difficult for us humans to see the pattern, but the decoder (computer) doesn't have any trouble, and finds the "majority parabola" immediately (figure 10). Two of the points don't lie on the parabola, so the decoder moves them back on, thereby correcting the errors. Again, with only four redundant numbers, this particular scheme can correct only two errors; to correct more errors, more redundancy is needed, the general rule being that two redundant numbers are needed for each error to be corrected.

What if we wanted to send four numbers at once? Just as two points determine a straight line, and three points determine a parabola, four points determine a *cubic curve* (figure 11). If we want to protect the four numbers from *three* errors, say, then according to Reed and Solomon, we need to choose *six* more points on the curve, thereby producing the 10-number codeword [133.1, 73.9, 39.6, 24.4, 22.5, 28.1, 35.4, 38.6, 31.9, 9.5] (figure 12). When this codeword is received, the decoder plots the points, looks for a cubic curve that goes through at least seven of them, and moves the errant points back onto the cubic, thereby correcting the errors.



Reed-Solomon codes made it possible for Voyager 2 to send back pictures such as this one of geologic details on Miranda, one of the Uranian moons.

On opposite page: Irv Reed (right) and Gus Solomon, who invented their error-correction system in 1960.

Uranus is 2 billion miles away, and Voyager's transmitting power is only 20 watts. That's weaker than the lightbulb in your refrigerator.

This "theoretical" discussion of Reed-Solomon codes gives a pretty accurate idea of how they work, but it's unrealistic in a number of important ways. As I mentioned earlier, Reed-Solomon codes deal with bytes, which are not quite the same as ordinary numbers; and in real applications, there are many more than two, three, or four pieces of data in each code word.

When I teach my students about Reed-Solomon codes, I try to make the subject more practical by dividing the class into teams and having each team write a computer program capable of implementing a fairly powerful RS code. In the particular code I give them, each codeword consists of 15 data characters protected by 16 redundant characters, where a "character" is one of the 26 letters, A, B. . . . Z, or one of the six additional symbols (space), ", #, $, %, and &, so the students work, in effect, with a 32-letter alphabet. If they want to transmit the 15-letter word RUMPLESTILTSKIN, for example, their program must first compute the 16 characters of redundancy, which in this case turn out to be RASZUOBUOS"&YTJS, so the codeword is

RUMPLESTILTSKINRASZUOBUOS"&YTJS

You may find this codeword unattractive; but to the Reed-Solomon decoder, it's beautiful: the 31 characters (when interpreted as special kinds of numbers) all lie on a 14th-degree polynomial curve. With 16 redundant letters, the codeword can resist any combination of eight or fewer errors. For example, if we change the word to

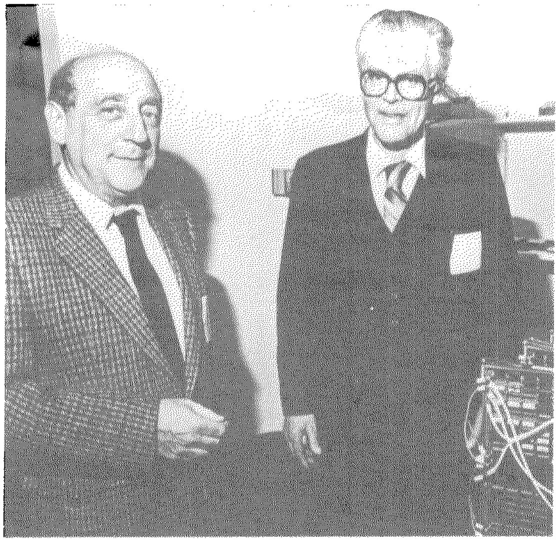
R MCELIECETSKINRASZUOBUOS"&YTJS

(which I like better), and give it to the decoder, it will see that the pretty pattern has been ruined but will find that 23 of the 31 characters still lie on a 14th-degree curve. (To do this, it uses an algorithm invented by Elwyn Berlekamp, the 20-questions-with-lies guy, who's now a professor at UC Berkeley and president of Cyclotronics, Inc. a company that builds Reed-Solomon hardware.) It will then force the eight offending characters back onto the curve and give us

RUMPLESTILTSKINRASZUOBUOS"&YTJS

again.

There are many applications of Shannon's theorems in general, and Reed-Solomon codes in particular, in today's technology. Error-correcting codes are used in many high-performance military and civilian communication systems. For example, the high-speed modems that today's computers use to talk to each other are made possible by fancy error correction, among other things. One of the most spectacular applications is in the exploration of the solar system. The Voyager 2 spacecraft sent pictures of the planet Uranus back to Earth in January 1986, using what must be the most sophisticated and powerful communication system ever built, on this planet at least. (Uranus is 2 billion miles away, and Voyager's transmitting power is only 20 watts. That's weaker than the lightbulb in your refrigerator.) That communication system has some pretty fancy error correction, which includes a Reed-Solomon code with 223 bytes of data and 32 bytes of redundancy in every codeword. Of course, Voyager's communication system



Reed-Solomon codes, as they're now called, began as only a theoretical curiosity, but today they're probably the most widespread and generally useful error-correcting code system.

depends on a lot of other things too: big and accurate antennas, low-noise receivers, sophisticated transmitters, and much more. Still, without the error-correcting codes, Voyager would have been able to send only about 20 percent of the data that it actually did send.

But applications to communications systems are just half the story. Shannon's theorems have also been applied to the *storage* of information, not just transmission. If you think about it, storage is another form of communication—communication in time rather than space; from *now* to *then* rather than from *here* to *there.* Anyway, there are many storage systems that use Shannon's prescription—computer tapes, disks, and so on. In terms of dollars invested, one of the most widespread applications of Shannon's theorems (via Reed-Solomon coding, in fact) is in data storage. If you own a CD player, then you own a data-storage system that uses Reed-Solomon codes. It's all done using Shannon's prescription, and zeros and ones.

A CD holds up to 74 minutes of music. The music is represented digitally, using lots of zeros and ones. In fact, it takes about 1.5 million bits to represent just one second of music, and more than 6 billion bits are needed for the entire 74 minutes. The bits are stored optically, with tiny "pits" on the mirrorlike surface of one side of the CD. These pits are recorded along a spiral track on the disk, a track that is more than three and a half miles long but only .5 microns wide (.5 microns is approximately the wavelength of green light; that's why light is scattered into a rainbow by the surface of the CD). The pits range from 0.9 to 3.3 microns in length. Such tiny features are quite susceptible to errors; fingerprints, dust, dirt, abrasions, and manufacturing irregularities can all cause problems. So an error-correcting code is used on these disks, and it turns out to be a Reed-Solomon code, in which the bits of information were first blocked into eight-bit bytes. The details are a little complicated (the industry's acronym is "CIRC," for "Cross-Interleaved Reed-Solomon Code") but to protect the 6 billion bits on the disk, another 2 billion error-correcting bits are added, so that fully 25 percent of the bits on the disk are for error correction. Your CD player at home contains a very sophisticated Reed-Solomon decoder, which processes about 2 million coded bits per second.

The result of all this is that a CD is remarkably resistant to errors. I have heard that you can actually drill holes in a CD and it will still play, and I know (since I've done it myself) that you can deliberately scratch one with a paper clip without losing any music. And because of the error correction the music you hear from a scratched disk isn't merely *almost* as good as the original; it's *exactly* as good as the original. Of course, if you get carried away and overdo it, you might really wreck your favorite CD (which is unfortunately what happened to my poor Buddy Holly CD when we shot the photo on page 26). Coding to combat malicious mischief is beyond the scope of this article. ☐

Bob McEliece has been professor of electrical engineering at Caltech since 1982. He's also an alumnus (BS 1964, PhD 1967), as is Irv Reed (BS 1944, PhD 1949). From 1967 to 1978 McEliece worked at JPL as supervisor of the information processing group in the communications research section, and then became professor of mathematics at the University of Illinois at Urbana-Champaign before returning to his alma mater. McEliece is especially well known for applications of discrete mathematics to various problems in communication theory.

This article was adapted from an April Watson Lecture featuring considerable audience participation. A good time was had by all, thanks to McEliece's talent for making error-correcting codes a lot of fun. Both Reed and Solomon were in the audience at Beckman Auditorium.

If you haven't already peeked, turn the page for the answers to the problems posed in the article.

Answers: Safety in Numbers

page 27

TIARA
ADAPT
TREND
NERVE
SPLIT

page 28

One possibility for six variations on a five-letter word was submitted by Paul Carpenter of Burbank after the Watson Lecture: STA?E

STAGE
STAKE
STALE
STARE
STATE
STAVE

Doublets:

LEAD
LOAD
GOAD
GOLD

page 29

The correspondence between the eight patterns of YES–NO answers to the three questions, "Is it A, B, C, or D?" "Is it A, B, E, or F?" and "Is it A, C, E, or G?" is as follows:

A:	YES	YES	YES
B:	YES	YES	NO
C:	YES	NO	YES
D:	YES	NO	NO
E:	NO	YES	YES
F:	NO	YES	NO
G:	NO	NO	YES
H:	NO	NO	NO

To solve the 20-questions-with-lies problem, it's best to think of each of the nine answers as a vote against some of the letters. For example, the first answer counts as one vote against each of the letters A, B, C, and D; the second answer is a vote against the letters C, D, G, and H; and so on. In this way, we can calculate a little table, giving the votes against each of the eight letters:

A: 1, 3, 5, 6, 7
B: 1, 3, 4, 6, 7
C: 1, 2, 3, 4, 6, 7
D: 1, 2, 4, 6, 7
E: 3, 6, 7
F: 5, 8, 9
G: 2, 4
H: 2, 4, 6, 7

This table shows that, for example, A has 5 negative votes; in other words, if the letter really were A, then I lied 5 times. Similarly, if it were B, I lied 5 times; if it were C, I lied 6 times, etc. But since I agreed to lie at most twice, and all letters but G have 3 or more negative votes, the letter must have been G, and I must have lied on answers 2 and 4. (Notice also that all 9 questions were needed, since after 8 *questions, both F and G were still in the running.*)

page 33

If [3.6, 5.9, 8.2, 10.5, 12.8, 15.1] is sent, but received as, say [3.6, 9.1, 8.2, 10.5, 6.4, 5.5] (with errors in positions 2, 5, and 6), then four of the points (9.1, 8.2, 6.4, and 5.5) lie on a straight line, but not the *original straight line!* (*figure A1*). *But the* decoder will have no way of knowing this, and so will move the two points that are off the line back on (figure A2) thereby producing the "codeword" [10.0, 9.1, 8.2, 7.3, 6.4, 5.5] and reporting that the transmitted information was [10.0, 9.1].

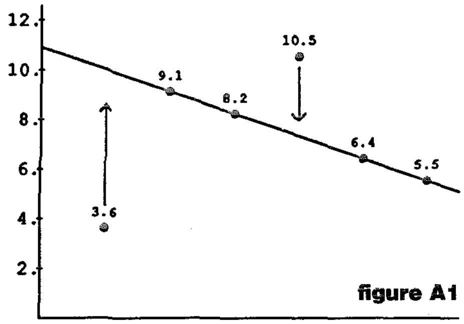


figure A1

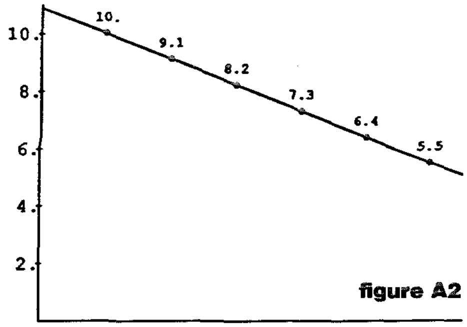


figure A2

Thu Le graduated this spring.

Out of Africa

Senior Thu Le took the Summer Undergraduate Research Fellowship (SURF) program to the University of Durban-Westville (UDW) in Durban, South Africa last summer. Le's project, sponsored by Professor of Geography Ned Munger, was to set up a similar program in the lab of Mario Ariatti, a biochemistry professor at UDW. Like its progenitor, the program puts undergrads in a real laboratory doing real research fulltime over the summer. The program is designed to get black students interested in science, in hopes that they will pursue advanced degrees.

Students in South Africa earn their bachelor's degree in three years. The student then has the option of a fourth year of independent study leading to a so-called "honors" degree, which in turn is a springboard to masters and PhD programs. But few blacks stay for the honors degree. "The black students' goal is to get out and find jobs that will help their families," explains Le. "They can't afford to stay too long. We need to help them get motivated. I was setting an example. So I designed this research project, and set it up, and some honors students will pick it up. There are two or three of them tentatively signed up for next year."

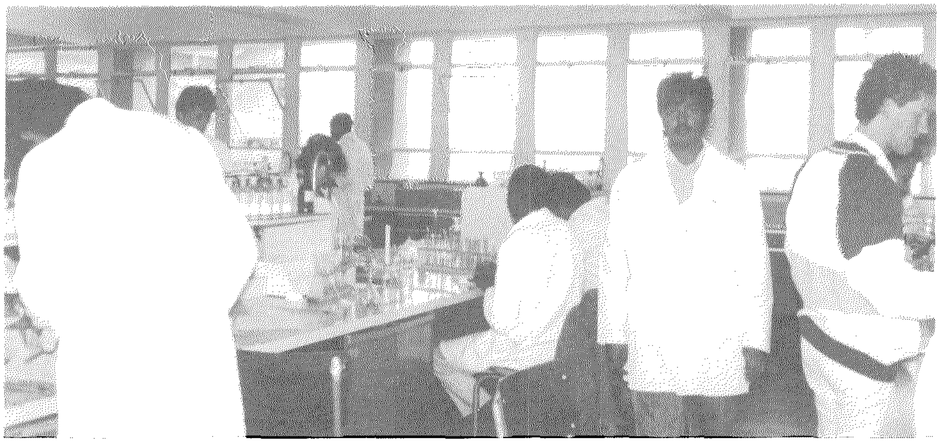
(Le certainly sets an example of motivation. Her family fled Vietnam in an open boat when she was 12. They were eventually picked up by an Italian freighter, and lived in Italy for three years before moving to California. She graduated at the top of her class at San Jose High, having mastered English and Italian along the way.)

Le's project explores one facet of the process through which genes in cells are turned on and off. When a piece of DNA is heavily methylated—its surface studded with methyl groups—the gene it encodes is quiescent. As the methyl groups are removed, the gene becomes active. An enzyme called DNA methyltransferase sticks the methyl groups onto the DNA and plucks them off. The research studies how methyltransferase binds to DNA, and how the binding process might be controlled. The ability to control binding at specific sites, and hence to turn genes on and off, would have implications ranging from curing some genetically based diseases to breeding tomatoes that stay ripe longer.

"I got a couple of projects started," she says, "but it was pretty slow because there's hardly any equipment or anything available there. We used radioactively labeled DNA, and you need a scintillation counter to track it. Durban-Westville didn't have one, so we had to use the local blood bank's, and they would only let us use it one day a week. And we had to make the DNA samples, too. Over here, you can do it on a machine. But we had to do it all by hand, which was good, in a way, because I learned how to do things I wouldn't have had to do at Caltech."

The UDW began as an all-Indian university—Natal province has a large Indian population, imported in the last century to work the sugar plantations—but went multiracial a few years ago. It is now about 75 percent Indian and 25 percent Black, with perhaps one or two percent whites—primarily the more liberal sort, who attend to make a statement. Le, who lived in one of the women's dorms during her stay—there are no coed dorms—was the only American (and the only Oriental) on campus. "The students were really friendly," says Le. "They look up to this country, but they hate it too. Most of them are very socialist. I'd just become a U.S. citizen, and I'd never had to defend myself so much in my life."

There were two or three riots in the two months Le was on campus—black student groups versus each other, the Indian students, or the police in various combinations. "I think that's about average," Le says. "I always kept a low profile when the police came in, because I didn't want my visa canceled. The police came every night during cultural week. They tried to film a play to see who was there. The director told them they couldn't under copyright law, but



The laboratory at UDW where Le set up her research project. Prospective honors students will carry on the work this summer.

Two other Caltechers will be in Africa this summer.

the police said they had to under the emergency regulations, so it was canceled instead. This happens all the time." The one time they didn't show was the night a black group allied with the police threatened to raid the campus. "They never come when there's an Inkatha raid, except later to collect the survivors."

Durban, like most large South African cities, has a multiracial downtown. "You can go to most of the movie theaters and bars, but there's still a sort of segregation. You can go to a white dance club with black friends, but you don't really feel comfortable. And you see the juxtaposition of the First and Third Worlds. The three main streets are white shops, all set up like American shops. The side streets are mostly Indian shops, which look like the Mexican shops down in Tijuana. There's a lot of haggling, and the shop windows are crowded, with everything crammed up in them."

Le also got out of the city, where she found that multiracial facilities are still the exception rather than the rule. "An Indian friend and I went touring, and we had to call ahead to the motel to make sure they were multiracial. If they're not, you might have to drive another 100 miles to the next one."

In addition to visiting wildlife parks for her own enjoyment, Le visited several high schools and universities as part of her project. The black high schools get the leftovers of the educational budget. Some don't even have electricity. Many instructors aren't fluent in English, further handicapping potentially college-bound blacks, as English is the language of the universities. Indian students, who rank just below whites in the apartheid system, have better facilities and learn English from an early age.

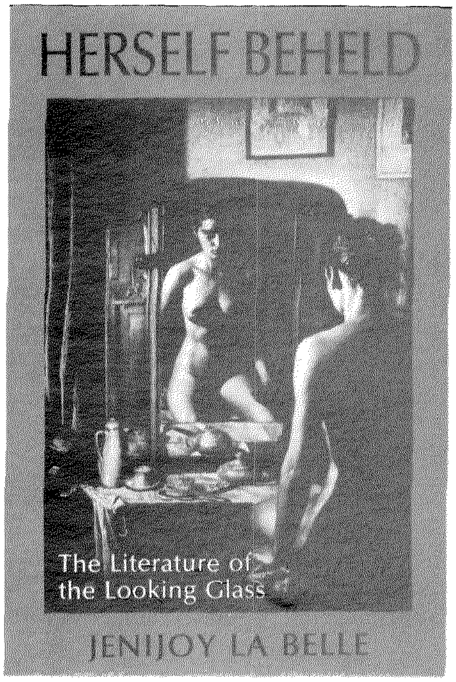
Le didn't spend much time in Johannesburg, the most conservative part of South Africa, but she did visit Soweto. "When Witwatersrand University was built," she says, "it was a white university in a white suburb. When it first admitted black students in the early '80s they couldn't live on campus in a white area, so they had a dorm on the border of Soweto, and were bused in. It's about a 20-minute drive." (An integrated dorm has since been built on campus.) Sibusiso Sibisi, a black lecturer in chemistry who lives in the Soweto dorm, took her around. (Sibisi was at Caltech during 1987-88 as a visiting associate, working with Institute Professor of Chemistry, Emeritus, John Roberts.) "Most of the houses are tiny, tiny ones, and then next door there'd be a big garbage dump. A lot of the houses are government-built, and rented out. When I was there, there was a rent boycott, then a garbage boycott, and it was a real mess. The garbage was literally up to your knees and it stank."

Le feels the best hope for peaceful change lies in U.S. aid to improve black education and sanctions on high-technology products such as computers, plus a fuel embargo. "The poor people won't suffer," she says, "because they don't have cars. But the rich will feel it."

Le, who graduated this spring, will be starting medical school at U.C. San Francisco this fall. She hopes the situation in South Africa will have improved by the time she's through, but even if it hasn't, "I'd love to go back. A lot of foreign doctors go there after training. There's lots of good experience, including diseases that have been wiped out in the First World. And they do need the help. At one black clinic I visited, there was a German doctor with two nurses, and he said in the first two hours they were open they had 200 patients waiting in line."

Two other Caltechers will be in Africa this summer. Samuel Clark, a freshman on a SURF grant, and Gary Bloomberg, a graduate student taking time off from applied mechanics, will be at the University of Namibia in Windhoek, teaching a math refresher course for black high-school science teachers. The three's air fares were paid by the Cape of Good Hope Foundation, of which Munger is president.☐—*DS*

Books



Cornell University Press, 1988
202 pages

Herself Beheld is a book-length meditation on women and mirrors. For the purpose of her book Jenijoy La Belle (who is a professor of literature at Caltech) has gathered her material from literary sources, mainly in the 19th and 20th centuries. One of the contentions underlying *Herself Beheld* is that the best texts, judged by literary-critical criteria, will have the most sensitive analyses of mirror scenes. La Belle's extraordinarily wide range of examples convincingly bears out this contention.

Herself Beheld is literary criticism which contains, or tends towards, some controversial propositions of a sexual-political nature. It is possible to admire the one, while demurring at certain aspects of the other. The first thing to say is that La Belle is a fine critic with a fine sensibility and an exceptionally fine turn of phrase. One reads *Herself Beheld* with a constant pleasure in the writing. The author's style moves between epigrammatic terseness and belletristic copiousness. *It is never less than lucid, and* a frequent felicity of expression suggests that La Belle might as easily find herself on the other side of the fence, with the creative rather than the critical authors. An example from the first chapter (wittily entitled "Introductory Reflections") gives a flavor of the book's prose:

> Female characters [in literature] view themselves in various kinds of mirrors. They peer at their reflections in wavy-surfaced mirrors, examine themselves from every angle in swinging mirrors, nod to their faces in silver-backed hand glasses, twirl in front of triple mirrors. They regard themselves in the demoralizing glasses in ladies' rooms. They lean close to mirrors studded with incandescent bulbs. Passing shops, they catch glimpses of themselves in polished panes. They pull thin rectangular mirrors out of their pocket-books. They use windows as dark mirrors when the daylight dies.

It continues for another page, as the author tumbles out her eloquent catalog of the multitudinous relationships that women have with these reflective surfaces that mean so much to them.

On other occasions, instead of compacting her illustrations into a list, La Belle will examine a particularly telling mirror scene at length, or return to it a number of times. Her extended discussions of George Eliot's *Daniel Deronda* and D. H. Lawrence's *Lady Chatterley's Lover* I found most illuminating. Judged purely as an exercise in critical commentary, *Herself Beheld* is admirable.

The fascination of this book is that *it goes beyond critical commentary to* consider what it is to be a woman in the modern world. As an essay on the sociology of femaleness, *Herself Beheld* is consistently shrewd and (in dealing with theorists like Lacan and Eco) intellectually sophisticated. But some of La Belle's theses are provocative. She takes as axiomatic, for example, that men and women use mirrors in quite different ways. "What women do with mirrors," she tells us, "is clearly distinct and psychically more important than what men do with mirrors in their pursuit of generally utilitarian goals." Men, she claims, *use* mirrors; to shave, to check that their ties are straight or that their hair is tidy. Women, by contrast "explore the reaches of the mirror for what they really are." A woman's engagement with her mirror involves her deepest identity. But "when a man stands before a mirror, he is usually there for a practical purpose."

I wonder. I have, for instance, a 15-year-old son who spends what seems to me a major part of his waking life looking into mirrors. If there is a practical purpose in this inspection, I have yet to find out *what it is. And I sus*pect, from what I see there, that the mirror in the men's room of the Athenaeum receives as much purely narcissistic gaze as its next-door neighbor.

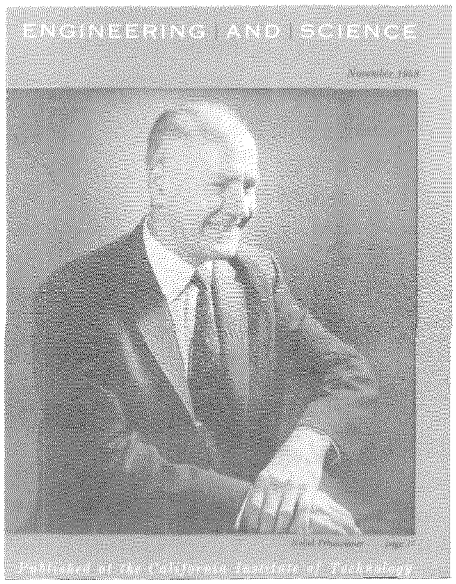
Another controversial thesis in *Her-*

self Beheld is that the act of looking into the mirror is, for the woman, as exploratory and self-defining as the acts of reading and writing. In other words, the mirror is a royal road to enlightenment and emancipation. "Texts and mirrors," La Belle tells us, "can perform similar psychological functions for women." Which means, a wag might argue, that classes in the proper use of the mirror would be as valuable to the humanistic enterprise as Freshman Literature. It will be hard to get that one past the curriculum committee.

Herself Beheld winds up with a chapter entitled "A Mirror of One's Own." The allusion, of course, is to Virginia Woolf's famous polemic, "A Room of One's Own" which argues that personal, private space is a necessary precondition for the liberation of women. La Belle would furnish Woolf's room with a full-length mirror. Traditionally, feminists have tended to see mirrors as part of the apparatus of male tyranny, and breaking the tyrannous looking glass is, in the received view, a necessary act of deliverance. No, argues La Belle. The mirror should instead be used as a weapon. She further argues that this emancipating use of the mirror is already happening: "instead of throwing the mirror away, women are making it into a more flexible tool. By taking the mirror into their own hands, women are eliminating the mirror as tyrant, as dominant male." It's heady stuff. Not everyone will agree with La Belle, but this is a book whose impact will surely be felt well beyond the specialist readership of lit-crit.

John A. Sutherland
Professor of Literature

Obituaries



ENGINEERING AND SCIENCE

George Beadle
1903–1989

George W. Beadle, who was chairman of Caltech's Division of Biology from 1946 to 1961, died June 9 in Pomona at the age of 85. He was the fifth Caltech faculty member to win the Nobel Prize.

Beadle earned his BS (1926) and MS (1927) degrees from the University of Nebraska, and finished his PhD in 1931 at Cornell. A National Research Council fellowship first brought him to Caltech that year, and he stayed on as a research fellow and instructor until 1936. After a year as assistant professor of genetics at Harvard, he spent 10 years as professor of biology at Stanford before returning to Caltech as professor of biology to succeed Thomas Hunt Morgan as division chairman. He brought a natural enthusiasm and enormous energy to the division, which he built into one of the best in the country. In 1961 he left Caltech to become the seventh president of the University of Chicago, a position he held until 1968. He continued to teach biology and conduct research as professor emeritus at Chicago until 1975.

In 1958 Beadle was awarded the Nobel Prize in physiology or medicine, which he shared with Edward L. Tatum and Joshua Lederberg. The Beadle-Tatum discovery in 1941 that genes control the synthesis of vitamins, amino acids, and purines and pyrimidines in the living cell gave science its first clue about how genes work. Their discovery, made by subjecting the red bread mold *Neurospora* to x-rays and ultraviolet light to produce mutations, first suggested that each of the biochemical reactions of a cell is governed by a particular gene, which led to the idea that each gene controls the production of a particular enzyme.

Beadle also worked with the other organisms that are major tools of theoretical genetics—*Drosophila,* which he studied under Thomas Hunt Morgan, and maize, in which he maintained a life-long interest from his college days. He was known for cultivating a field of corn wherever he lived—from his backyard in Pasadena to the somewhat less rural South Side of Chicago.

Among his numerous other awards are 36 honorary degrees. Beadle was co-author (with his Caltech colleague, Alfred H. Sturtevant) of the 1939 standard work, *An Introduction to Genetics,* and also wrote *Genetics and Modern Biology* in 1963. He and his wife, Muriel, wrote *The Language of Life* in 1966. He had been a member of the Caltech board of trustees since 1969.

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GENERAL DYNAMICS

A Strong Company For A Strong Country

Mabel Beckman 1900–1989

Mabel Beckman died June 1 at the age of 88. With her husband, Arnold O. Beckman, chairman emeritus of Caltech's board of trustees, Mrs. Beckman was a one of Caltech's most generous long-time friends. Landmarks of the Beckmans' support cover a substantial portion of the campus—Beckman Auditorium, the Mabel and Arnold Beckman Laboratories of Behavioral Biology, the Arnold and Mabel Beckman Laboratory of Chemical Synthesis, and the new Beckman Institute, to be dedicated in the fall.

Mrs. Beckman seemed always to be at her husband's side through their 64 years of marriage, from his years as a Caltech graduate student, then a faculty member; through the founding of Beckman Instruments 53 years ago (Mrs. Beckman traveled by train calling on chemical supply houses all over the country to market the first pH meter); and their later years together as philanthropists particularly interested in supporting scientific research and education.

They met at Thanksgiving 1918, when Arnold Beckman, then a member of the Marine Corps stationed at the Brooklyn Navy Yard, had just missed being shipped to Vladivostock. Then came a second stroke of luck. Having been ordered to consume a Thanksgiving dinner that the Red Cross was providing for wounded marines (he wasn't wounded and had already eaten), Beckman met Mabel Meinzer of Brooklyn, who was helping serve the food. In describing their meeting, Beckman often expressed wonder at how pure chance determines the course of one's life.

They were married in 1925, and the next year Mabel Beckman came West with her husband in a Model T Ford to begin a relationship with Caltech that was to last 63 years. They lived for many years in Altadena, and when they later moved to Corona del Mar, they maintained close ties to the campus.

To honor Mrs. Beckman, the Mabel Beckman Prize was established by the Institute in 1986 to be presented to a junior or senior woman at Caltech who "has achieved academic excellence and demonstrated outstanding leadership skills, a commitment to personal excellence, good character, and a strong interest in the Caltech community." The prize this year was awarded to Julie Ann Sheridan, who graduated with honors in electrical engineering.

A campus memorial service for Mabel Beckman was held July 10—fittingly in Beckman Auditorium.

Francis Buffington 1916–1989

Professor of Materials Science, Emeritus, Francis S. Buffington, died April 23 at his home in La Canada-Flintridge. He was 72.

Buffington earned his SB degree (1938) and ScD (1951) from MIT. He was appointed assistant professor of mechanical engineering at Caltech in 1951 and associate professor in 1956. In 1963 he became associate professor of materials science and professor in 1983; he retired as professor emeritus in 1985. Buffington's research concerned the diffusion of solids and phase transformation in solids.

Generous with the time he devoted to Caltech, he served on numerous faculty committees, including the graduate committee, the membership and bylaws committee, the faculty board, and the curriculum committee, of which he was chairman in 1980-81. He was also associate dean of graduate studies, and took on several student-advisory roles in the Division of Engineering and Applied Sciences. As option representative, Buffington knew all students individually and could recall instantly the details of their academic lives.

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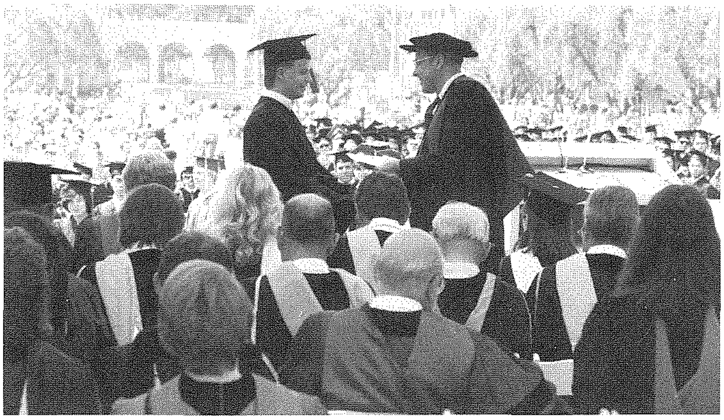
Random Walk



As the largest graduating class in Caltech history, 231 students received BS degrees at the 95th commencement exercises. The Institute also awarded 151 MS degrees and 134 PhDs at the June 16 ceremony. The commencement address was delivered by James J. Duderstadt (MS '65, PhD '68), president of the University of Michigan. Here, Caltech President Thomas E. Everhart congratulates a graduate.

Honors and Awards

Norman Davidson, Chandler Professor of Chemical Biology, Emeritus, and Interim Chairman of the Division of Biology, has won the 1989 Robert A. Welch Foundation Award in chemistry.

Charles Elachi, lecturer in electrical engineering and assistant lab director for JPL's office of space science and instruments, has been elected to the National Academy of Engineering.

President Thomas Everhart has been chosen by the American Society for Engineering Education for its 1989 Benjamin Garver Lamme Award.

Leroy Hood, Bowles Professor of Biology and Director of the Center for the Development of an Integrated Protein and Nucleic Acid Biotechnology has been selected for the 1989 Cetus Corporation Biotechnology Research Award.

Professor of Theoretical Physics Steven Koonin has been named a Fellow of the American Association for the Advancement of Science.

Edward Lewis, Morgan Professor of Biology, Emeritus, has been elected a Foreign Member of the Royal Society of London.

Professor of Chemical Engineering Manfred Morari has received the 1989 Curtis W. McGraw Research Award of the American Society for Engineering Education.

Four faculty members have been elected to the National Academy of Sciences. The election of Professor of Chemistry Robert Grubbs, Hayman Professor of Mechanical Engineering and Professor of Jet Propulsion Frank Marble, Moore Professor of Computer Science Carver Mead, and Professor of Chemical Physics Ahmed Zewail brings to 60 the number of Caltech faculty in the Academy.

Seven faculty members have been named Presidential Young Investigators by the National Science Foundation, out of a nationwide pool of 197 applicants. They are Frances Arnold, assistant professor of chemical engineering; Harry Atwater, assistant professor of applied physics; Geoffrey Blake, assistant professor of cosmochemistry; Melany Hunt, assistant professor of mechanical engineering; Andrew Myers, assistant professor of chemistry; Mitchio Okumura, assistant professor of chemical physics; and Stephen Wiggins, assistant professor of applied mechanics.

Senior Craig Sosin is one of 20 winners of *Time* magazine's annual College Achievement Awards.

Distinguished Alumni

Five Distinguished Alumni Awards were given this year. James J. Duderstadt (MS '65, PhD '68), president of the University of Michigan; Max V. Mathews (BS '50), of Stanford University's Center for Computer Research in Music and Acoustics; Robert L. Noland (BS '41), president and CEO of Ketema, Inc.; Cornelius J. Pings (BS '51, MS '52, PhD '55), senior vice president for academic affairs and provost at the University of Southern California; and Donald C. Shreffler (PhD '62), professor of genetics at the Washington University School of Medicine, join 104 alumni so honored.

Oops!

In the last issue of *E&S* Paul Bellan's letter in response to a statement that there had been negligible progress in nuclear fusion contained the sentence: "Although there have indeed been instances of optimism, in general the scientists involved have realized that fusion research, being one of the most challenging technical problems addressed by man, would take both a long time and much effort before success would be achieved." This should have read "*excessive* optimism." We regret the error, but correcting it gives us the opportunity to note that Bellan's letter was dated March 21, ironically just on the eve of the most excessive optimism of all.