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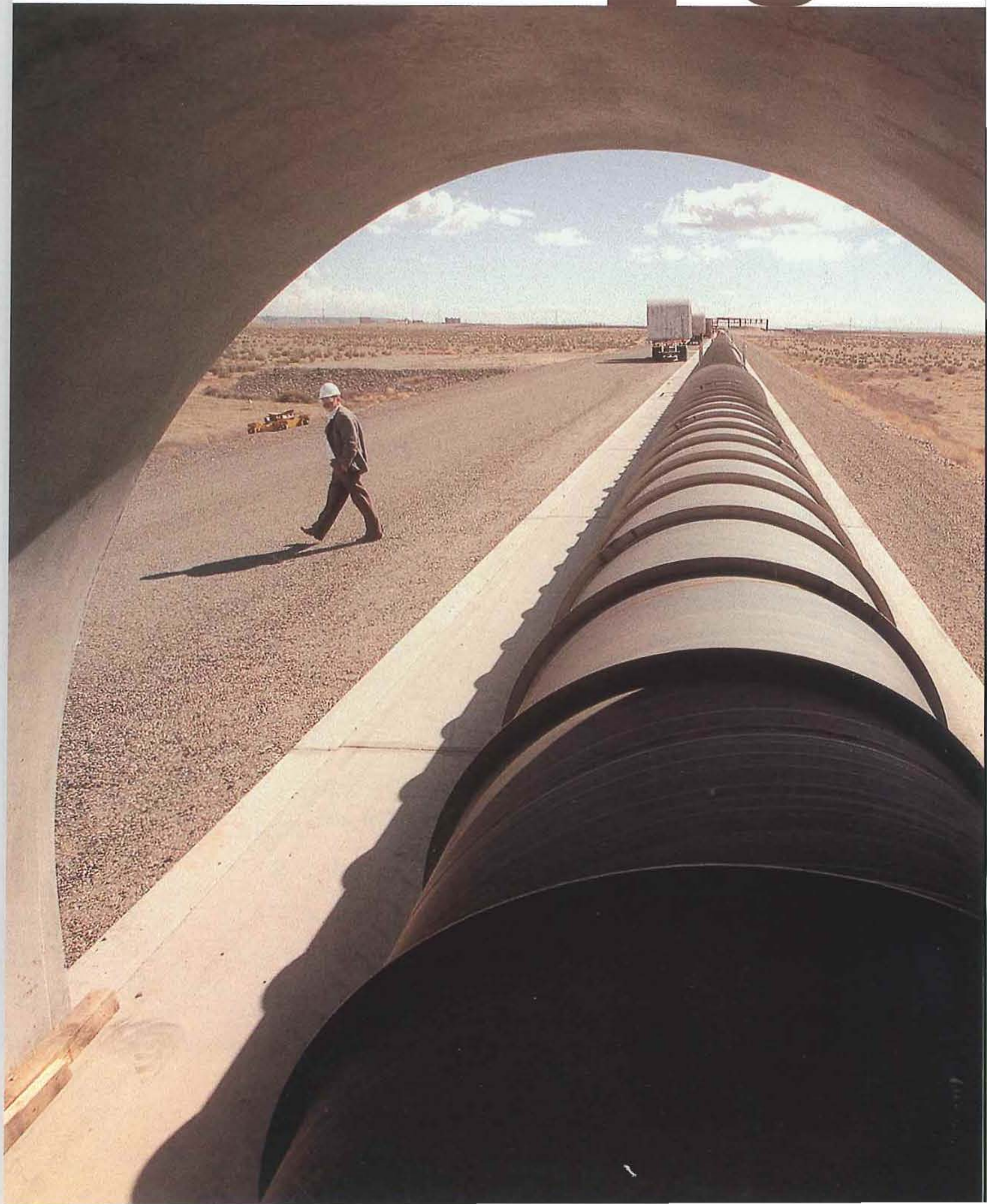

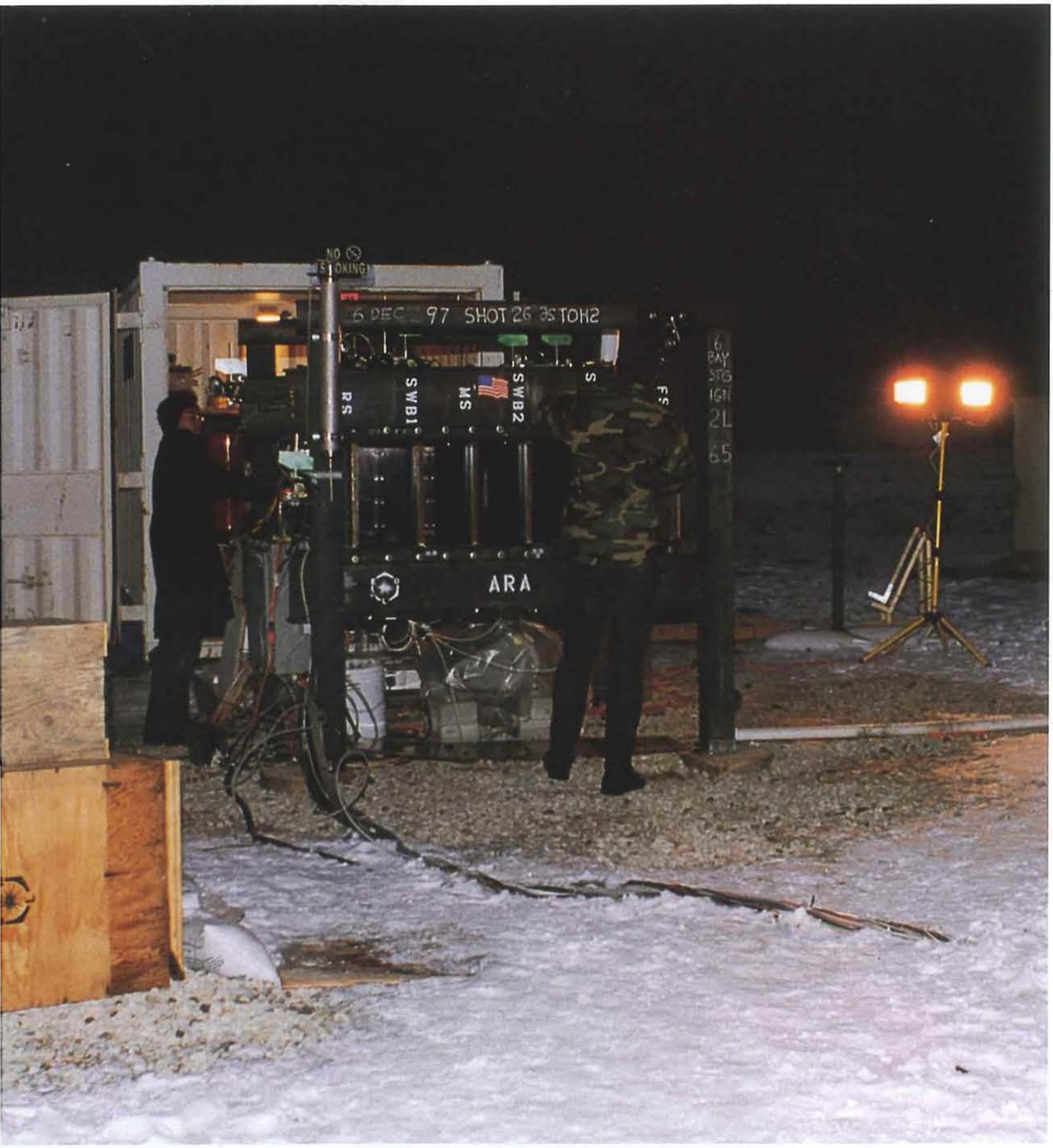

The explosion of TWA's flight 800 launched the biggest air-crash investigation in U.S. history. As part of the probe, a Caltech group built a scale model of the fuel tank where the explosion is believed to have occurred. The test explosions took place near Denver last fall—although it might as well have been winter; they got 31 inches of snow on October 24 and 25. (A second storm, later in the tests, dropped another foot and a half or so.) Here postdocs Julian Lee and Chris Krok (in camouflage jacket) prepare the model for the next day's test. For more on Caltech's role in the investigation, see the story on page 18.

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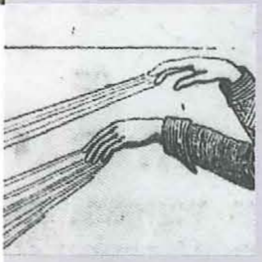

On the cover: No, it's not the Trans-Alaska Pipeline, but two-and-a-half miles of stainless steel tubing, protected by concrete arches, reaching across a desert in the Pacific Northwest. Inside, a laser beam, bouncing between mirrors on either end (and in an adjoining arm) will try to pick up an almost imperceptible signal, when the Laser Interferometer Gravitational-Wave Observatory turns on in the year 2002. The story of LIGO and Caltech's venture into big science begins on page 8.

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Its Mars mission may be over, but JPL's rover still gets around. "The American Space Experience" at Disneyland's freshly updated Tomorrowland includes a full-scale model of the lander and rover as well as models and interactive displays about other JPL missions. At the opening ceremony, JPL Director Ed Stone cuts a rover-shaped cake while Mickey Mouse, park president Paul Pressler, and Pathfinder project scientist Matt Golombek look on.

Next on the rover's itinerary is a New Year's Day appearance on "Martian Mischief"—the 1999 Rose Parade float to be built by the city of La Cañada Flintridge, where the bulk of JPL is located. As the rover trundles across a floral Mars-scape, Martians will pop up out of hollow rocks to wave at it, but whenever it turns to look toward them, they will hide. The computer-animated float's mechanical, electrical, and hydraulic systems are being built by a group of volunteers that includes about 50 JPLers, and lots more help will be needed, especially come decorating time in December. Any member of the Caltech/JPL community with an urge to get a little bit of Mars (or at least some marigolds, carnations, lentils, carrot flakes, and potatoes) under their fingernails should call Bob Ferber at (818) 790-2013.

In other way-cool toy news, Mattel and JPL will reprise the tremendously popular Hot Wheels set based on the Pathfinder mission with a Hot Wheels Jupiter/Europa Encounter Action Pack. The set, due out in early 1999, will include replicas of the Galileo spacecraft, the descent probe, and a Deep Space Network antenna dish.

BIOLOGY CAMPAIGN ANNOUNCED

Caltech has formally announced the goal of raising $100 million in a campaign to support new initiatives in the biological sciences. The Biological Sciences Initiative (BSI) will allow the Institute to create approximately a dozen new faculty positions in biology and related disciplines, construct a new biology building, develop new joint training programs with medical schools, and address several major biological questions that can be answered only through sustained research in state-of-the-art facilities.

"Our campaign is ultimately aimed at complex questions like the nature of consciousness, how memory and learning operate at the molecular level, how cells grow and die, and how genetic networks function," says David Baltimore, president of Caltech and a Nobel Prize–winning biologist himself. Adds Mel Simon, Biaggini Professor of Biological Sciences and chair of the Division of Biology, "We will go beyond the traditional disciplines and integrate our approaches with those of our colleagues in chemistry, physics, and engineering to achieve a more intimate understanding of how biology works." ☐

PAULING EXHIBITION TO TOUR COUNTRY

The life and legacy of the late Linus Pauling (PhD '25) will be celebrated in a free exhibition opening on September 20 at the Herbst International Exhibition Hall at the Presidio in San Francisco. *Linus Pauling and the Twentieth Century* will run in San Francisco through November 7, and will then tour the country. The itinerary is not yet final, but will include a visit to the Los Angeles area in 1999.

Pauling's groundbreaking discoveries in chemistry and his tireless campaigning to limit the spread of nuclear weapons led to his being the only person ever to win two unshared Nobel Prizes—chemistry in 1954 and peace in 1962. "The exhibition demonstrates how scientific pursuits and efforts to minimize human suffering need not be mutually exclusive," says Linus Pauling, Jr., chair of the exhibit's advisory committee. "We hope [it] will serve as inspiration for new generations to meet humanity's challenges in the 21st century."

The exhibit is cosponsored by the Pauling family; Oregon State University, which houses Pauling's papers; and Soka Gakkai International, a lay Buddhist organiztion dedicted to world peace with which Pauling had close ties.

For further information, visit http://www.paulingexhibit.org.

ASTROPHYSICS THESIS MAKES (GRAVITY) WAVES

When you're beginning a career in cosmology, it's only fitting to start with a big bang. That's what Ben Owen (PhD '98) is doing—not only did he win the Clauser Prize for best dissertation, but his work has already been the subject of an international symposium. His dissertation answers a nagging, decades-old astrophysical question: Why do young neutron stars have such slow spins? The research, done with Visiting Associate in Theoretical Astrophysics Lee Lindblom and Sharon Morsink (a postdoc at the University of Wisconsin–Milwaukee), predicts that newborn, rapidly spinning neutron stars will pulsate wildly, throwing off their spin energy as gravitational waves.

Neutron stars contain roughly the mass of the sun, packed into a sphere about 15 miles in diameter. They are typically formed in the supernova explosions of massive stars. They also tend to spin like crazy. Astronomers infer this spin from a telltale "blinking" in radio signals or sometimes even a strobelike blinking in visible light. Based on the rate of blinking, we know that these particular neutron stars—known as pulsars—can spin as rapidly as 600 times per second. And, based on the laws of Newtonian physics, there's no compelling reason why a normal star shouldn't speed up to the fastest rotation rate possible once it goes supernova and collapses into a neutron star, just like ice skaters who pull in their arms to spin faster.

But all of the young neutron stars observed to date spin at 120 revolutions per second or less—a factor of five slower than the fastest known pulsar, which is very old and is thought to have been spun up long after the supernova event by other mechanisms.

Owen, Lindblom, and Morsink theorize that circulation patterns on the neutron stars create a sort of drag in space-time. Called "r-modes" because they owe their existence to rotation, these circulation patterns look much like the eddies that move oceanic currents in circular motions on Earth. Owen's dissertation shows that the r-modes emit gravitational waves that cause drag as they leave the star, slowing down its spin. The drag, in turn, causes the r-modes to grow when they would normally die away due to internal friction. If an r-mode sloshed material nearly from pole to pole, the neutron star should slow down to one-tenth its original rate of rotation within a year—to rates typical of those seen in the fastest young pulsars.

But the effect is a self-defeating one, Owen says. The r-modes are kept going by gravitational waves, which are stronger when emitted by rapidly rotating stars. But the gravitational waves leaving the star cause it to spin down, which makes the waves weaker, which in turn means there is less power to keep the r-modes going. So the neutron star eventually reaches an equilibrium. "If the r-modes get very large, they'll start radiating a lot of energy as gravitational waves," Owen says. "But they can't do that forever, because the rotational energy they're radiating is what keeps them alive in the first place."

Owen's work is purely theoretical at this point, but could be tested when LIGO is operational. (See page 8.) If a supernova goes off in our cosmic neighborhood—say, within 60 million light-years—LIGO should be able to detect the gravitational waves thrown toward Earth. If Owen is correct, "When a supernova occurs, we should first see the waves start very abruptly at up to 1,000 cycles per second, and then chirp down to about 100 or 200 cycles per second over the course of a year." ☐ *—RT*

Caltech's Avery House became Stanford's bookstore recently for an episode of *Party of Five*, the Fox-TV teen drama. Some house residents added a few Caltech touches—a *California Tech* newspaper rack and, on the Stanford-crimson bulletin board, nestled among the San Francisco Forensics League flyers and the jobs-wanted ads with 415 area codes, an Interhouse poster.

JPL LENDS STUDENTS AN EAR

A decommissioned antenna that used to talk to spacecraft is now talking to middle and high school students. The 34-meter dish, part of JPL's Deep Space Network (DSN) complex at Goldstone in the Mojave desert, has been converted into a radio telescope to be operated by the Apple Valley Unified School District's Lewis Center for Educational Research. (JPL will continue to maintain and upgrade the telescope.) The telescope can be controlled over the Internet, making it available to classes all over the country.

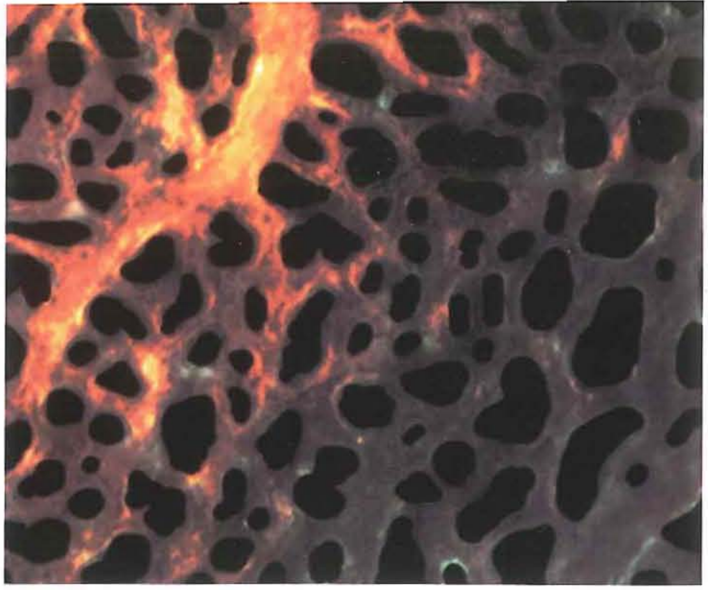
JPL is collaborating with the Lewis Center to develop science and technology curricula to go with the telescope. The first element, "Jupiter Quest," involves long-term studies of Jupiter's temperature, atmosphere, and radiation belts—a particularly appropriate assignment because during its 30-year DSN stint, the antenna kept tabs on Pioneers 10 and 11 and the Voyagers during their Jovian flybys. (It tracked Voyager 2 well past Neptune.) The students' observations are real science, complementing those being done elsewhere, and will eventually be published in journals. ☐

STARTING ARTERIES

When do arteries and veins become different from one another? According to Professor of Biology and Howard Hughes Medical Institute Investigator David Anderson, it has been assumed that these identities develop only after the circulatory system forms, presumably as a consequence of differences in such things as blood pressure and dissolved oxygen levels. But new work in his lab shows that this is not the case.

Anderson, postdoc Zhoufeng Chen, and grad student Hai Wang have discovered that the cells destined to form arteries and veins are already genetically distinct at the earliest stages of blood vessel formation in the embryo. Moreover, these budding arteries and veins "have to 'talk' to each other to develop properly," says Anderson. By "talking," he means that complementary molecules found on surfaces of primitive arteries and veins must interact with each other for proper blood vessel formation to occur. The findings may help explain how an intact circulatory system can be put into place before the heart even begins to beat.

Specifically, the Anderson team found that a molecule



Blood vessels in the yolk sac of a mouse embryo form a network of arteries and veins. The vessels link up with each other through molecular signals. The red-gold tones in this photo are from an antibody that binds to ephrin-B2, a cell-surface molecule found only in developing arteries; its receptor molecule, Eph-B4, is found only in developing veins.

Reprinted from Wang, et al., *Cell*, Volume 93, Number 5, pp. 741–753. Copyright 1998 Cell Press.

known as ephrin-B2, present on developing arteries, must communicate with its receptor, Eph-B4, present on developing veins. These proteins are made by endothelial cells, the cells that first form primitive vessel-like tubules in the embryo and then go on to form the inner lining of arteries or veins. This process appears to be fundamental—if it fails to occur, embryonic development ends almost as soon as the heart begins to beat.

The discovery occurred when Wang and Chen were performing a "knockout" experiment to see if the ephrin-B2 gene is essential for the development of the nervous system. When they eliminated the gene that codes for ephrin-B2 in mouse embryos, they found no nervous system defects, but did notice that there were defects in the developing vascular system and heart.

Fortunately, Chen and Wang had inserted a "marker" gene that makes cells turn blue where the ephrin-B2 gene would normally be turned on. They found, surprisingly, that the gene was active in arteries but not in veins. They then showed that the receptor gene, Eph-B4, was active in veins but not arteries. Eph-B4 and ephrin-B2 fit together like a lock and key, signaling each cell that the other has been engaged. This complementarity was seen on vessels throughout the developing embryo. The fact that elimination of the ephrin-B2 gene caused defects in both arteries and veins suggests that not only do arteries send a signal to veins via ephrin-B2, but that veins must also signal back to arteries. The fact that both ephrin-B2 and Eph-B4 span the cell membrane suggests that each protein may be involved in both sending and receiving a signal.

The findings may have broad implications, Anderson suggests. "One should reconsider the molecular biology, pathology, and drug therapies of the vascular system in terms of the molecular differences between arteries and veins." It is likely, says Anderson, that arteries and veins will differ in their expression of many other genes that have yet to be discovered. Such genes may lead to the development of new artery- or vein-specific drugs, or may help to target known drugs specifically to either arteries or veins. The research appeared in the May 29 issue of the journal *Cell*. □—*RT*

MAYBE THEY'LL MAKE A MOVIE ABOUT THIS ONE, TOO

You've seen *Armageddon*. You've seen *Deep Impact*. Now read the paper in the May 22 issue of *Science* that describes geochemical evidence from a rock quarry in northern Italy that indicates that a shower of comets hit Earth about 36 million years ago.

The findings not only account for the huge craters at Popagai in Siberia and at Chesapeake Bay in Maryland, but posit that they were but a tiny fraction of the comets active during a span of two or three million years during the late Eocene period. The work provides indirect evidence that a gravitational perturbation of a cloud of comets beyond Pluto's orbit, called the Oort cloud, was responsible for sending a wave of comets swarming toward the center of the solar system.

The authors, from Caltech, the U.S. Geological Survey's Flagstaff office, and the Coldigioco Geological Observatory in Italy, say their evidence points to a very large increase in the amount of extraterrestrial dust hitting Earth in the late Eocene period. The team included Gene (BS '47, MS '48) and Carolyn Shoemaker; Gene died in a car crash last year while the research was in progress. (See *E&S*, 1997, No. 3.)

According to Associate Professor of Geochemistry Ken Farley, the lead author of the paper, Shoemaker's contribution was crucial. "Basi-

A 65-million-year-old dusting of iridium—rare on Earth but common in asteroids—was first discovered in samples taken from this road cut near Gubbio, Italy. This iridium layer has since been found worldwde—strong evidence that one or more asteroid hits nuked the dinosaurs. From right: Gene Shoemaker takes samples to look for ^{3}He, while Alessandro Montanari looks on and Carolyn Shoemaker keeps the sample log.

cally, Gene saw my earlier work (see *E&S*, Summer 1995) and recognized it as a new way to test an important question: Are large impact craters on Earth produced by collisions with comets or asteroids? He suggested we study a quarry near Massignano, Italy, where seafloor deposits record debris related to the large impact events 36 million years ago. He said that if there had been a comet shower, the technique I've been working on might show it clearly in these sediments." Carolyn Shoemaker adds that she and her husband went to Italy last year to perform field work in support of the paper.

The technique measures the helium isotope known as ^{3}He, which is rare on Earth but common in extraterrestrial materials. ^{3}He is very abundant in the sun, and some of it is ejected as part of the solar wind. The helium is easily implanted into and carried along by such extraterrestrial objects as asteroids, comets, and their associated dust particles. Thus, arrival of extraterrestrial matter on Earth's surface can be detected by measuring the matter's associated ^{3}He.

The ^{3}He-bearing material is unlikely to include large objects like asteroids and comets. Because these heavy, solid objects fall into the atmosphere with a high velocity, they melt or even vaporize. The liberated ^{3}He mixes with the atmosphere and is ultimately lost to space—it never gets trapped in sediment.

But tiny particles are another story. They can slowly pass through the atmosphere at low temperatures, retaining their helium. These particles accumulate on the seafloor as part of the sediment, providing an archive going back hundreds of millions of years.

Elevated levels of ^{3}He would suggest an unusually dusty inner solar system, possibly because of an enhanced abundance of active comets. Such an elevated abundance might arise when a passing star or other gravitational anomaly kicks a huge number of comets from the Oort cloud into elliptical, sun-approaching orbits.

And indeed, samples from the Italian quarry showed an elevated flux of ^{3}He-laced materials in a sedimentary layer some 50 feet beneath the surface—a depth that suggested that the ^{3}He had been deposited about 36 million years ago. This corresponds to the dating of the craters at Popagai and Chesapeake Bay.

More precisely, the ^{3}He measurements show enhanced solar system dustiness associated with the impacts 36 million years ago, but with the dustiness beginning 0.5 million years before the impacts and continuing for about 1.5 million years after. Thus it appears that there were a large number of Earth-crossing comets, and much dust from their tails, for a period of about 2.5 million years.

In addition to the Shoemakers and Farley, the paper was cowritten by Alessandro Montanari, who holds joint appointments at the Coldigioco Geological Observatory in Apiro, Italy, and the School of Mines in Paris. □—*RT*

SOME DAY MY PRINTS WILL COME

The punk rats on the cover of the last issue were a big hit—several people have asked if reprints were available. Artist Erika Oller has graciously given us permission to sell prints at cost—approximately $25.00 each. Call us at (626) 395-6730 for details.

MAKING MEMORIES STICK IN YOUR HEAD

A sticky molecule found at the junctions of brain cells may be a crucial chemical ingredient in learning and memory, according to Assistant Professor of Biology and Howard Hughes Medical Institute Investigator Erin Schuman. In the June issue of *Neuron*, she, grad student Lixin Tang, and Chou Hung (BS '96) report that a calcium-dependent family of molecules known as cadherins plays a significant role in chemically joining the synapses—the junctions of nerves. Neuroscientists believe that the environment of the synapses is where memories are stored.

Cadherin molecules span the cell membrane, with protrusions, or domains, that stick out into the synapse, which is actually a gap 10–20 nanometers wide—an easy reach for a protein molecule—between adjoining cells in a neural circuit. Since cadherins are found on both sides of the synapse, they "may form a sort of zipper-like structure at the junction of the presynaptic cell and the postsynaptic cell," says Schuman. "We show in this study that these molecules participate in making the synapses bigger and stronger, a process called 'long-term potentiation' that may be involved in memory storage."

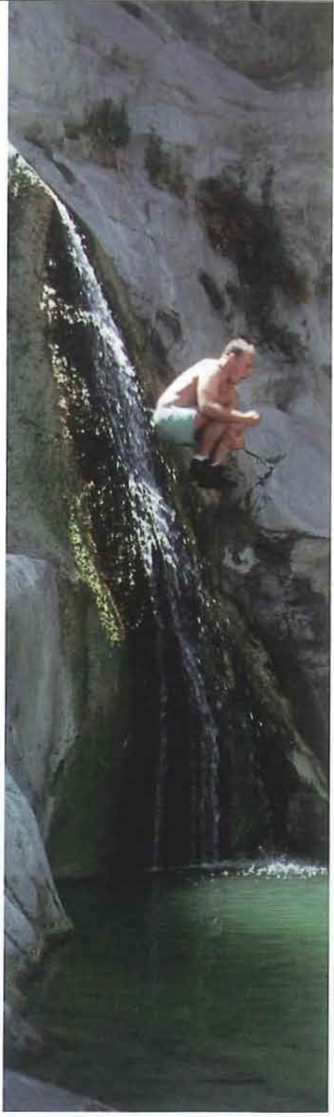
The research involved turning off the cadherins in the brains of adult rats and mice to see what effect that had on long-term potentiation. "It has been known for some time that cadherins are important during early development," says Tang. "But they are also expressed well into adulthood. So we were interested in seeing what would happen when cadherin was disrupted in the adult brain."

The researchers shut off the cadherins by either introducing antibodies that bound to the part of the cadherin molecule that sticks out into the synapse, or with an inhibitory peptide that mimicked the real cadherin's presumed binding site. Either way, the interloper got stuck between the zipper's teeth, as it were, and long-term potentiation was significantly reduced.

However, the synapses' overall signal transmission rate and their structural integrity were unchanged by the antibodies. This would indicate that the cadherins are used very specifically by the nerves for changing the strength of synapses, but not for the basic transmission of nerve impulses.

And the inhibitory peptides were only effective in shutting down long-term potentiation if they were introduced at the beginning of the process. When the peptides were introduced about 30 minutes afterward, they had no effect. This suggests that there may be factors other than the cadherins involved in long-term potentiation, and that these factors cannot be blocked by the peptides, Schuman explains. Like the antibodies, the peptides have no effect on baseline signal transmission or structural integrity when they disrupt the cadherins.

Cadherins require calcium ions in order to stick to one another, and it's known that calcium ions temporarily leave the synaptic junction during nerve impulses. So perhaps the calcium's departure momentarily destabilizes the cadherin-cadherin bonds, allowing the peptides to block long-term potentiation. Schuman and colleagues found that adding calcium ions to the solution bathing the nerve cells protects the cadherins from the inhibitory peptides. This suggests that cadherins might be able to work as "activity sensors" outside nerve cells, changing their binding behavior in response to changing calcium levels. □—*RT*



Astronaut candidate Reisman experiences weightlessness of another kind while hiking with Chris Brennen in the San Gabriel Mountains.

AND HE'S BUILDING A STAIRWAY TO HEAVEN

When Garrett Reisman (MS '92, PhD '97) was a grad student here, he was president of the Caltech Flying Club. Now he hopes to trade in his Cessna for a space shuttle—he is one of the latest crop of 25 astronaut candidates chosen from across the country to begin a yearlong training and evaluation program at the Johnson Space Center in August. Reisman will be training as a mission specialist. "We don't actually fly the shuttle; we're the guys who sit in the back and operate the payload. But we do get to do other cool stuff—we take spacewalks and run the robot arm. The shuttle drivers don't—they have to stay at the controls to keep the shuttle in the right relative position." Although it's far too early for him to have been assigned to a specific mission, he notes that his class of trainees will be the ones building and maintaining the International Space Station.

Reisman, who got his degrees under Professor of Mechanical Engineering Chris Brennen, credits Brennen with helping land him the appointment. "He wrote lots of letters of recommendation, and he told them about all the things we had to do to keep the water tunnel going. When you're an experimentalist, you have to do a lot of problem solving. I did plumbing, I changed out pumps, I did a lot of stuff besides just running my experiments. They were looking for mechanical and aeronautical engineers with hands-on experience, and the cavitation work I did at Caltech probably made the difference." □

One of the dual instruments of the Laser Interferometer Gravitational-Wave Observatory sprawls across the desert near Hanford, Washington, each arm extending four kilometers and meeting at the corner of an L. The support buildings at the corner house laboratories as well as electronic and optical equipment, which will send a laser beam, split in two, back and forth down the two arms to intercept the infinitesimally small signal of a gravitational wave.

> "LIGO represents the transition of a field from small science to big, and as such is an important case study. It was a transition done largely internally at Caltech—and, in the end, done very successfully."

Realizing LIGO

by Jane Dietrich

Stretching across flat, empty desert in central Washington State (where it's easily seen on commuter flights), and mirrored on Louisiana's timbered coastal plain, a pair of gigantic L-shaped structures lie in wait for something that no one has ever seen. Along their two-and-a-half-mile-long arms run tubes containing one of the world's largest vacuum systems (the volume equivalent of about 15,000 kitchen refrigerators), in which laser beams will bounce back and forth anticipating the slightest jostling that would indicate the arrival of a cosmic signal. The tubes, four feet in diameter—you could walk through them crouched over—are constructed of a ribbon of 1/8-inch-thick stainless steel, rolled up like a toilet-paper roll and spiral welded along the seams. Continuous arches of six-inch-thick concrete cover the beam tubes, protection from the rattling desert wind as well as hunters' stray bullets; tumbleweeds pile up along the arms and must be harvested regularly with a hay-baling machine lest they ignite a conflagration.

This is LIGO, the Laser Interferometer Gravitational-Wave Observatory, at $371.3 million ($296.2 million for construction alone) the National Science Foundation's most expensive project, and one that comes with no sure-fire guarantee. When it turns on in the year 2002, LIGO will be searching for a signal as small as a thousandth of the diameter of a proton.

What are gravitational waves and why should we spend hundreds of millions of dollars to try to see them? Deduced by Albert Einstein in 1916 as a consequence of his general-relativity laws of physics, gravitational waves are ripples in the curved fabric of space-time, generated when huge masses precipitate violent events—when supernovas explode or black holes collide, for example. The gravitational energy released squeezes the warp and stretches the woof (or vice versa) of that fabric as it ripples outward, weaving a legible tapestry of the universe's cataclysmic events. But by the time the edges of this ripple reach Earth, the signal is extremely faint—near the edge of detectability by today's human technology.

If scientists can detect the signal, they may be able to discern some of the 90 percent of the universe that is hidden from the view of current instruments—optical and radio telescopes, X-ray and gamma-ray detectors—all of which explore only the electromagnetic spectrum. Deciphering gravity waves could show how two black holes engulf each other and reveal the mechanisms of a collapsing star. The gravitational equivalent of cosmic background radiation, created when the universe was less than a billionth of a second old, could help us decipher the details of the birth of the universe.

But do gravitational waves even exist? How do we know Einstein was right? Scientists interested in the phenomenon got lucky in 1974, when Joseph Taylor and Russell Hulse at the Arecibo Radio Astronomy Observatory in Puerto Rico observed two neutron stars (very dense balls of neutrons, the remnants of dead stars) orbiting each other. One of this pair was a pulsar, sending out a regular radio beam that allowed Taylor to measure precisely the drifting period of the signal and to calculate that the drift was exactly what should come from the orbit's losing energy by radiating Einstein's gravity waves. By 1974 the search for gravity waves was already under way, but Taylor's discovery reinforced the conviction among the searchers that they were indeed looking for something real.

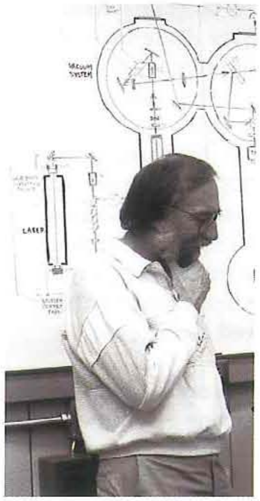
When scientists began their search, they didn't know how strong or how frequent gravity waves would be, and how sensitive their instruments would have to be to observe them. There was no precedent; it was virgin territory. Over the almost four decades after the search began, the need for more and more sensitive detectors eventually took it out of the laboratory and transformed it into "big science"—perhaps too big, some have said, for an institution like Caltech. The transformation was not accomplished without growing pains, as different scientific styles clashed and management methods were superseded, and as the difficulty of the task challenged some of the traditional ways of doing science, producing culture shock on a campus where most science has been done in small groups.

In the beginning was Joseph Weber of the University of Maryland, the acknowledged father of the field. In the early 1960s he built a detector based on a multi-ton aluminum bar, which may or may not (all experts now agree, not) have oscillated to incoming gravity waves in 1969, but his experiment inspired groups of physicists around the world, many of whom are now united in LIGO.

In 1963 Kip Thorne, then a graduate student at Princeton, met Weber and became fascinated with gravity waves. Thorne was a member of the theoretical relativity community, a field that theorized about black holes but had little contact with experiment. Arriving at Caltech in 1966, Thorne began spearheading an effort among theorists to convert his field into an observational one. We had "this beautiful theory of black holes," he says, "and no experimental data on the black holes themselves." Thorne considered gravity waves an ideal tool for observing black holes. To further that goal, he became "house theorist" for a talented group of experimentalists building bar detectors in Moscow under Vladimir Braginsky, who had been inspired by Weber.

Ron Drever, at the University of Glasgow, had also heard Weber lecture and decided to try to build better detectors. (Three decades later, Drever admits that if he had known how difficult it was going to be, he might never have gotten into gravity-wave detection; "but I thought it was going to be much easier than this.") Rainer (Rai) Weiss at MIT was also excited by the new field, and in 1970 had already come up with the concept of an interferometer-type detector (which was very much along the lines of what is now stretching across the flats of Washington and Louisiana). Weiss analyzed these detectors—figured out the noise sources the interferometers would have to confront and devised promising ways to deal with them. "Rai saw, right from the beginning, all the noise sources that today constrain LIGO," says Thorne. "His prescience was remarkable." Weiss, however, couldn't sell his ideas to MIT or NSF and was not able to get funding to build a prototype of his detector.

Kip Thorne

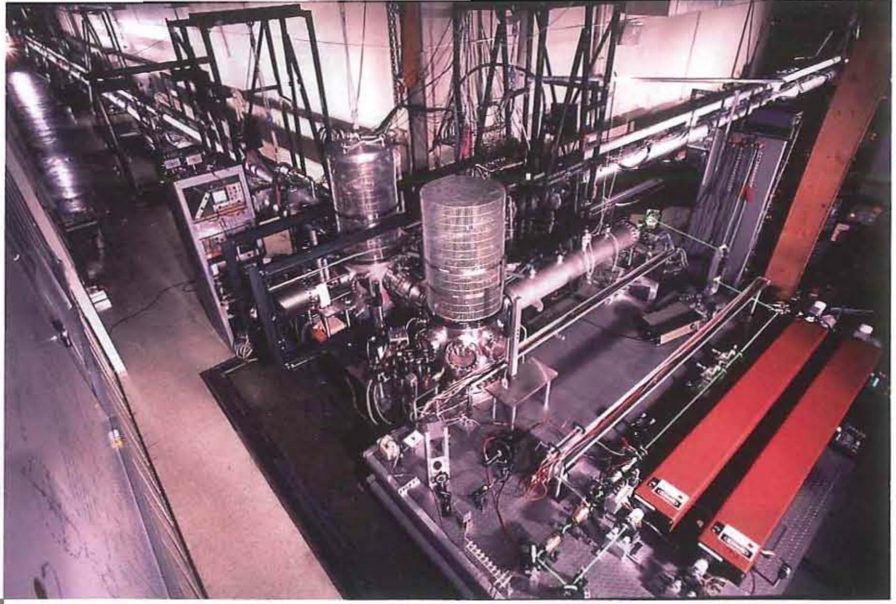


As two black holes orbit each other, in this representation of the curvature of space, they create outward-propagating ripples of curvature called gravitational waves.

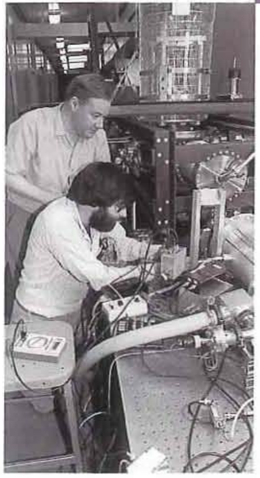
"I thought it was going to be much easier than this."

Meanwhile, back at Caltech, Thorne (who is now the Feynman Professor of Theoretical Physics), decided to urge the Institute to get into gravity waves. His 1976 proposal was supported with enthusiasm by a faculty committee consisting of Barry Barish, Alan Moffet, Gerry Neugebauer, and Tom Tombrello, and was ultimately endorsed by the Division of Physics, Mathematics and Astronomy and by the administration. The decision was made to mount a strong effort in this new field—to build a prototype detector and to bring in an outstanding experimental physicist. The call went out to Drever, whom Thorne described as "highly creative, inventive, and tenacious," qualities that were deemed necessary to the project. Drever, who was known for his skill at designing things that work, had grown pessimistic about the capabilities of bar detectors and was starting to experiment with interferometers. He was loath to abandon his work in Scotland, but he saw the possibility of building a larger prototype in Pasadena. Before making the decision to move permanently to Pasadena, he agreed to a five-year arrangement: half time at Caltech and the other half in Glasgow, where he was building a 10-meter prototype interferometer. (After the five years he became a full-time professor of physics at Caltech.) When the design of Caltech's 40-meter interferometer got under way, "I did most of the drawings on the plane flying over the pole," says Drever.

In an interferometer, free-hanging test masses placed at the corner and ends of an L would theoretically move when a gravitational wave passed by, stretching apart infinitesimally along one arm of the L and squeezing together infinitesimally along the other arm. This motion can be detected by laser light. A laser beam, split in two at the L's corner, travels down each arm and back—a shorter distance along the squeezed arm than the stretched one. When recombined at the L's corner, the two beams interfere, producing a



The prototype interferometer, a hundredth the size of the monster on page 8, was begun in the early '80s on the Caltech campus. The green laser beam generated from the optical setup at lower right enters the system through the horizontal pipe (center), and from the beam splitter in the mesh cage is bounced down the 40-meter arms.



Ron Drever and Stan Whitcomb (foreground) constructing the 40-meter prototype in 1983.

change of light intensity that reveals the arms' stretch and squeeze, and thence the gravity wave. (A vacuum inside the arms minimizes scattering and gives the laser beam the clearest possible path.) To maximize the signal strength, the arms of such an interferometer should be as long as possible, ideally even thousands of kilometers, which of course is not practical—on Earth anyway; in space is a different matter.

Former Weber student Robert Forward and a group at Hughes Research Laboratories built the first laser interferometer detector in the early '70s, but never continued with the project. Also during the '70s, Weiss at MIT and a group in Garching, Germany, were developing approaches and improving techniques in interferometer design. In his seminal 1970 work, Weiss came up with the idea, which the Germans eventually built, of hanging mirrors on the test masses and bouncing the laser back and forth many times between them, in effect "lengthening" the arm. If the light bounced hundreds of times between the mirrors, its total travel distance could be a quarter of a gravity-wave wavelength, with arms just a few kilometers long rather than thousands.

Finding a site on a small, compact campus in Pasadena to build even a prototype (no one knew yet just how big it had to be) posed a problem for Drever's undertaking. It was Robert Christy, then acting president of Caltech, who suggested wrapping the arms (in a sort of lean-to shed) around two sides of the already existing Central Engineering Services building on Holliston Ave. The length of 40 meters for the arms was fixed, says Drever, not by any theoretically ideal number, but by a tree in the way that no one wanted to cut down. Caltech put half a million dollars into the project. The staunch institutional support on Caltech's part, along with strong backing from a blue-ribbon committee convened by the National Science Foundation, swayed the NSF to throw its weight and money behind the project.

Stan Whitcomb, a former infrared astronomer, joined the project as assistant professor in 1980 and directed construction of the prototype, which was largely put together by undergraduates and graduate students (see *E&S*, January 1983). What attracted him to something so speculative as gravitational waves? "The challenge of building a detector that's so sensitive that you can't imagine that it has a hope of being successful," says Whitcomb. "And also the intellectual excitement of seeing something where the theorists don't have a good prediction for what we might see." Like Drever, Whitcomb says he "probably didn't realize how really difficult it was going to be." "In a sense we've been saved by technology developments that occurred after the start of this project," he continues, "things we didn't know about."

In the late '70s and early '80s, according to Thorne, "Ron was generating wonderful ideas—a lot larger share than you would expect for any one individual." Drever wanted to improve on Weiss's mirror scheme, which would need very large mirrors, so he hit on the idea that each interferometer arm should be a Fabry-Perot optical cavity, in which the laser light would bounce back and forth hundreds of times from the same spot on each mirror (instead of the separate, discrete spots in Weiss's scheme). Although this was technically more difficult, it had the advantage of allowing the mirrors to be much smaller. "It seemed to me economical," says Drever. "The mirrors have got to be cheap." Unfortunately, at that time Fabry-Perot interferometers typically worked only over a distance of a few centimeters, because lasers couldn't be made sufficiently stable in frequency to use larger distances. So, even though it was an accepted "fact" at the time that a laser could never be stabilized with the accuracy that the interferometer required, Drever devised a solution. He invented an optical-band technique that locks the laser onto the normal-mode oscillations of a large physical system, a technique similar in principle

Far right: The 4-inch-thick, 10-inch-diameter mirrors at the ends of the beam tubes recycle the laser light. The polished mirrors are coated with up to 35 layers of a purple dielectric coating designed to achieve the right reflectance and transmission of light for the wavelengths used by LIGO. The final coating was put on in May. Right: The front entrance to the main support building at Hanford has been landscaped since this picture was taken.

to one that Robert Pound at Harvard had originally developed for microwave frequencies. Now called Pound-Drever locking, it's used widely in laser spectroscopy and other areas of science and engineering.

Drever also (the German group thought of it independently) came up with the idea of recycling the light, so that it actually builds up and becomes more intense as it bounces between the mirrors. "We were very lucky in a sense because we found some wonderful mirrors that had been developed for military applications," says Drever. These mirrors with very small losses were still "kind of semi-secret," but Drever managed to get hold of some samples, which turned out to be perfect for his technique. "With these wonderful mirrors, you don't need to actually lose the light. We could pass the light through the system again and again and again, maybe hundreds of times. The net effect was that you could make a much more sensitive system with the same laser."

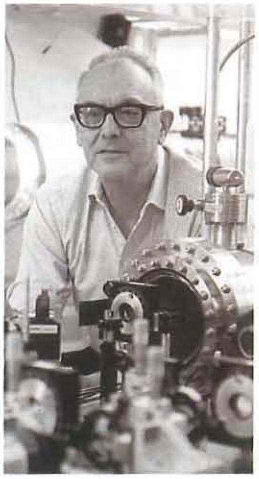
With Drever and Whitcomb building their 40-meter prototype, NSF refused to fund a similar prototype at MIT, but encouraged Weiss's desire to proceed with bigger plans, in space as well as on the ground. Weiss was thinking in terms of kilometers rather than meters. While a meter-sized instrument would be fine for testing techniques, it was highly unlikely to achieve the sensitivity necessary to detect gravitational waves. (On the other hand, scaling up by a factor of 100 is not easy; the rule of thumb in experimental physics is to enlarge subsequent generations of an experiment by a factor of 3 to 10.)

In 1983, Drever, Weiss, and Thorne together talked with Richard Isaacson and Marcel Bardon at NSF about building two kilometer-scale interferometers: a Caltech interferometer, and an independent MIT interferometer, which might cooperate in their gravity-wave searches. While Bardon and Isaacson embraced the prospects for such instruments, they insisted that any such project must be a truly joint Caltech/MIT undertaking, with the two groups working together on all aspects of a single, unified design.

The result was a "shotgun marriage"—Thorne's words, though Weiss, realizing that for something on this scale collaboration was necessary and unavoidable, didn't resist. Since MIT's administration had far less enthusiasm for the enterprise than Caltech's, the center of gravity waves moved west to Pasadena under a steering committee made up of Drever, Weiss, and Thorne. This was hardly a perfect union; there were strong disagreements between Weiss and Drever over technical matters in particular and scientific style in general. Drever was generally considered an "intuitive" scientist, while Weiss was deeply analytical. Weiss had worked on large projects with all their sharing and delegation of power; Drever had not, and was more accustomed to individual work. And Thorne, the committee's chair, wasn't an experimentalist at all. Decisions had to be made by consensus, and each was reached slowly, with great debate and agony. Under this rickety (Weiss's word) troika, the gravity-wave project stayed afloat with NSF support for another couple of years, basically as an R&D enterprise. Applications for funding to build the full-scale interferometer were twice turned down due to insufficient referee enthusiasm.

On the enthusiasm front, things started to look even worse in the summer of 1986, when Richard Garwin, an influential physicist who had served on numerous government advisory committees, voiced his suspicions of the grand claims for interferometer technology and demanded that NSF commission a thorough study of the project. A committee of scientific heavy hitters, cochaired by Andrew Sessler of UC Berkeley and Boyce McDaniel of Cornell, then met in November for an intense week of presentations and deliberations, deciding almost from the beginning that the technical and scientific challenges *were* worth NSF's support. The committee strongly endorsed the project, including the Caltech/MIT proposal to go whole-hog and build two full-scale interferometers at the same time instead of sequentially. (It would be very difficult to detect gravity waves with just one; a coincidence between two is required to separate any real signal from the noise.)

But the committee chairs also took note of the profound management problems and, as urged by Weiss and others, demanded a new organizational structure: a cohesive team with a single strong leader who had the authority to make decisions without consensus. Enter Rochus (Robbie) Vogt, the Avery Distinguished Service Professor, former provost, former division chair, former chief scientist at JPL (where he continued to lead the Voyager cosmic-ray experiment team), and former acting director of the Owens Valley Radio Observatory. After months of pleading by Drever, Weiss, and Thorne, Vogt finally agreed to sign on as the project's director and principal investigator. "Without Weiss or Drever, this thing would never have even started," says Thorne, "but without Robbie it would never have taken off."


Robbie Vogt

The project that Vogt took command of in 1987 was still in reality two groups, which had developed completely different research programs with much duplication of effort and no common design. Said one member of the project, "Every time somebody wrote a working paper, somebody from the other institution wrote a dissenting working paper." This stopped under Vogt, who became a hands-on manager with a vigor that shocked some, but shook new life into the project. He made some hard decisions and shut down some research activities at MIT (he chose Drever's optical design and Drever's type of laser), but within a couple of years he had organized everyone into working toward a common goal. Not *just* a manager, Vogt was intimately involved in the scientific and engineering design of the project.

"It's Caltech's great fortune that Robbie was able to pull something together that could go forward," says Whitcomb, who had left the project in 1985, believing it could not happen under the conditions that prevailed at that time. Whitcomb returned as deputy director in 1991 after Vogt had resurrected what had now been christened LIGO. "Basically, he brought together a group of scientists who had never built something on this scale and led them to a conceptual design that was down-to-earth, quite practical, and used real technology," says Whitcomb. "He had to educate scientists without much practical real-world experience in what it means to design a piece of scientific hardware on a reasonable scale and have something that's practical to build."

Under Vogt's firm guidance, the new organization submitted a proposal for a full-scale interferometer that—after strong endorsement by outside reviewers—NSF accepted (see *E&S* Summer 1991). But while NSF could approve projects, an undertaking of this size and cost also required the blessing of Congress, which had to vote on providing the funds. Vogt then took on Washington. Admittedly a relative novice in government appropriations, Vogt in 1991 linked up with Hall Daily, Caltech's director of government relations (although "government" then most often meant Pasadena city hall), and the two set out to convince Congress to give them $47 million for the next fiscal year. The House appropriations subcommittee promptly zeroed it out, to the shock of NSF, which had expected the battle to come later, in the Senate. The project stayed alive on a fortuitous appropriation of $500,000, and in the winter of 1992, Vogt and Daily, along with newly hired consultant April Burke (to be their

Above: The vacuum tanks arrive at Hanford in August 1997. Right: By February 1998 the vacuum equipment is installed at the point of the L. The center vertical tank houses the beam splitter. At right one of the beam tubes takes off into the desert.

"eyes and ears" in Washington), set to work in earnest. Resistance in the House remained strong. There was philosophical opposition to the NSF's involvement in big-science projects. In comparison to the Superconducting Super Collider and the space station, LIGO wasn't really "big," but NSF had never attempted anything on this scale before. In addition, some in the scientific community opposed it. Some doubted that an instrument capable of doing the job could be built with 1990s or 2000s technology; and others, particularly astronomers, were disturbed by the risk that LIGO's first interferometers would see nothing, and thought the money could better be spent on electromagnetic telescopes, where success could be far better assured. Though NSF insisted that shifting the money to electromagnetic telescopes was not an option, many doubted that claim.

Vogt had in the meantime worked out a realistic budget, now totaling $220 million ("really an economy price," says Vogt), a budget that was embraced by NSF, which had been skeptical of previous estimates. Finally, there was a real, legitimate, bottoms-up estimate for what this thing would cost. It was divided into stages, with a "ramp up" period as construction demanded most of the funds, and a "ramp down" stage as construction costs tapered off and operations and advanced research and development that would require less money took center stage. The Caltech team also needed a strategy. To succeed in Washington, according to Daily, "you have to grab personal attention, tell a true story, and make a case that there's value attached to spending taxpayer money for some purpose." And Vogt became a master at selling his story. "Robbie was astounding," says Daily, at persuading people and delivering clear explanations of what gravity waves were all about. "He treated congressmen and staffers alike with respect and didn't talk down to them."

"It was a delight to talk to [Sen. Johnston]. . . . He was genuinely interested in science and cosmology." He was also chairman of one of the 13 appropriations subcommittees. . . .

Besides eyes and ears, LIGO needed a champion for its cause in Washington. It found one first in Sen. George Mitchell of Maine, and later, in 1993, after Maine had lost out in its bid for a LIGO site, in Sen. J. Bennett Johnston of Louisiana, whose state had been more fortunate. (The winning sites were Hanford, Washington, and Livingston Parish, Louisiana—both sufficiently remote, quiet, and above all, flat.) When Burke wangled a 20-minute audience with Johnston, Vogt so intrigued the senator with his cosmology tales that Johnston canceled his next appointment, and the two of them ended up sitting on the floor with Vogt drawing pictures of curved space on the senator's coffee table. "It was a delight to talk to him," Vogt says. "He was genuinely interested in science and cosmology." He was also chairman of one of the 13 appropriations subcommittees (and remained the ranking minority member after 1994). Although Johnston was unable to save the Superconducting Super Collider, which he had also championed and which Congress shot down in 1993, he was successful in getting LIGO through the Senate.

Louisiana turned out to be providentially well positioned in the House as well as in the Senate. Congressman Bob Livingston, in whose district the LIGO site lay (before it was redistricted out) and whom Vogt had already converted into a cosmology fan, became chairman of the House Appropriations Committee after the Republicans acceded to the majority position in Congress in 1994. LIGO continued on a roll, and its full funding looked assured.

For Vogt, Washington was a good experience. "I came back with a much more positive view of Congress than I ever had. And the reason is, I met many good people who worked very hard, who were idealistic and wanted to make things work—staffers in particular, but also congressmen and senators, who had absolute integrity and worked very, very hard under difficult conditions. I came back with much more respect for the system than I had before."

Meanwhile, however, back at Caltech, LIGO was outgrowing Vogt's team. The rules for big science, or even sort-of-big science, were changing, and NSF now favored a different kind of structure, one akin to the large, management-intensive organizations that built accelerators for high-energy physics. Vogt's project was lean on management; he likens its style to that of Lockheed's famous Skunk Works: "You basically build a project and you build a wall around it and say, 'Throw the money over the wall.' And in *n* years, I break the wall down and deliver a beautiful thing—an airplane or a LIGO." In 1994, NSF let Caltech know that it wasn't too keen on just throwing money over Vogt's wall, and Caltech's president, Tom Everhart, responded with another change in leadership.

(Vogt's close-knit and intense research group had also developed internal problems. Drever, whose research style some thought incompatible with the ever-larger-growing project, was separated from the project, triggering outrage and controversy among the Caltech faculty. When the dust finally settled, Drever had been promised his own independent laboratory, funded separately from LIGO.)

Barry Barish, the Linde Professor of Physics, succeeded Vogt. The end of the Vogt era was painful for just about everybody, but today Vogt waxes philosophical about his departure. Barish,

As seen from inside (above), a beam tube, at four feet in diameter, is almost big enough to stand in, hunched over. The tubes are laid out in sections on a concrete slab (right) and then covered with sections of concrete arch, six inches thick, seen here partially finished at the Louisiana site. The completed tunnel, in the less verdant expanse of Washington (above right), has access entrances every 250 meters.

he says, "represents what today is politically acceptable, and I represent what is politically no longer acceptable. That doesn't make him right and me wrong or vice versa. It just is a fact that life has changed, and some of us refuse to change —because maybe someone ought not to compromise, and some of us are in the fortunate position of being able to decline to do so." Vogt remained with LIGO until the summer of 1997, setting up the organization and methods for designing and constructing LIGO's first interferometers, initiating the design, doing cost reviews, and acting as mentor to a group of SURF (Summer Undergraduate Research Fellowship) students working on LIGO. Now he talks about doing something completely different—perhaps indulging a long-time desire to become a "gentleman scholar." He's also coteaching a course in the fall, with Associate Professor of History Diana Barkan, on the development of big science in the 20th century and the technological, social, and political factors that have altered the way science is practiced.

Recast in the image of a high-energy physics project, LIGO has thrived. Barish happens to be a genuine experimental high-energy physicist, and in 1994 had just recently returned from the biggest science of all—the $3-billion-plus SSC, where he was leader of one of the SSC's two detector groups. Barish also had roots in gravity waves, having been a member of the original committee that first recommended that Caltech get involved.

When Barish took over LIGO, he applied a lesson learned from the SSC—that two simultaneous sets of management, one for the construction and one for the scientific laboratory, only clashed with each other. "I wanted to make it as simple as possible," says Barish, "and the first task was just to build the thing. So I wanted a simple project management, a structure that was as unimaginative as you could possibly be—the kind of organization that builds a bridge." Barish assumed the title of LIGO's principal investigator, but left the title of laboratory director vacant until such time as the "bridge" was finished.

Everyone's favorite word to describe Barish's organization seems to be "robust" (as opposed to Vogt's Skunk Works, which was termed "fragile"). When LIGO, at full ramp-up, reached a certain size, it needed, says Thorne, "a robust management and a robust organization that could deal simultaneously with the construction of facilities, the R&D and planning for detectors, the pressures from the funding agencies, the pressures from the Caltech and MIT administrations and faculty, the continual reviews that the funding agency feels are necessary to insure success, and so forth." Barish's capacity to juggle all these things stems from his ability to delegate great amounts of authority, which consequently demands a larger management staff—not so "lean and mean" as its predecessor. This would cost more money, but NSF and Congress, realizing that it was necessary for the success of the project, accepted the increased costs without a whimper.

From the SSC Barish brought in Gary Sanders, another experimental high-energy physicist, to be project manager of LIGO. As project manager for one of the SSC's detectors, Sanders is, like Barish, one of a fairly small group of scientists who have had the unique experience of running big projects. As Sanders describes it, "A few of us have learned how to act like builders for a few years, and then stop and act like scientists for a few years, and then maybe go on and be builders again."

To act like a builder can often mean not acting like a scientist. A construction project, says Sanders, "is driven by schedules, the need not to fall behind—and the need *not to be too clever* and try to improve things. You have to think: this is what you're going to build; it's good enough to do the job; it's what you promised to build; build *this* and not a new idea that you just had yesterday. This is antithetical to what you do in the laboratory when you're doing research. There you strive each day for the best possible thing."

In June 1996, the contractor Chicago Bridge & Iron moved onto the Hanford site, in less than a year erected the total of eight kilometers of beam tubes in clean-room conditions (the stainless steel itself is so clean that it doesn't leak hydrogen into the vacuum inside the tubes), and then moved on to do it all over again in Livingston Parish, where it should be finished this summer. The support buildings are basically finished at both sites, and at Hanford the resident staff of 12 (and growing), under the direction of Fred Raab, has moved in, grateful for flush toilets at last. Operating funds (separate from construction funds) are already paying the utility bills. Recently, the high-precision seismic isolation system to shield the suspended mirrors from vibration has arrived at Hanford.

Besides containing scientists' offices and labs for optics, electronics, and vacuum systems, the main buildings (which are virtually identical at both sites) have a multipurpose area for lectures and visitor programs. The Livingston site, under the direction of Mark Coles, will put particular emphasis on educational outreach, with a museum and interactive exhibits.

All this construction represents the bulk of the expense of building LIGO, and although the technical achievement of creating one of the largest ultrahigh-vacuum systems on the planet is no mean feat, the real excitement and the intellectual challenge of the interferometer itself is just beginning. The vacuum equipment is currently being installed and tested. The detectors, being designed and built under the direction of Stan Whitcomb, are about a third of the way through fabrication. The first parts of the laser system have arrived at Hanford, but other detector bits are scattered at assorted manufacturers and institutions—including MIT and the University of Florida, as well as Caltech—in various stages of fabrication. About half the optics were finished as of this summer, many of the pieces residing in basements on the Caltech campus. The first mirrors, 10 inches in diameter and 4 inches thick, got their final coating in May. Some of the electronic components, such as the control systems and data-acquisition systems, are still in the final design stages. The integration of all these parts—pulling them together and getting the whole thing to work with the sensitivity it's been designed for—over the next couple of years is going to be the most challenging and the most difficult part of building LIGO, according to Sanders.

During the transitional phase, as construction is completed and operation begins, Barish and Sanders wear two hats—as builders *and* scientists; soon their builder roles will wither away, as the ramp-down period ends and LIGO settles into its long-term life with a budget of about $20 million annually for operations and another $2.5 million for advanced R&D. Last fall the LIGO Project officially became the LIGO Laboratory, a scientific undertaking, with Barish as director and Sanders as deputy director, and with a Caltech staff of about 80 people. Most have offices in Bridge Lab, but those responsible for data analysis and simulation of the detectors' performance moved this summer to the sixth floor of Millikan Library. The LIGO Laboratory also includes Weiss's substantial group at MIT and those being established at Hanford and Livingston Parish.

Gary Sanders, above; Barry Barish, right.

In addition, Barish has established the LIGO Scientific Collaboration, which includes, along with members of the LIGO Laboratory, also Thorne's theory group and his old friends in Moscow, Drever's new laboratory and his former colleagues in Glasgow, and groups of scientists from Stanford, the universities of Colorado, Florida, Michigan, Oregon, and Wisconsin-Milwaukee, and Northwestern, Penn State, and Syracuse universities, as well as two groups from Germany, and one from Australia. The Germans and Scots together are building their own GEO interferometer near Hannover, smaller and less sensitive than the U.S. duo, but likely to be turned on sooner, as is a similar Japanese device. The Australians have a design for another large interferometer, on the scale of Hanford and Livingston Parish, but as yet no funds. And near Pisa, France and Italy are building VIRGO, equivalent to one of LIGO's interferometers, which they expect to turn on around the same time—the year 2002. "We have a good working relationship with them," says Barish. "We collaborate on some technical things, and we expect to compare data, as data come in."

And when will the data come in? After the interferometer is finished will begin what Barish calls the learning period. "Everything will be built. Nominally we'll have light bouncing around, but we think it will take us two years (until 2002) to get the kind of sensitivity we've proposed and designed and to actually do science." No one is promising that the first gravitational wave will be seen in 2002. Says Barish, "From the experimental point of view, we'll do *our* part. From nature's point of view, there's always an uncertainty. But that's what happens when you look in a new direction where nobody's ever looked before."

Sanders is an optimist, the consequence of having survived other big projects. "Most of those efforts found new physics; many of them found physics that was different from what they set out to find." But while an optimist as to the outcome, he's also a realist about how difficult it's going to be to make it work. "It's going to be harder than we thought to actually make it work—that will take the next two to three years."

"From the experimental point of view, we'll do our part. From nature's point of view, there's always an uncertainty."

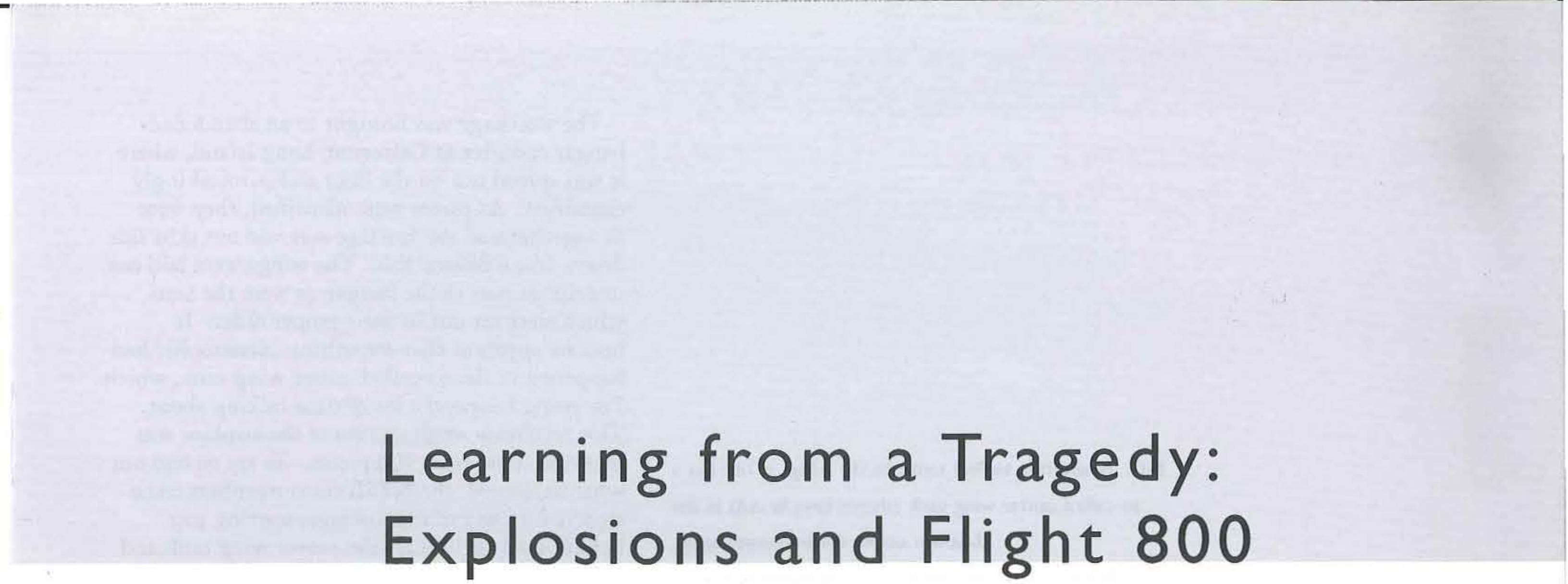
"This is the first of a kind, a tremendously ambitious device, so any troubles will start after we build it. I don't think we will have any major problems building it, but making it work is going to be the challenge," adds Barish.

Thorne, who back in 1976 thought the search for gravity waves might take 10 years, has been forced by LIGO's travails and many delays to lengthen his time horizon somewhat. He's not sure that LIGO's first searches will sight gravity waves—and he so told all reviewers of LIGO proposals from 1984 onward, a confession that helped ignite astronomers' opposition. However, he says, "I'm very optimistic that we'll be seeing waves by the middle of the coming decade, when enhancements of the first interferometers have increased their sensitivity 10-fold." For LIGO, as Sanders points out, was not built as an "experiment," but as a "capability" to do experiments, a platform for successive generations of detectors, which will continue to scan the universe to unravel its mysteries—just as electronic advances have enabled the 200-inch Hale Telescope on Palomar Mountain to keep its big eye on the sky for 50 years. Drever, as well as other members of the LIGO Scientific Collaboration, is already at work on the next generation of advanced detectors. Drever's lab's role, he says, is to "develop new ideas that are going to work—more sensitive instruments that can fit into the LIGO facilities to get much higher performance than we currently know how to do."

In hindsight, should a small campus like Caltech, where most science is done in small groups, have attempted to manage such a large project? Some critics along the way have insisted that it properly belonged in a national laboratory like Los Alamos or in an organization like the Jet Propulsion Laboratory. Thorne, who began it all, disagrees. "LIGO represents the transition of a field from small science to big, and as such is an important case study. It was a transition done largely internally at Caltech—and, in the end, done very successfully. We even learned how to keep small-science groups like mine and Drever's healthy, alongside a huge project. Caltech could have just spun LIGO off like JPL was spun off, or have turned it over to JPL. But then our campus would not have nearly the degree of exciting gravity science and measurement technology that we will have with LIGO firmly ensconced here. Biologists face similar issues, as technology and science opportunities drive some of their science bigger and bigger. It would be unfortunate for Caltech not to learn how to do these things in ways that keep the intellectual ferment and payoffs right here on campus." ☐

If there are trees, this must be Louisiana. Otherwise the two LIGO sites are essentially identical. Two interferometers are necessary to single out a gravity-wave signal from the noise.

Ultimately, about 95 percent of the airplane and its contents were retrieved—more than 20,000 items, some as small as a quarter.



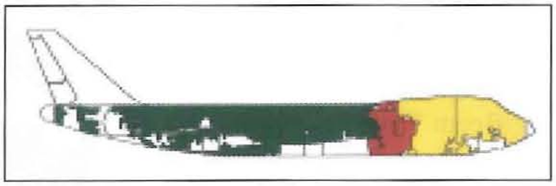
Learning from a Tragedy: Explosions and Flight 800

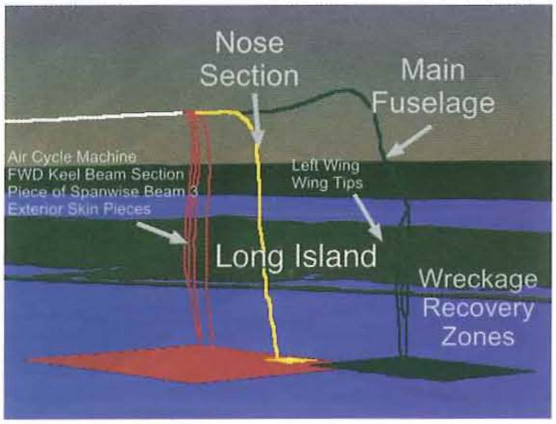
by Joe Shepherd

The wreckage of flight 800 was cataloged and spread out on the floor of an empty hangar in Calverton, about 30 miles from the crash site. In an attempt to reconstruct what happened, 94 feet of the fuselage was reassembled into a structure dubbed "jetosaurus rex."

On the evening of July 17, 1996, a Boeing 747-131 operated by Trans World Airlines as flight 800 from New York to Paris crashed just off the coast of Long Island. All 230 persons aboard perished. Thousands more have been affected in some way—including the people in my lab here at Caltech, which has been involved in the crash investigation since November 1996. The mystery of flight 800 has not been solved. The investigation continues—we're still carrying out experiments, and the National Transportation Safety Board (NTSB) probably will not close the case for some time. The NTSB is an independent federal agency whose mission is to investigate accidents in all transportation modes—airplanes, pipelines, railways, highways, ships, and so on. They probe the circumstances surrounding an accident, try to find its probable cause, and, most importantly, make recommendations to prevent a recurrence—recommendations that have greatly affected aviation over the years. (The Safety Board has no regulatory authority.) The agency is a small one—only about 400 people total—so it has to call on outside help in its investigations. Thus a typical investigation, which is headed up by a senior Safety Board investigator, includes many parties. For example, in an aviation accident, the Federal Aviation Administration (FAA) is a party by law, and the other parties include the airframe and power plant manufacturers, the unions, and the operators, all of whom have expertise in various fields relating to the accident. In this case, the investigators were divided up into 19 teams—the biggest air-crash investigation in U.S. history.

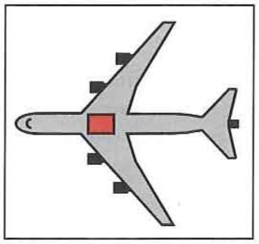
Flight 800 started off routinely, but when it was about 14 minutes out of JFK Airport and at about 13,800 feet, the airplane exploded, scattering debris over some 150 square miles of ocean. It took about nine months for the NTSB, the FBI, the Navy, the Coast Guard, and other agencies to recover and catalog the wreckage. Divers spent 1,773 hours on the bottom, 120 feet deep, and 13,000 trawl lines scoured 40 square miles of ocean floor. Ultimately, about 95 percent of the airplane and its contents were retrieved—more than 20,000 items, some as small as a quarter. The wreckage was found in three zones, shown in red, yellow, and green on the map below. The parts in and around the center of the aircraft were found in the red zone. The portion of the fuselage ahead of the wings was found in the yellow zone, and the remainder of the plane was in the green zone, which lies somewhat to the east. (Remember, the aircraft was traveling from west to east.)

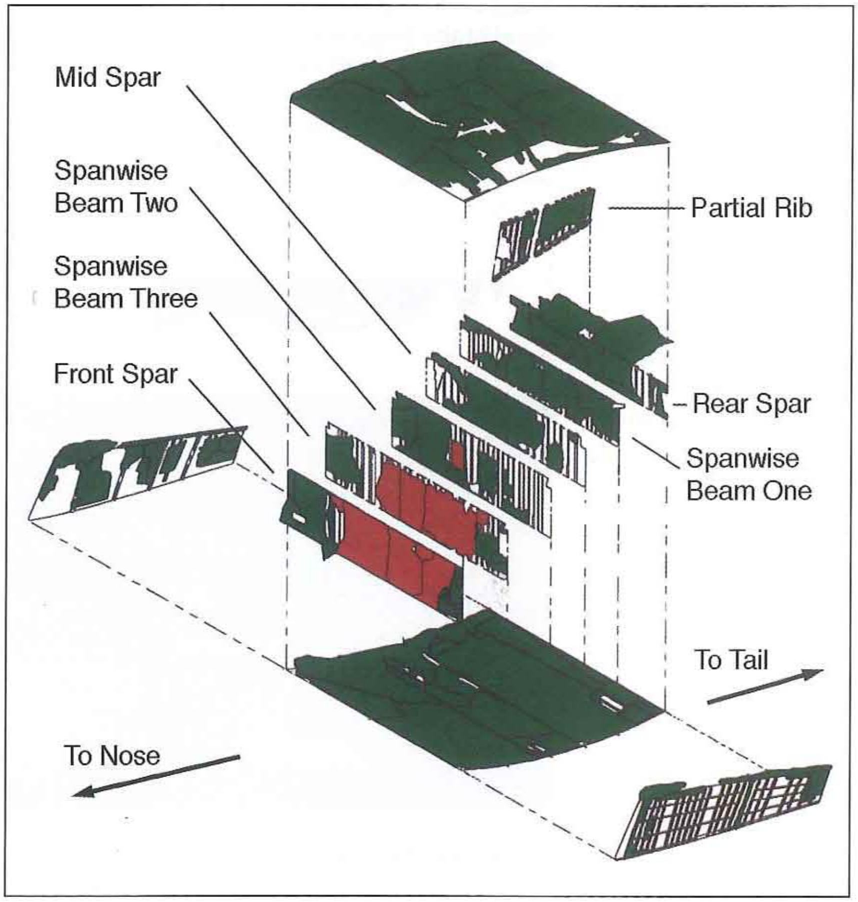




This view of the crash site looks northwest toward Long Island. (New York City is out of view to the left, and the airplane was traveling in the direction shown.) The red, yellow, and green regions show where wreckage from various parts of the aircraft were found.

Left: In addition to fuel tanks in the wings, a 747 has a so-called center wing tank (shown here in red) in the fuselage under the passenger cabin.
Below: The center wing tank is spanned by several structural members that divide it into seven bays, numbered zero through six from fore to aft. Bay zero, between the front spar and spanwise beam three, is a dry bay. Later 747s carry fuel in bay zero, but in the 100 series it's open to the air, so spanwise beam three is effectively the fuel tank's front wall. Again, the colors refer to where the wreckage was found.





The wreckage was brought to an abandoned hangar complex at Calverton, Long Island, where it was spread out on the floor and painstakingly examined. As pieces were identified, they were fit together and the fuselage was laid out skin side down, like a filleted fish. The wings were laid out in another part of the hangar, as were the seats, which were set out in their proper order. It became apparent that something catastrophic had happened in the so-called center wing tank, which I'm going to spend a lot of time talking about. This relatively small section of the airplane was found in more than 700 pieces. To try to find out what happened, the NTSB team members reconstructed 94 feet of the fuselage, starting just behind (and including) the center wing tank and running forward—some 1,600 pieces of wreckage, all told. They built a steel skeleton, dubbed "jetosaurus rex," to which they wired fuselage pieces and interior components so that they could climb around inside and look at the relative locations of deformed metal, cracks, and so-called "witness marks" made where pieces of the aircraft hit each other as it came apart. (The reconstruction, not counting the skeleton, weighed about 60,000 pounds.) After intensive examination, including exhaustive computer simulations—finite-element structural analyses by Boeing engineers—they concluded that the only way to explain all the observations was if there had been an explosion in the center tank. The Safety Board reconstructed a detailed sequence of how the aircraft broke up, and it believes that the explosion was one of the first events in the accident.

Almost all of the tank's pieces were found in the green zone, except for a few very significant components—the front spar, spanwise beam three, the manufacturing panel from spanwise beam two (don't worry about the names; I'll explain them momentarily), and the machinery under the tank—which were found in the red zone. The center wing tank is actually in the fuselage, under the passenger cabin, and runs from wing to wing—if you're sitting in the plane looking out over the wing, you're sitting on top of the tank. It's about 20 feet long, 20 feet wide, $6^{1}/_{2}$ feet high in front, 4 feet high in back, and contains a series of floor-to-ceiling partitions that run from one side to the other. These partitions contain access holes that Boeing's workers use while they're assembling the aircraft. Before the plane leaves the plant, these holes are covered by the so-called manufacturing panels and sealed shut, never to be opened again. In addition, each partition has at least one access hole with a removable cover, called a maintenance panel, that allows workers to clamber from bay to bay within the tank later on. And, finally, the bottom and top corners of the partitions are notched, allowing fuel to flow between bays.

Now, the folks who build airplanes are divided into structures people and propulsion people, and

It became apparent that something catastrophic had happened in the so-called center wing tank, which I'm going to spend a lot of time talking about. This relatively small section of the airplane was found in more than 700 pieces.

Left: A peek inside the center wing tank, specifically bay one, showing two fuel probes (white arrow), a vent tube (black arrow), a fuel fill tube (green arrow), and a wiring bundle and terminal block (red arrows).
Below: The front portion of air conditioning unit number three, which lives under the center wing tank.

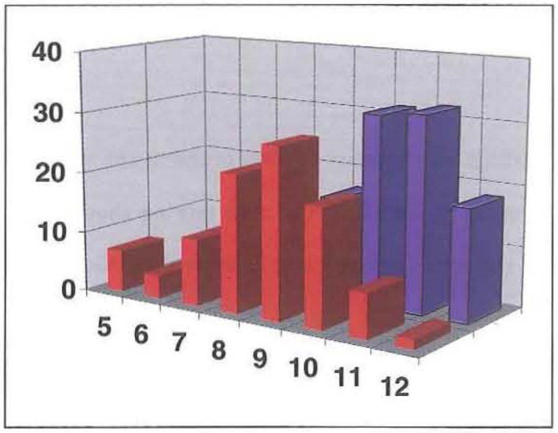
the two look at their airplane quite differently. The structural guys see it as an exquisite monocoque construction that has to have a few engines hung off it in order to fly. The propulsion folks think of it as four beautiful engines with a bit of wing for lift. The structural guys built this tank, and the partitions are actually structural members that run to very nearly each wing tip and carry the wing's bending moment through the fuselage. In order, the partitions are the front spar (which is also the front wall of the tank), spanwise beam three, spanwise beam two, the mid spar, spanwise beam one, and the rear spar, which doubles as the tank's rear wall. The NTSB believes that the explosion blew spanwise beam three into the front spar, causing both to fail. The center section of the airplane disintegrated, breaking the plane in two just ahead of the front spar. The nose plunged into the ocean, while the rear half of the fuselage, which remained attached to the wings and engines, continued on for some distance. This is why, if you follow the flight path, you come to the wreckage of the center section first, then the nose, and finally the rear section.

This is where Caltech came in. Since fuel-tank explosions are, thankfully, an extremely rare occurrence, this conclusion caused puzzlement and concern in the aircraft industry. So Merritt Birky, the Safety Board's senior investigator in charge of the fire and explosion team, asked Caltech's explosion-dynamics lab to assist him in investigating the explosion. We study such things as fuel properties, flames, and the detonation process (an explosion is really just a very fast-moving flame), and a lot of our work is connected with hazard evaluations for nuclear power plants, nuclear storage facilities, rocket sites, and so on. Our laboratory is part of GALCIT, (the Graduate Aeronautical Laboratory at the California Institute of Technology), which was founded in 1926 under Theodore von Kármán and has had a long-standing connection with aircraft design and aviation safety.

Now, in order to have a flame, you've got to have three things. One, you need fuel—in this case, the little bit of aviation-grade kerosene, called Jet A, that was left over when the flight arrived at JFK from Athens. The 747 is a marvelous airplane that can fly all the way from New York to Paris with just the fuel in its wings. Airlines don't like to carry around extra fuel, which is weight that could be used for more passengers, so they didn't refill the center tank when they refueled at JFK. Two, you've got to have air. Well, the tank was full of air, except for about 50 gallons of kerosene lying on the floor of this 13,000-gallon tank—a layer maybe three-sixteenths of an inch deep. And three, you need some source of ignition.

But to get an explosion, you need fuel vapor. If you set liquid fuel on fire, you'll just get a puddle of burning fuel. This is not something you *want* in an aircraft, but it's not going to cause an explosion. So how do we get vaporized fuel? Well, July 17 was a hot day, and there's a set of air-conditioning units that sit underneath the tank. As the air conditioners run, the heat from the machinery could have seeped upward and heated the fuel, causing some of it to evaporate. So now we have fuel vapor and air, and if we have ignition, we can possibly have an explosion.

So we had to answer three questions. Would an explosion have taken place on that particular day? Well, that depends on the exact mixture of liquid fuel, vapor, and air in the tank. Assuming there



The composition of Jet A's vapor (red) versus its liquid (blue) at 50° C in a half-full tank. The horizontal scale indicates the number of carbon atoms per molecule, and the vertical scale is the percentage of molecules with that number of carbon atoms.

was an explosion, could it have ruptured the tank? Well, that depends on how strong the pressure wave was. And the toughest question, which the chairman of the Safety Board always likes to remind me of whenever I see him, is: Where was the ignition source? Sometimes just a spark can start an explosion, and whatever started this one left no visible trace in the wreckage. To answer that, you have to know how the explosion actually propagated from bay to bay through the tank. What that meant for the combustion was not clear when we started, so we've learned some things about how flames propagate inside multicompartment tanks.

We started with the flammability question—was there the right proportion of vapor and oxygen in order to burn? If we start out with air and slowly begin adding fuel vapor, it won't burn—the fuel molecules are too widely dispersed to propagate the reaction. But as we add more vapor, we reach the lower limit of flammability (or, in this case, of explosion). And soon, as we keep adding more vapor, there won't be enough oxygen to go around—we've hit the upper limit of flammability, and again, it won't burn. So there's a narrow region within which the mixture is explosive; outside of that, it's safe. The lower limit, which is what we're interested in, is about 0.7 percent of Jet A vapor in air. This number has been known for years—it's fundamental to jet-engine design.

Now Jet A is a very complicated mixture of a whole bunch of different kinds of molecules. It's not a simple thing like natural gas—I wish it were. And how much vapor you have depends on how willing the molecules are to evaporate, which in turn depends on their exact chemical structures, the liquid's temperature, and how much liquid there is. (It turns out that a very thin layer behaves differently than Jet A does in bulk, as you'll see.) So the Safety Board hired Jim Woodrow of the University of Nevada at Reno to analyze the chemical makeup of Jet A. Notice that what's in the liquid (blue) is very different from what's in the vapor (red), because the big, heavy molecules—the ones with 10 or more carbon atoms—are a lot more sluggish at a given temperature and don't escape into the vapor so readily.

So we need to know the temperature, which is not an easy measurement in this complicated tank. The beams and spars radiate and conduct heat, but the most important thing is the heat source—those three air conditioning units and their associated duct work. These aren't simply overgrown versions of the air conditioner in your bedroom window—they're heat exchangers that actually run off hot air from the engines (or from a small gas turbine in the rear of the airplane that generates electricity when the main engines aren't running). These "Environmental Control Units" (that's Boeing-speak) take in air at over 230° C and 60 pounds per square inch (psi) and convert it to −1° C and 15 psi to pressurize the cabin. That's what you breathe, and it also keeps you comfortable and civil to your neighbor while you're sitting at the gate for several hours, which is what happened in this case. Each air conditioner puts out a different amount of heat, and a lot more heat comes from the ducts, creating hot spots on the tank floor where the liquid fuel can evaporate.

So last summer, we worked with the Safety Board, Boeing, and the FAA on a series of tests in which we flew a 747-100 that had thermocouples mounted throughout the tank. (Our role was primarily to point and say, "Hey—why don't you put a thermocouple over here?") The graph on the opposite page shows the temperatures recorded while the plane sat, air conditioners running, for the length of time that TWA 800's did. Dan Bower of the Safety Board and then-postdoc Raza Akbar [BS '89] analyzed the data and found that the air-conditioner compartment got hot enough to boil water—over 100° C—and the air in the tank's interior got as hot as 60° C in places. The temperature usually falls fairly quickly after takeoff—once the plane begins climbing, the outside air pressure drops and air bleeds out of the tank through vents, while the remaining air expands and cools. The outside air temperature drops as well, cooling off the air conditioners, which are just under the airplane's skin. But for a while at the beginning of the flight, the temperatures can run pretty high. The coolest temperature we saw at the time when the explosion occurred was about 40° C.

Knowing the temperature, it's pretty straightforward to find the number of fuel molecules one can have in a given volume of air. This is called the vapor pressure, and it rises with the temperature. A simple way to think about it is, how much water do you have in the air if you have 100 percent humidity? We all know that it can be much more humid on a hot day than a cold one. Vapor pressure is measured in millibars—

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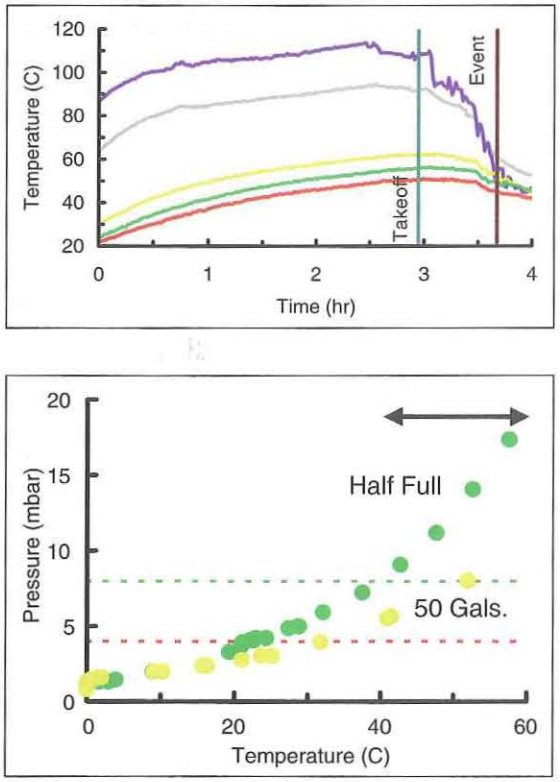
a bar is the pressure of the atmosphere at sea level at 0° C, and a millibar is one thousandth of that. On a hot, muggy day with a thunderstorm approaching, the vapor pressure might be 40 millibars. So postdoc Julian Lee measured the vapor pressure of Jet A and found that it's five millibars or higher at the temperatures we encountered in the flight test. Remember that number. You'll note that although we know that flight 800's tank only had about 50 gallons of fuel, we also did a set of studies simulating a half-full tank. This is because all the previous work in this area had been done using a half- or quarter-full tank, so in order to show that the 50-gallon case behaves quite differently—as you will see shortly—we needed to include the fuller tank as a reference point.

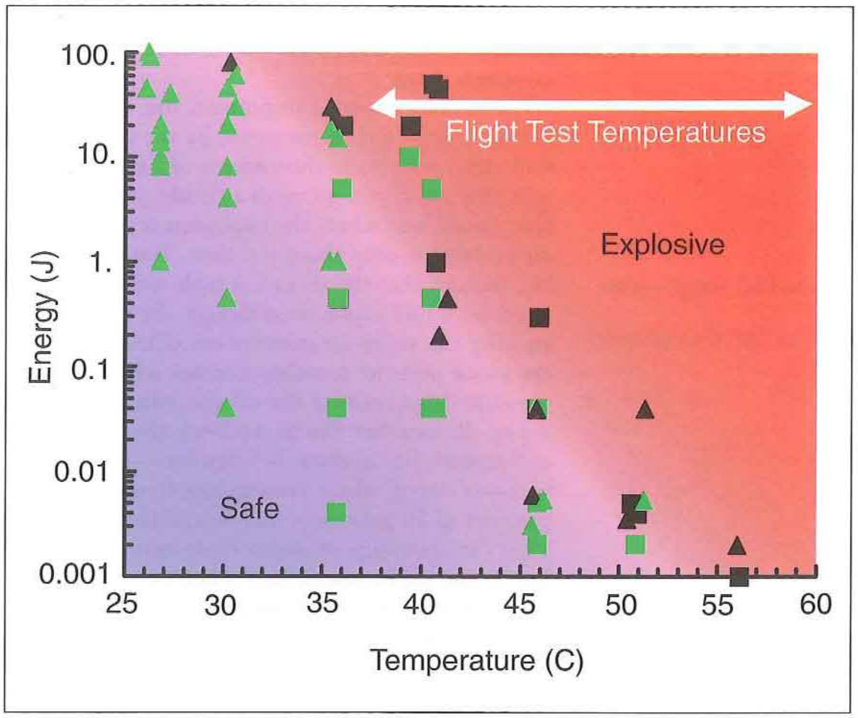
The amount of air is important, too, because the flammability limit is measured by the amount of fuel vapor relative to the amount of air, and the air gets thinner as you go up in altitude. At 13,800 feet, which was where the explosion occurred, the air pressure is only about 0.6 bars. But the fuel is hot enough that the air in the tank remains saturated with fuel vapor, even though the vents are sucking the vapor-air mixture out of the tank. So the vapor pressure remains constant while the air pressure drops, raising the relative percentage of vapor. Remember that at sea level, the lower limit of flammability is about 0.7 percent—7 millibars—of vapor, which corresponds to the vapor pressure of 50 gallons of fuel heated to 50° C. That's in the range of the air temperatures we saw in the flight test, which is bad enough. But it gets worse: at 13,800 feet the lower flammable limit is just under four millibars. (This has been known since studies by the FAA and the Air Force in the late '60s, and Julian's experiments confirmed it.) Julian found that 50 gallons of fuel will give us five millibars at temperatures as low as 30° C; and John Sagebiel of the Desert Research Institute took air samples from the tank during the flight test and verified that more than five millibars of fuel vapor were present. So we're well over the lower flammable limit.

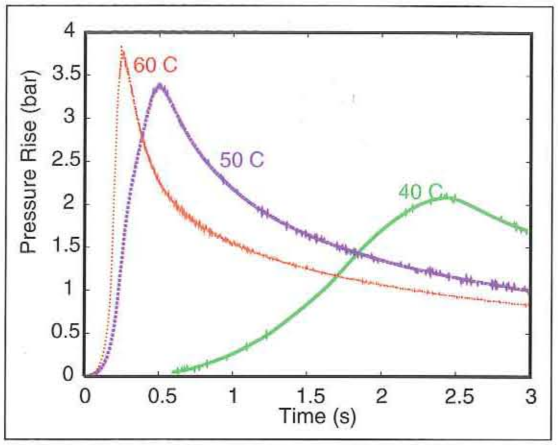
But would it really explode? Julian put some Jet A in a coffee-can-sized pressure vessel, heated it up, and zapped a little spark between a pair of electrodes. (A spark is convenient because you know how much energy you're putting into it, but we've also done this experiment with things such as hot filaments.) The vaporized portion of the fuel was completely consumed in less than half a second. That's certainly fast enough to qualify as explosive, so we've answered the first of our three questions.

He did this over and over again with different amounts of fuel at different temperatures, and discovered that as the temperature increases from about 30 to 60° C, the minimum ignition energy drops enormously—nearly 100,000-fold. That's a very significant finding, and it's another reason why we feel it's very important to keep the temperature down inside these tanks. It turns out that the heat capacity of the fuel itself is what keeps the tank's temperature within safe limits. Even a tank that's only one-eighth full—1,625 gallons, or 10,563 pounds of fuel—will soak up a lot of heat before it warms to a temperature where you'll get significant evaporation. But a nearly empty tank has nowhere to store all that heat except in the air, which has a much lower heat capacity, so everything gets much hotter much faster. That factor of 100,000 is actually the ignition-energy difference between the partly full tank and the tank with 50 gallons in it—because



Top: When you sit at the gate for hours, the passengers aren't the only things that get hot. The blue line shows the temperature in the compartment beneath the center wing tank that holds the air conditioners. The gray line is the temperature of the tank floor, and the yellow, green, and red lines are air temperatures from various points in bay three. Bottom: Jet A's vapor pressure increases with temperature. The dotted lines are the lower flammable limit at sea level (green) and 13,800 feet (red). The arrow shows the temperature range seen in the flight test.





Above: The amount of energy it takes to ignite a sample of Jet A drops by a factor of nearly 100,000 over a surprisingly narrow temperature range. The green data points represent samples that did not ignite; the black ones exploded. The squares are tests simulating 50 gallons of fuel and the triangles represent a quarter-full tank.

Above, right: The height and sharpness of the pressure peak measures the explosion's punch. As the temperature goes down, the explosion gets progressively weaker until eventually the vapor doesn't even ignite.

the fuller tank stays cooler, there's so little vapor in it that you need one heck of a jolt to ignite it. All this is shown in the plot above. The vertical axis is logarithmic, meaning that each increment on the vertical scale is 10 times larger than the previous one. We measure energy in joules, and a joule is one watt for one second. In other words, if you turned on a 100-watt bulb for one second, that's 100 joules. You can get that type of energy from 110 volts AC—household wiring. And 0.01 joule is what you get from a typical static-electricity shock when you shuffle your feet across the carpet.

There are seven fuel gauges in the tank that run on 24-volt wiring with a system to limit the current to less than one-tenth of the minimum ignition energy. But other systems draw more juice—for example, the fuel pumps run on 110 volts AC, as do the cabin lights. These wires are bundled together elsewhere in the plane, and the possibility exists that some insulation degraded, resulting in arcing between the 110-volt wiring and the fuel-quantity instrumentation system wiring. The Safety Board and the FAA are looking at the wiring issues.

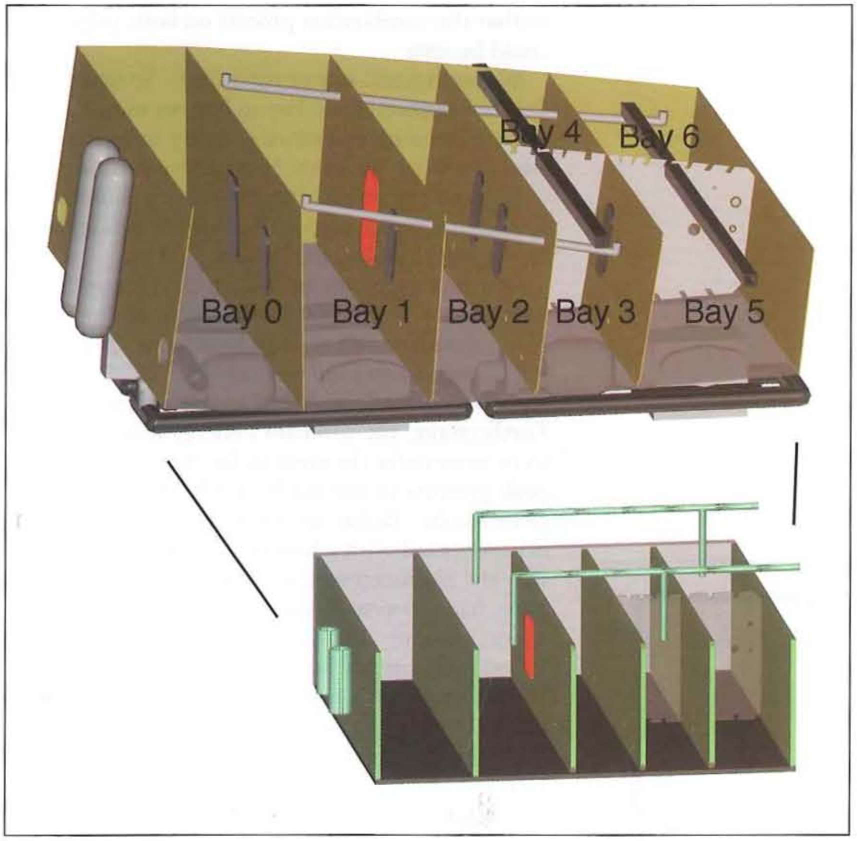
So now we know that the mixture was explosive and that a smallish spark would suffice to ignite it, but could it have damaged the tank? Yes, it could have. Julian found that the explosion's force increased rapidly with the the mixture's increasing temperature, a result confirmed by Chris Krok [PhD '97] in a much larger, 1,180-liter tank. At 60° C, the pressure jumped three and a half bars in a tenth of a second. Even at 40° C we got a peak pressure of almost two bars. It only takes on the order of 20 psi, or one and a half bars, to rupture the front of the tank, so we have enough pressure to cause tank failure. Well gee, you say, that doesn't seem like much—I put more than 20 psi in my tires. But this pressure is being applied over an enormous area. Spanwise beam three is 20 feet wide and 6 feet high, and you're pushing on every square inch of it with 20 pounds. That's more than 500,000 pounds of force—more than the weight of the aircraft itself—all on one structural member, and pushing horizontally on a member that's designed to resist vertical loads. It's just not up to it.

This brings us to the question that's really driving our investigation—where was the ignition source? Knowing *where* the source was would tell us *what* it was.

Backtracking to the source meant we had to deal with the very sticky issue of what the explosion did as it went through the tank, which meant we needed a replica of the tank that was big enough to incorporate the details, such as the vent pipes and the holes in the partitions, that determined how the explosion propagated. The model also had to be sturdy enough to withstand a large explosion over and over again. An explosion in our lab isn't very exciting, because we don't want it to get away from us—it's a little tiny noise in a thick steel vessel. When we give visitors a tour, they look around and say, "Is that it? That little pop? That's *all?*" If you want to make a lot of noise, you've got to go outdoors. So we built a quarter-scale model of the tank to use outdoors—at an abandoned Titan missile base near Denver, as it turned out. Denver is the home of Applied Research Associates (ARA), a firm that specializes in explosive tests and that was our partner on this project. ARA has a lease on the base for just this kind of work, and one of the first things they had to do was weld the doors to the launch-control bunker shut—it had become a popular hangout for local teenagers.

Chris designed the model; Accurate Manufactur-

Left: The tests that included a layer of Jet A in addition to the simulant produced some spectacular fireballs.

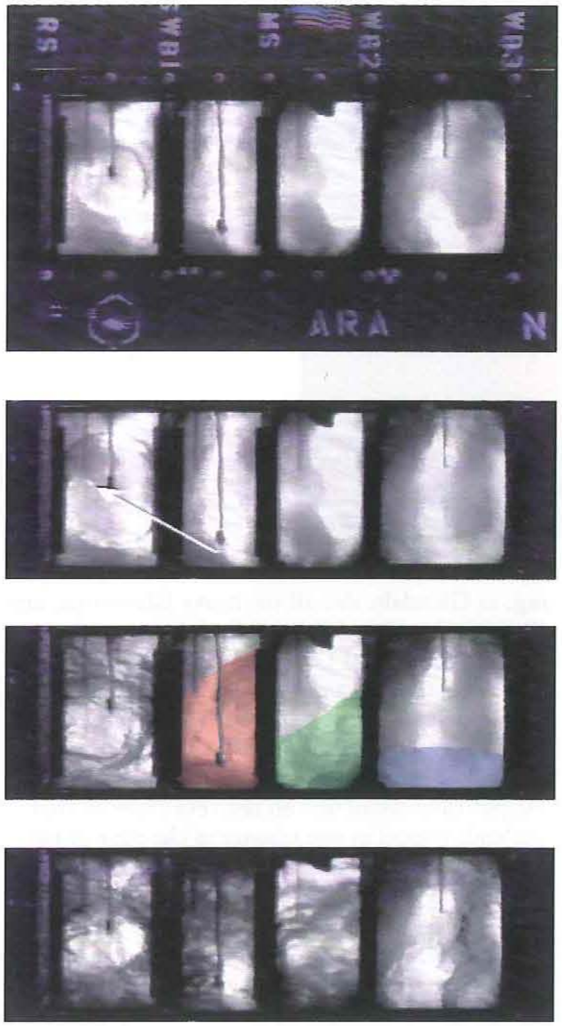


Above: The model (bottom) includes several key features of the real tank (top). Although the corner notches are hard to see in this rendering, other holes, including the manufacturing panel in spanwise beam two (red), are clearly visible. The two fore-and-aft pipes are the vent tubes that connect bays one and six, and bays one and three, to the vent stringers (the dark, transverse, rectangular tubes) that lead out to the wing tips and the outside air. The two cylinders on the front spar are potable-water tanks.

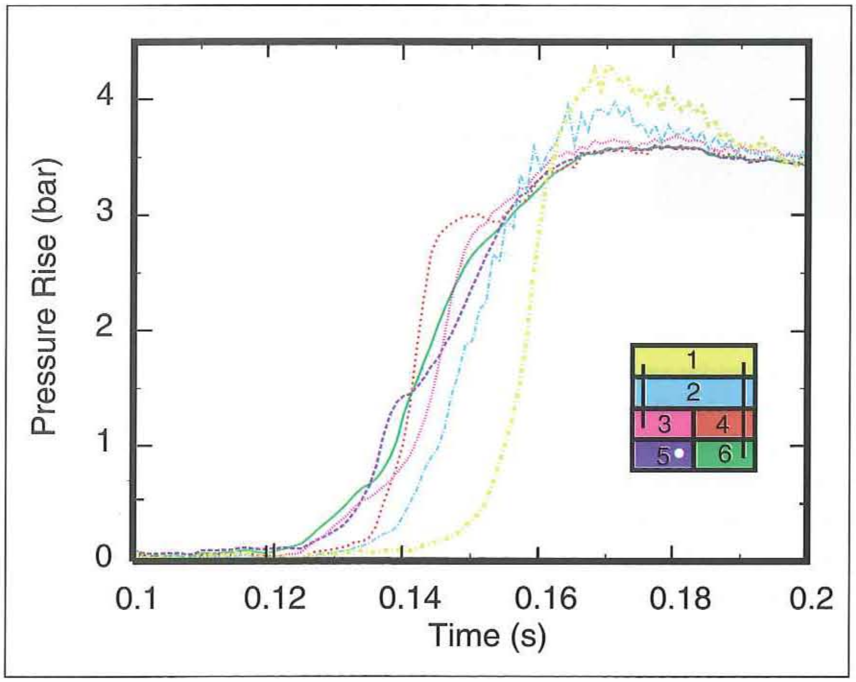
ing, in Glendale, did all the heavy fabrication; and Chris, Julian, Pavel Svitek (the support engineer for our group), and I did the final assembly. Chris's design was a quarter-scale model not only spatially—where the tank was 6 feet high, our model was 18 inches high; 20 feet long became 5 feet long; and so on—but temporally as well. He put flow restrictors on the vent pipes so that the tank vented in one quarter of the time of the full-size tank, and he sized the corner notches and the other holes in the partitions so that flows between the bays were also to scale. Whatever we saw in the model would happen in one-quarter of the time of whatever happened in full-scale.

The model had three-quarter-inch steel top and bottom plates, reinforced with I-beams, and a three-quarter-inch fixed rear spar. The other partitions were removable. The top plate contained our sensors, as well as plumbing connections for the vents and the gas-handling system. (Julian and Chris spent two months building all the instruments.) High-speed pressure transducers recorded the passage of shock waves, while slow-speed pressure transducers recorded the slower pressure changes due to combustion and venting. Thermocouples measured the temperature, and photodiodes detected infrared and visible radiation. Motion detectors in the top and bottom plates indicated when the beams and spars broke free. Finally, electrical feed-throughs allowed igniters to be placed anywhere in the tank to simulate suspected ignition locations, such as the fuel probes or the terminal blocks where various electrical connections are made.

The sides were one-and-a-quarter-inch-thick sheets of a polycarbonate plastic called Lexan—the same stuff that bulletproof windows are made out of—allowing us to follow the combustion with high-speed cameras. (We also videotaped all the tests.) The partial rib, which runs fore-and-aft from the rear spar to the mid spar and which contains numerous holes, was also made of Lexan,



Left: In the tests, each bay had its own high-speed camera, running at roughly 400 frames per second, trained on it, so that these pictures are actually composites overlaid on a photo of the model. Note the labels across the top—"RS" is rear spar, "SWB1" is spanwise beam one, and so on. The silhouettes that look like hanging microphones are the igniter and the backup igniter; the thinner silhouettes are thermocouples. These photos are from test number four, in which the partitions were firmly secured to see how the explosion moved from bay to bay. The circles in the first two frames and the ripples in the other frames are shadows cast by the flame front. The colors in the third frame have been added for emphasis.



Below: Pressure data from the same test. The inset in the lower right corner is a schematic of the model, with each bay color-coded to match the pressure traces. The red dot in bay 5 marks the ignition point. The sudden pressure rises mark where each bay exploded. Bays six and three both feed into bay four, giving it the sharpest pressure rise of all—the "shoulder" on the graph. The pressure in each bay exceeded the failure pressure, but what actually does the damage is the pressure differential on opposite sides of a partition.

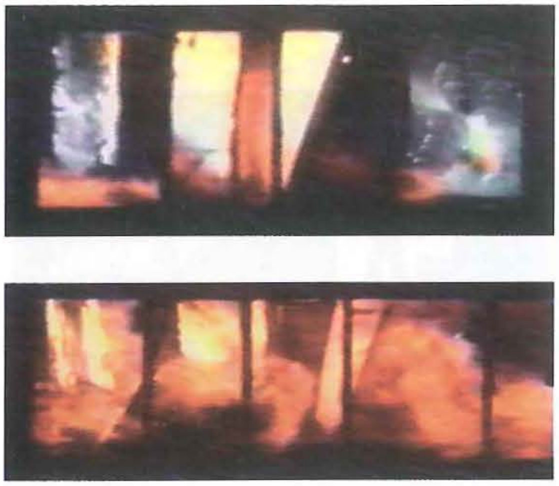
so that the combustion process on both sides of it could be seen.

We had several sets of partitions. To study how the flame moved from bay to bay, we used $^{3}/_{4}$-inch-thick aluminum partitions securely bolted into steel brackets. To study how partition failure influenced the process, we used much thinner aluminum sheets, secured by just enough screws so that they'd break free at about 20 psi.

Denver may be the mile-high city, but it's still well short of flight 800's altitude, so we had to find a simulant fuel vapor whose flame speed at 0.82 bar (Denver's air pressure) and 25° C was equal to Jet A's flame speed at 0.6 bar and 50° C. Furthermore, the simulant's energy content had to be essentially the same as Jet A, so that the peak pressure in our model would be the same as in full-scale. Before we went up there, Chris had experimented with a bunch of fuels and discovered that the hydrocarbons, such as methane or propane, had too slow a flame speed but too much energy content. On the other hand, hydrogen burned too quickly and wasn't energetic enough. He finally hit on a mixture of 7 percent hydrogen and 1.45 percent propane in air that, like Baby Bear's bowl of porridge, was just right. So to start each test, we'd suck 8.45 percent of the air out of the model and refill it to ambient pressure with premixed hydrogen and propane. (It's pretty astonishing that we could still seal our model after all those explosions—we used a third of a tube of silicone caulking compound per test, and lots of double-sided foam-core tape.) We then stirred the tank with a bellows pump. In the actual aircraft, of course, convection from the hot spots in the tank did the mixing in the three hours it sat on the runway.

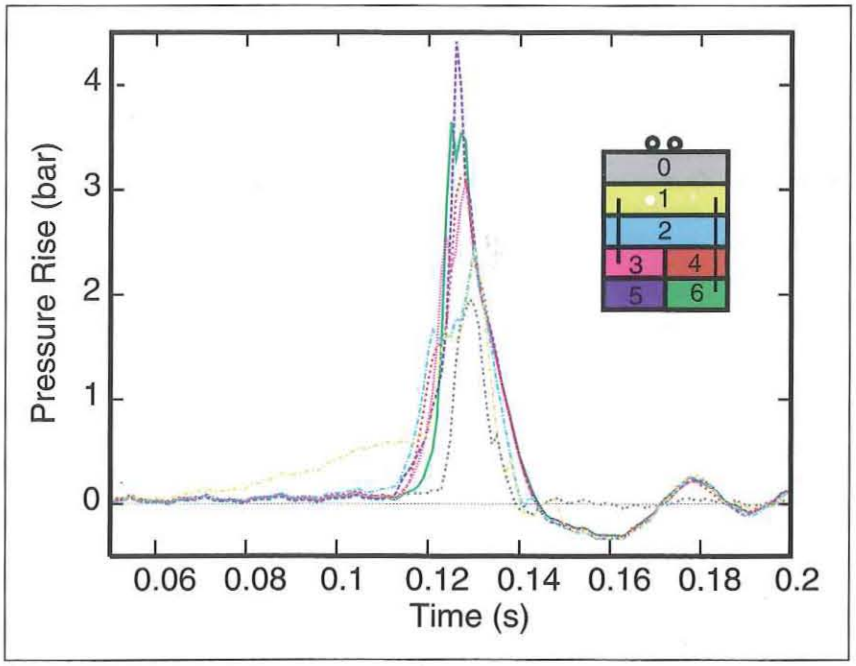
Chris, Julian, Pavel, and the ARA guys did 30 explosions from October through December. I went up there twice, but my main contribution was to sit back here in Pasadena and worry a lot. They'd send me the data every day, and I'd process

The second test was number 21 in the series. The photo below, from the high-speed camera in bay two, shows that spanwise beam two was actually bowed backward from the force of the explosion in bay one (to the right) before being ejected forward by subsequent explosions. You can also see the cloud of liquid Jet A (arrowed) being kicked up by the jet from the corner notch.



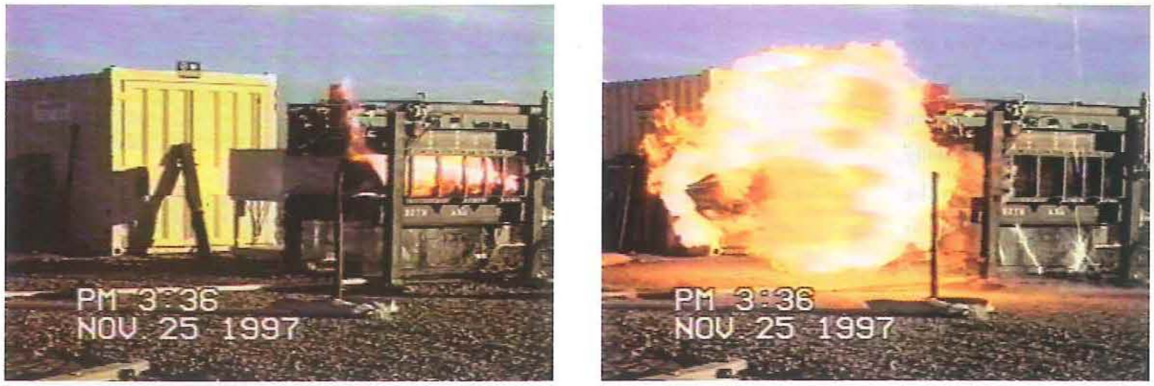
Above: These stills were lifted from the videotape. In the top photo, the bottom side of spanwise beam two has broken loose, and flames are beginning to engulf the rear bays. In the bottom photo, the rear bays have exploded, and all the partitions have come loose. (The front spar and spanwise beams two and three were ejected out the front of the tank. The mid spar and spanwise beam one remained in the tank, although the mid spar was blown forward. Spanwise beam one was shoved toward the rear.)

Below: Since this test was done with weak partitions, pressure data was taken in bay zero (the dry bay) as well. The pressure rises were more closely bunched together, and the pressure dropped sharply when the panels blew out, rather than slowly venting away as in the strong-partition test.



it and post it on the Web so that the other team members at Sandia, at NTSB headquarters, in Norway, and in Canada could get daily updates. It was easier than faxing umpteen people, and the folks doing computer simulations of the explosion could download our data directly. We provided some very nice results for the simulations, which in turn takes us closer toward our goal of finding the ignition source. In order to give you a better feel for the very complex sequences of events we recorded, let's look at some pictures and pressure data from two of the tests.

The first test I'll show you used the strong partitions. The ignition source was in bay five, which is to the left in the pictures on the opposite page. In the first frame, the nice, regular bubble surrounding the igniter is the flame front. You can see it's very even. We also know that it's growing relatively slowly, because you don't see any pressure rise until 0.12 seconds, and even then it's very gentle for the next hundredth of a second. At the same time, the advancing flame front pushed unburned gas ahead of itself through the holes into bays three and six, causing similar pressure rises. Although you can't see it, these jets of unburned gas roiled the air in bays three and six, priming them to explode—turbulent air will carry a flame front very rapidly, producing a very fast explosion. In the second frame, the flame is passing into bay six through a hole in the partial rib (arrow). Bay six was immediately engulfed in flame, as seen in the third frame, and this explosion caused the pressure in the bay to skyrocket, squirting a tongue of fire (red) through the corner notch into bay four. Similar jets of flame are visible in bays two (green) and one (blue). It's a cascade—the jet in bay four drives compression in bay two, which in turn spills over into bay one. The bays also ignite in that order, as mirrored in the pressure data. Because bay two is roughly twice as big as the preceding bays, it takes longer to burn and thus bay one ignites relatively slow-



A set of still photos from the videotape of test 21. In the first frame, you can see the front spar beginning to tear loose. In the second frame, at least one panel can be seen near the front of the fireball. In the third and fourth frames, the remaining Jet A in the bottom of the tank burns off. The fireball, although visually impressive, does very little damage to the tank.

ly—a whopping hundredth of a second later.

The second test used weak partitions, and included a thin layer of Jet A on the floor in addition to our simulant fuel vapor. The ignition source was in bay one. The pressure data shows that bay one began to get pressurized in about six hundredths of a second, but it's so large that it continued to burn for another six hundredths of a second—an eternity on this time scale—before anything else happened. But this sent jets of gas through the notches into bay two, creating turbulence in advance of the flame's arrival. As we saw before, this set up a cascade effect, so that when the flame did arrive at bay two, it moved like lightning and engulfed the remaining bays in a hundredth of a second or so. And, finally, the cycles of negative and positive pressure that began at about 0.14 seconds were due to partition failures—the flying panels created partial vacuums in their wakes, and the combustion products vented toward the front of the tank.

We've been spending a great deal of time analyzing our data over the last several months. We're examining the details of how the pressure differentials vary across components, and when each differential reaches failure pressure. Our Canadian collaborators are comparing the results to the breakup sequence the Safety Board deduced from the wreckage analysis, which indicated that certain parts of the center wing tank stayed intact longer than others. It's what we call an inverse problem—we have the results, and our task is to figure out what we started with. We hope to find a signature that will allow us to draw some conclusions about where the ignition source might have been. We do see that the ignition location influences the pattern, but we don't have any kind of a smoking gun.

There are several complicating factors. For example, the fuel vapor probably wasn't evenly distributed throughout the bays. The liquid fuel certainly wasn't—it was sloshing around in the bottom of the tank, which is covered with a whole bunch of stiffeners. (The tank's floor and ceiling are actually extensions of the lower and upper skins of the wings, and help carry the wing's bending moment through the fuselage.) Since the aircraft was still climbing, the fuselage was tilted up by about five or six degrees. Fuel would puddle up behind each stiffener, spilling over from stiffener to stiffener en route to the notches that drain back to the next bay. There's a lot of uncertainty about the fuel distribution, and that's an important point we're considering.

The real explosion happened with Jet A vapor rather than our simulant, so this summer we're going back to Denver to do quarter-scale tests using Jet A. (We'll have to pump the tank down to simulate the explosion altitude.) Furthermore, our structural-failure scenario is extremely simplified—the center wing tank's upper and lower skins came apart at the same time that the beams and spars moved. And unfortunately, size matters. There are some aspects of explosions that simply don't scale well, so the Sandian and Norwegian groups are modeling our quarter-scale flame to determine how our results relate to the full-scale situation.

So then, what does all our work have to do with the real world? Three months after the crash, the NTSB recommended that the FAA pursue ways to make the center fuel tank less flammable. The accumulated weight of our results, coupled with others' studies and pressure from the Safety Board and the public, has since caused the FAA to take up the recommendation. A committee of industry/FAA committees called the Fuel Tank Harmonization Working Group, which is not a barbershop quartet, looked at such things as a further reduction in ignition sources, cooling the tank, using fuels with lower flash points, and possibly installing inerting systems. (An inerting system introduces an inert gas, such as nitrogen, into the tank to drive out some air and hence oxygen mole-

cules, reducing their number to below the lower limit of flammability.) A draft of the results of that study are now undergoing review. In addition, there have been several airworthiness directives—legally binding orders from the FAA to the airlines—about possible sources of ignition associated with the fuel-quantity instrumentation system wiring inside the center wing tank. The FAA has also mandated a tank-inspection program on both the 737s and 747s, so the next time one of these planes goes in the shop for what they call heavy maintenance (or within two years, whichever comes first), there'll be a whole list of things to look at. (People don't go into these tanks very often, and for good reason—it's a very tough environment. You have to use a breathing apparatus, and crawl through small holes into confined spaces that just give me the heebie-jeebies. And once you open all those access panels, you've disturbed the tank's integrity, so that it all has to be resealed afterward.) And because a cooler tank is safer than a hot one, the NTSB has suggested that additional fuel be put into the tanks during extended gate holds or other long periods on the ground. But the NTSB and the FAA are still debating the specifics, which would depend on how long the aircraft had been sitting, and what it had been doing earlier.

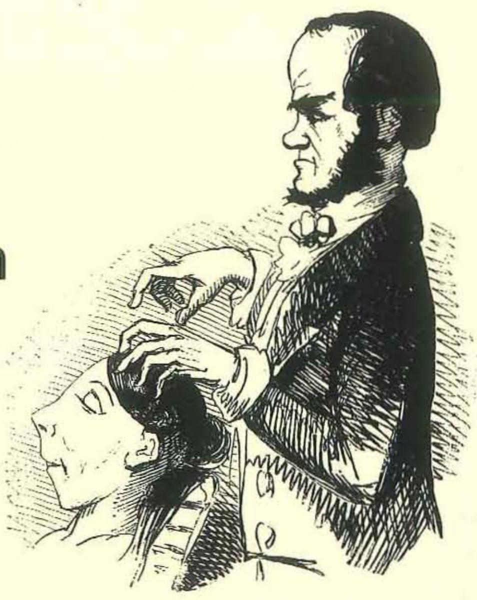
Finally, let me put all this talk of explosions into perspective. Air travel is extraordinarily safe, particularly in the United States. On average, there's an accident resulting in fatalities—from all causes, not just fuel-tank explosions—roughly once in every two million departures. Last year, U.S. airlines made 10 million departures, and there have been something like 317 million departures worldwide since the start of jet travel in 1959. In all that time there have been about a dozen fuel-tank explosions. Some of those involved JP-4, which is very similar in volatility and vapor pressure to gasoline (and thus much more hazardous than Jet A!), and is now rarely used in commercial aviation. There are only three known explosions of center wing tanks, of which TWA 800 is one. One of the other two was connected with a bomb—a 727 flown by Avianca Airlines in 1989. Someone in Colombia was getting rid of an enemy, and unfortunately brought down the entire plane. The remaining one happened in 1991, on a runway in Manila, to a 737 belonging to Philippine Air Lines—the closest parallel we can find to Flight 800. This aircraft had been modified after it left the factory, and it is believed that this modification, or a faulty fuel float switch, caused the explosion. (In 1976, an Iranian Air Force 747 that had been converted into a tanker exploded. That was a wing tank proper, however, and lightning is believed to be involved; furthermore there was mixed loading with JP-4.) However tragic, explosions of center wing tanks are extremely rare. Even so, measures are being taken to drive the probability down even further. ☐

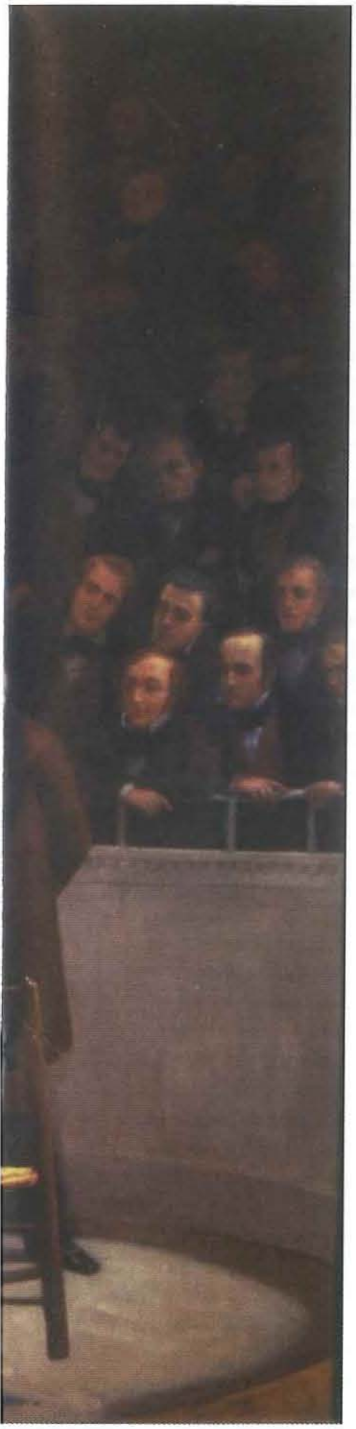
Joe Shepherd has been an associate professor of aeronautics at Caltech since 1993—his second career here; he got his PhD in applied physics from Caltech in 1980. (He earned his BS in physics from the University of South Florida in 1976.) Before returning to Pasadena, he was on the faculty at Rensselaer Polytechnic Institute and a staff member at Sandia National Laboratory. He has been studying explosions for the past 20 years and has worked the whole spectrum of such events, from tiny droplets evaporating in tabletop experiments all the way up to nuclear explosions in the Nevada desert. Over the last five years, he has led the research group that developed the Explosion Dynamics Laboratory and put Caltech in the position to make a unique contribution to this investigation.

This article is adapted from a recent Watson Lecture.

Below: In *The First Operation Under Ether* (painting by Robert Hinckley), which took place in Boston on October 19, 1846, William Morton demonstrated the anesthetic properties of ether during jaw surgery. Courtesy of the Boston Medical Library in the Countway Library of Medicine, Boston, Massachusetts.

Mesmerism and the Introduction of Surgical Anesthesia to Victorian England





by Alison Winter

Above: The most famous Victorian mesmerist, John Elliotson, is portrayed in a 1943 *Punch* cartoon as a concert pianist, "playing" the head of a plebeian woman with mesmeric influences, as if her brain were a set of piano keys.

One of the most celebrated moments in the history of surgery is the introduction of anesthesia in the 19th century. During the 1840s, ether, choroform, and then nitrous oxide were first used in surgical practice as anesthetic agents. Historians have traditionally seen this innovation as the critical moment in an age-old battle between doctors and pain. I'm going to make an argument, however, that might initially seem both ungenerous and contrary to common sense: I'll suggest that the introduction of anesthetic techniques in the 1840s certainly had *something* to do with the alleviation of pain, but a much more important factor was the professional anxieties of Victorian doctors and their struggles for authority at a critical moment in their history. Ether was introduced as an alternative to a preexisting anesthetic technique that was threatening to many doctors—the practice of mesmerism.

In the first half of the 19th century, British physicians and surgeons were ostensibly governed by three organizations—the Royal College of Physicians, the College of Surgeons, and the Society of Apothecaries—but actually there was very little regulation. Early Victorian doctors often claimed that they were making great strides in clinical research and medical education, but they were by no means a united and powerful community: there were few legal regulations over medical practice, and a wide variety of competing healers offered their services in a chaotic medical marketplace. And Victorian patients were less impressed with the progress of medicine than doctors themselves. Medical students were caricatured in the press as drunken buffoons, and it was commonly said that doctors' ignorance and irresponsibility made them as likely to kill as they were to cure their patients.

There were constant complaints about "quackery," but these complaints came from so many different sources as to suggest that the problem was ubiquitous—there was no single type of practitioner one could seek out who was sure to be trustworthy. There were "quacks" with formal medical training and without it, in the metropolis and in the provinces, on the faculty of the universities, and engaged in private practice. Conventional doctors were accused of quackery, as well as people we might now identify as fringe or alternative therapists, such as homeopathists, herbalists, hydrotherapists, mesmerists, and hypnotists. But calling them fringe therapists would be anachronistic, because the situation was so chaotic in early Victorian England

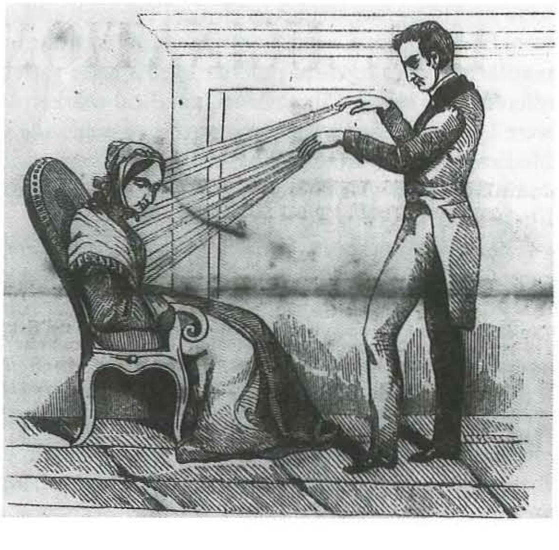
> To prove that the ordinary senses were really gone, the mesmerist and members of the audience fired pistols near the subject's ears, pricked her skin with needles . . . and waved smelling salts beneath her nostrils.

Right: A mesmerist creates a state of unconsciousness in his subject by moving his hands closely over her. Below: Here the magnetic influence is rendered visible (by the imagination of the artist) as a physical force radiating from the mesmerist's body into that of his subject. (Both illustrations from Charles Dupotet, *L'Art du Magnétiseur*, 1862.)

that they were not really marginal at all—they were just part of the fray.

Among them, the mesmerists were particularly significant to the history of anesthesia. Animal magnetism, or mesmerism, was a practice in which one person claimed to influence another through the movement of his hands near the surface of the other person's body. It was invented in the late 18th century by the Austrian physician Franz Anton Mesmer, who thought that he had discovered a means of manipulating physical forces, or "magnetic fluids," in the service of health.

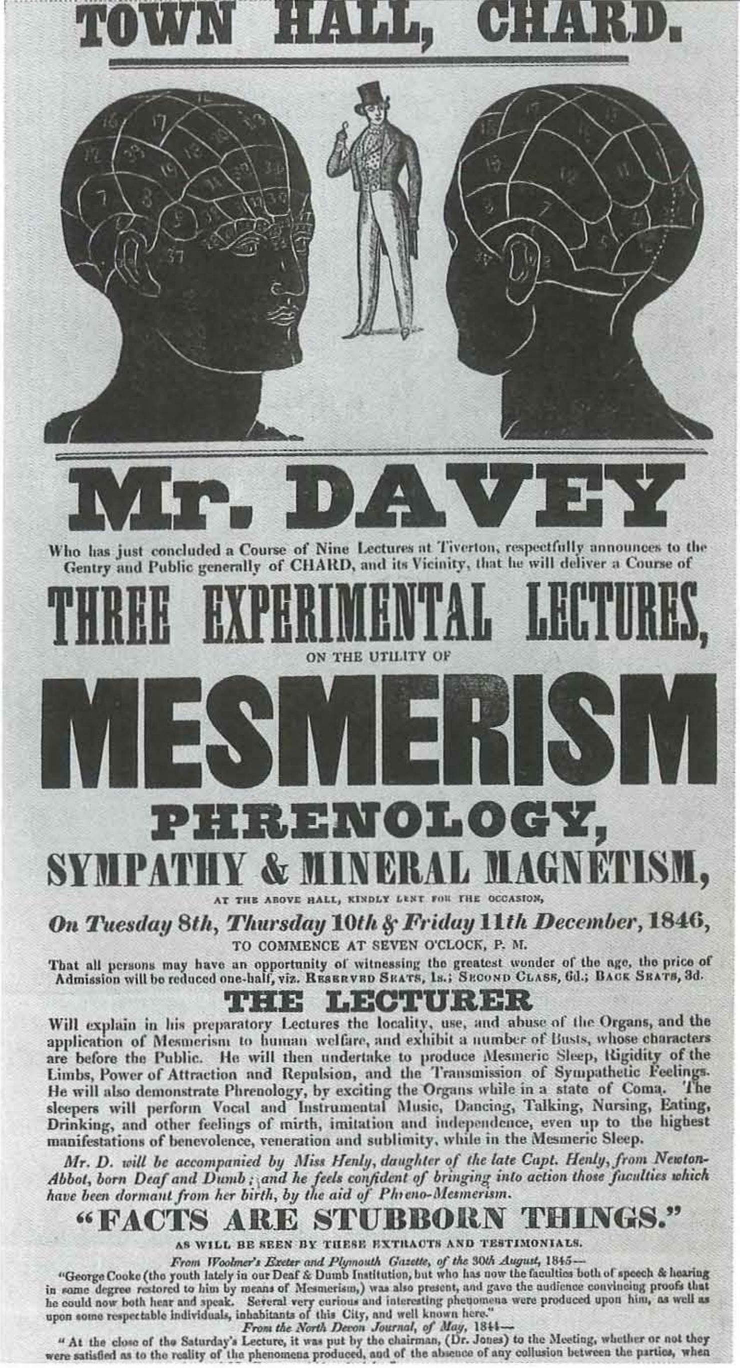
Mesmer and his followers thought that when the mesmerist moved his hand in front of the patient, a physical influence of some kind passed between them. The influence created physiological changes in the patient's body. Mesmerism was controversial from the very beginning, but it survived and spread throughout Europe. In the 19th century, it became a widespread form of psychological experiment and medical therapy. In the illustration above, a mid-19th-century mesmerist moves closely over his subject, his physical proximity creating a state of artificial sleep or altered consciousness; the illustration below portrays the magnetic influence as a kind of ray, akin to light. Once the mesmeric state had been achieved, further manipulations could cure illnesses, or, alternatively, produce amazing psychical phenomena such as clairvoyance, prophecy, and the suspension of sensation.



During the 1840s, hundreds of itinerant mesmerists traveled along Britain's lecturing circuits, displaying their phenomena before paying audiences. They used public shows to attract private clients who might be willing to pay large sums for personal treatment. During the public demonstrations, mesmerists and skeptics would tussle over the question of whether the strange phenomena were real or the subject were faking. This was the first question people asked, and they went to extravagant and sometimes horrifying lengths to answer it.

To prove that the ordinary senses were really gone, the mesmerist and members of the audience fired pistols near the subject's ears, pricked her skin with needles (the stereotypical patient was always a woman, although mesmerism was praticed on both genders), and waved smelling salts beneath her nostrils. There were more aggressive tests as well: acid poured on her skin, knives thrust under her fingernails, electric shocks run through her arms, and noxious substances placed in her mouth (such as vinegar, soap, and even ammonia). Experiments sometimes provoked physical skirmishes over subjects' bodies as the mesmerist and his challengers inflicted rival tortures. If these produced a response, skeptics dismissed the experiment. If there was none, the trance was all the more plausible—or the fakery all the more skillful and reprehensible.

This was how mesmerism's anesthetic powers were discovered. After dozens of public demon-



TOWN HALL, CHARD.

MR. DAVEY

Who has just concluded a Course of Nine Lectures at Tiverton, respectfully announces to the Gentry and Public generally of CHARD, and its Vicinity, that he will deliver a Course of

THREE EXPERIMENTAL LECTURES,

ON THE UTILITY OF

MESMERISM

PHRENOLOGY,

SYMPATHY & MINERAL MAGNETISM,

AT THE ABOVE HALL, KINDLY LENT FOR THE OCCASION,

On Tuesday 8*th*, *Thursday* 10*th* & *Friday* 11*th December*, 1846,

TO COMMENCE AT SEVEN O'CLOCK, P. M.

That all persons may have an opportunity of witnessing the greatest wonder of the age, the price of Admission will be reduced one-half, viz. RESERVED SEATS, 1s.; SECOND CLASS, 6d.; BACK SEATS, 3d.

THE LECTURER

Will explain in his preparatory Lectures the locality, use, and abuse of the Organs, and the application of Mesmerism to human welfare, and exhibit a number of Busts, whose characters are before the Public. He will then undertake to produce Mesmeric Sleep, Rigidity of the Limbs, Power of Attraction and Repulsion, and the Transmission of Sympathetic Feelings. He will also demonstrate Phrenology, by exciting the Organs while in a state of Coma. The sleepers will perform Vocal and Instrumental Music, Dancing, Talking, Nursing, Eating, Drinking, and other feelings of mirth, imitation and independence, even up to the highest manifestations of benevolence, veneration and sublimity, while in the Mesmeric Sleep.

Mr. D. will be accompanied by Miss Henly, daughter of the late Capt. Henly, from Newton-Abbot, born Deaf and Dumb; and he feels confident of bringing into action those faculties which have been dormant from her birth, by the aid of Phreno-Mesmerism.

"FACTS ARE STUBBORN THINGS."

AS WILL BE SEEN BY THESE EXTRACTS AND TESTIMONIALS.

From Woolmer's Exeter and Plymouth Gazette, of the 30th August, 1845—

"George Cooke (the youth lately in our Deaf & Dumb Institution, but who has now the faculties both of speech & hearing in some degree *restored* to him by means of Mesmerism,) was also present, and gave the audience convincing proofs that he could now both hear and speak. Several very curious and interesting phenomena were produced upon him, as well as upon some respectable individuals, inhabitants of this City, and well known here."

From the North Devon Journal, of May, 1844—

"At the close of the Saturday's Lecture, it was put by the chairman, (Dr. Jones) to the Meeting, whether or not they were satisfied as to the reality of the phenomena produced, and of the absence of any collusion between the parties, when

Above: A typical broadsheet advertises a public demonstration of mesmerism by an itinerant lecturer. The mesmerist proclaims that "facts are stubborn things" as a promise that the audience will find mesmerism's reality impossible to deny. (Courtesy of the Somerset Records Office.)

strations in which unconscious mesmeric subjects were unwittingly tortured, people began to think of putting anesthesia to a more constructive purpose, namely, in surgery.

The first well-publicized British operation to use mesmeric anesthesia was an amputation of a leg at the thigh. The patient was James Wombell, a 42-year-old Nottinghamshire laborer, and the mesmerist a barrister named William Topham. For several days Topham used mesmerism to put Wombell "into repeated states of diminished pain and deeper sleep." Finally, he reached a state of complete insensibility, and during the surgery he manifested none of the usual signs of pain except for a "low moaning." This sound was not influenced by the course of the operation; it did not change, and Wombell did not stir, when the major nerve to the spine—the sciatic nerve—was cut. Afterward Wombell claimed to have felt no pain, though he did say he had "once felt as if I heard a kind of crunching." He recovered and lived for 30 years.

It might seem obvious that Wombell would have had no incentive to misrepresent his experiences—if he said he felt nothing, then he felt nothing. But this was violently disputed when his case was published. Doctors argued vehemently about whether he had been faking his lack of pain during the operation and lying about it afterward. Some claimed that he had colluded with the mesmerist and surgeon to pretend that he felt nothing when he had actually been conscious the entire time. That is, he had felt all the pain of the amputation but had used what muscles remained in that leg to hold it still even when the knife cut through the sciatic nerve.

To understand this skepticism, we need to get a better sense of what it meant in this period to lose sensation. Since the late 18th century, a wide range of drugs, gases, and vapors could suspend sensation, but until the 1840s, it seems that no one thought to use these agents for the relief of pain in surgery. One might think that this inaction stemmed from the fear of side effects. But 19th-century doctors were not very scrupulous about this kind of concern. The first half of the century saw the introduction of powerful chemicals into therapeutic treatment, many of which are ranked as toxic when used in lower doses than those used at the time for therapeutic purposes. Doctors had no worries about using untried chemical cures on charity patients in hospitals. If they had wished to experiment with anesthesia in the teaching hospitals, there would have been nothing to stop them.

Nor can one attribute the delay to the ignorance of the general public about these chemicals and their effects on the body. Ether and nitrous oxide,

Left: The first major surgical operation on record using mesmerism was performed in 1828 in Cherbourg, France—a breast amputation on one Mme. Plantin by Dr. Jules Clocquet. When Plantin appeared to feel none of the pain of surgery, the experiment was declared a success, although the patient died soon after. (From Figuier, *Mystères de la Science*, 1880.)

and their perception- and consciousness-altering effects (including insensibility to pain) were on show in music halls and popular scientific displays. Their effects were witnessed by virtually all ranks of society, and they could be obtained commercially. A great proportion of individuals had seen the effects of chemicals like ether, alcohol, laudanum, opium, and nitrous oxide, and had access to them.

It is extraordinary, on the face of it, that 50 years should have passed before ether and nitrous oxide were routinely used in surgery. One rather obvious point is that sensation and insensibility had a very different significance in the 19th century from what they have come to mean since anesthesia became a routine part of medical practice. The connection to surgery, once made and demonstrated, was obvious, but making this connection was not trivial. If it had been, surgical anesthesia would have been developed in the late 18th century, when natural philosophers were most interested in developing different kinds of gases and vapors and documenting their effects on the body. Instead, the deliberate suspension of pain during surgery came as an afterthought in early Victorian mesmeric research. Even after mesmeric anesthesia was developed, four years passed before chemical anesthetic agents became well known (although there were sporadic, individual experiments with these agents earlier).

What may be even more surprising is that many doctors did not like the thought of anesthesia when mesmerism's powers were first demonstrated. Some were actually horrified by the prospect. One medical editor protested that the idea of one person producing insensibility in another was too terrible even to admit into consideration. If pain could really be suspended, he threatened, "the teeth could be pulled from one's head" without one's even realizing it. He concluded that the suspension of pain would "tear down" all the "fences" in society. It was not merely the state of insensibility that was horrifying, of course, since alcohol and opium could dull pain and remove consciousness. But these were not dispensed by someone else; they were consumed by the individual concerned, and he or she controlled the dosage. For the medical editor, the thought that one person could remove from others their sensitivity to their surroundings involved a horrifying violation of the individual's agency. The disturbing nature of the hypothetical scenario he laid before his readers further accentuates the difference in bodily sensibility between the 1830s and the late 20th century.

There is no way of knowing whether the patients of such outraged doctors would have reacted similarly if the connection between the production of insensibility and its potential use in surgery had been presented to them. I am bound to suspect that they would have been less fussy than the medical editor quoted above. But either way, patients, like doctors, did not make the connections that would have given them the choice.

One factor in the changing attitude to pain was the rising power of surgeons, and their rivalry with physicians. During the early 1840s the College of Surgeons lobbied for, and in 1843 received, the Royal accreditation that had long been the sole privilege of the Royal College of Physicians. In the late 1830s and early 1840s, the surgeons' drive to lever themselves into positions of greater authority provoked resentful articles about individual surgeons, representing them as unregenerate, inhumane, and barbarous hypocrites who talked reform but practiced barbarism. And in these portraits, pain was portrayed as something the surgeon maliciously manipulated. For instance, in 1840, the *Medical Times* ran a striking series of "portraits" of the master of the London surgical scene, Robert Liston, professor of surgery at University College London. Liston was preeminent for his speed with the knife and skill at manipulating it. But even Liston could be represented as a malicious, maladroit rogue within the reformist medical press. The *Medical Times* portrayed him as an example of old-fashioned surgeons' unfeeling attitudes to their patients. The way it sketched Liston's crude, cruel, and vulgar personality involved Liston's attitude to his patients' pain.

The article told a dramatic story of a struggle between him and a patient on the operating table. During a lithotomy the patient "attempted to close his limbs in a vain attempt to avoid stretching the gaping wound" and thereby suffer even greater pain. His surgeon shouted, "'Slack your legs, man; slack your legs—or I won't go on.'" Then he "coolly relinquished the operation," and stated coldly, "'No, I won't go on, . . . unless he loosens his limbs.'" Eventually the patient was able to relax his legs. Liston then proceeded with the operation and, telling the patient, "here's your enemy," removed the stone from his bladder. The article concluded with a scornful summing up: "His element was blood, and he raised himself towards the pinnacle of professional renown upon the mangled trophies of his amputations and the reeking spoils of the operating theater." One could only pity the "trembling patients" who waited to "feel the temper of his knife."

One medical editor protested that the idea of one person producing insensibility in another was too terrible even to admit into consideration. If pain could really be suspended, he threatened, "the teeth could be pulled from one's head" without one's even realizing it.

Pain was traditionally a sign of surgeons' masterful status, like the clotted blood they left on their aprons. In the eyes of this journalist, however, the surgeon was a sinister figure perversely vaulting himself to greater power by making a greater spectacle than necessary of the patient's pain and his dependence on the surgeon. In this instance, *mental* control of another person's body was a greater sign of surgical power than *physical* control. At the same time, though, it facilitated an indictment of the surgeon. Pain was treated here not as an inevitable part of a patient's experience, but as an evil that should be minimized wherever possible. This was the implicit assumption that made Liston's manipulation of his patients' pain pivotal in the *Medical Times*'s attempt to discredit him.

Another factor in the controversy over mesmeric anesthesia was related to Victorians' fascination with altered states of mind. Victorians used a dizzyingly large vocabulary for suspended animation: sleep, coma, insensibility, catalepsy, suspended animation, transient death, human hibernation, and anesthesia were only a few of the terms purporting to describe different conditions. To Victorians, no single behavior could uncontroversially be termed "anesthetized." During these debates it was not easy to decide when a patient was insensible. If he moaned, critics claimed he must have been awake; he had merely forgotten the experience. If he lay still, critics took his motionless state as an indication of conscious control over his body. Similar uncertainty surrounded the question of the patient's testimony, because of course this would be one state where you couldn't remain sober as a judge to testify. At the very moment when you were supposed to be keeping track of what was going on, you became unconscious.

As the years passed, between 1842 and 1846, mesmerism became increasingly successful. One major boon to the campaign was the introduction of mesmerism to India, where a certain kind of operation was particularly helped by anesthesia. These were operations for the removal of large tumors, or hydroceles, particularly of the scrotum. Scrotal hydroceles were not uncommon in India and could grow to enormous sizes (in some cases the diseased scrotum weighed more than the rest of the individual's body). They were extremely hard to remove in the years before anesthesia, because patients usually died of shock on the operating table. When mesmeric anesthesia began to be practiced in India, it became particularly well known for its successful application to these dramatic and horrible cases. Back in Britain, Victorian assumptions about the "simpleminded" nature of India's indigenous peoples made these operations into persuasive evidence for the reality of mesmeric phenomena. According to the London journal editors, the Indian subjects were either too naive or dim-witted to fake the effects.

Ether anesthesia

By mid 1846, mesmeric anesthesia looked poised to enter hospitals as a routine surgical technique. Then, in November, the anesthetic properties of the vapor of ether became widely known.

Ether's history was remarkably similar to mesmerism's: its powers over the body became known in the late Enlightenment, when doctors were studying the effects on the body of all kinds of airs. During the first several decades of the

Mesmerists were enraged at the welcome ether was receiving from doctors who had previously rejected the possibility of the suspension of pain. Ether effectively put paid to mesmerisms's best hope for medical legitimacy.

Detail of the painting on page 30 of the first operation under ether. William Morton stands at left, holding the inhaler. Note the pale and motionless appearance of the patient and the professional solemnity of the surgeons. The scene is strikingly different from witnesses' accounts of the event itself.

19th century, ether and mesmerism were both recreational practices in popular-science demonstrations. And during the 1840s, ether was attacked as an obstacle to medical reform. Medical students' fondness for consuming the inebriating vapor in "ether frolics" was undermining their education and encouraging habits of dissipation.

It was largely through the ineffectiveness of a Boston dentist, both in mesmerism and in the administration of nitrous oxide, that inhalation anesthesia was developed in 1846. Horace Wells had been experimenting for some time with mesmerism in the hope of anesthetising his dental patients. But Wells was no mesmerist. His every effort was an abysmal failure. Then, in 1845, he noticed that subjects "drunk" on nitrous oxide during a popular-science demonstration appeared to feel no pain. He immediately arranged a public demonstration of his own, and administered the nitrous oxide himself. He claimed that his idea would bring forth a "new era of tooth pulling." Unfortunately, the procedure did not anesthetize the patient. Wells found to his dismay that practice and skill were necessary for success. He retired in humiliation and later committed suicide when his former dental partner, William Morton, received the credit for discovering inhalation anesthesia only one year later.

Morton had been present for Wells's disastrous performance, and decided to make his own experiments using ether instead. After much practice (incidentally on someone who had asked to be mesmerized), Morton carefully arranged his demonstration of ether on October 19, 1846. Morton administered the ether, and Dr. John Collins Warren performed the surgery. The operation involved a small incision to the jaw, followed by some minor dental work. According to several accounts, the patient moaned and moved restlessly under the knife. Ether had not made him insensible, he later testified, though his pain had been somewhat dulled. The incision had felt to him as though a "hoe" had been "scraped" across his skin. But as Warren finished the surgery, the audience went wild with cheers, throwing papers onto the stage and shouting their enthusiasm. "Gentlemen," proclaimed Warren, "this is no humbug."

Eventually, ether's debut as an anesthetic agent would be revised when it was celebrated in a famous painting of 1880, which hangs in the Boston Medical Library (see page 30). Here, instead of the uproar the audience displayed during the real event, they are sober, calm and serious; in contrast to the patient's testimony at the time, he is not conscious, and all traces of the commotion described in 1846 are gone.

An ethereal epidemic

The London dentist Francis Boott seems to have been the first British practitioner to hear the news about ether, some three weeks later when the post arrived by sea from the east coast of America. He immediately wrote to Robert Liston, the aforementioned professor of surgery at University College Hospital. Liston was on the lookout for techniques that would enhance the powers of surgery without carrying the sort of taint that he thought mesmerism had, and he moved quickly to stage a highly publicized performance of the first operation using ether anesthesia.

Liston's operation was designed to remind his audience of landmark mesmeric demonstrations. The operation, an amputation of the leg at the thigh, was the same procedure that Topham and Ward had performed four years earlier. The setting, University College Hospital, had been the venue for Victorian Britain's very first experiments in mesmerism almost a decade before. During the operation, the patient moaned and stirred restlessly, but did not cry out.

When he had finished, Liston crowed that "this Yankee dodge beats mesmerism hollow." Later that day he wrote to his friend, Professor James

Miller of Edinburgh, exulting, "HURRAH! Rejoice! Mesmerism, and its professors, have met with a heavy blow, and great discouragement." What Liston was celebrating, it would seem, was at least as much a victory over mesmerism as it was a triumph over pain.

The next stage in the battle for control of anesthesia was extensive coverage in the press. In the first six months of 1847, the *Lancet* is said to have published 112 articles on ether anesthesia; and so intoxicated were British doctors with the new technique that one medical journal referred to an "ethereal epidemic" among the profession. The medical press stressed the "medicalness" of ether by positioning it as the opposite of mesmerism; that is, it was scientific and it was restricted to respectable practitioners. No one specified what its scientific principles were, and the claim that ether could be restricted to a select few professionals was wishful thinking. But ether's reputation as being scientific and professional, in contradistinction to mesmerism's quackery, was encouraged throughout the next several years and decades.

The decline of mesmeric and ether anesthesia

Mesmerists were enraged at the positive reception that ether was receiving from doctors who had previously rejected the possibility of the suspension of pain. Ether effectively put paid to mesmerism's best hope for medical legitimacy. Although mesmerism continued to be a thriving practice outside the medical community, even to the point of the establishment of several "mesmeric infirmaries" during the late 1840s, the defeat of mesmeric anesthesia was widely perceived to spell the end of mesmerism's potential legitimacy within medicine. Mesmerism was not taken into hospitals as a preparation for surgery during these years, even when first ether, and then chloroform, were deemed unsuitable anesthetic agents and exchanged for others.

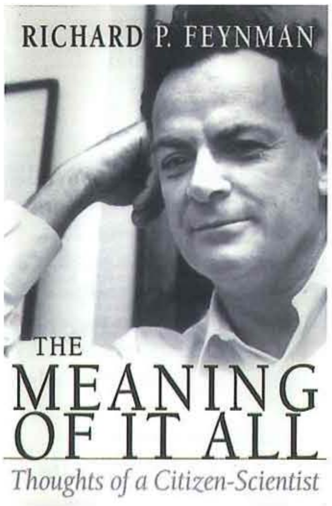
The late 1840s saw the decline of both ether and mesmerism. The magnetic fluids dissipated from the surgical scene during the "ethereal epidemic": but ether's success evaporated as well a couple of years later. By the end of the decade, chloroform had superseded it as the agent of choice. According to contemporary accounts, this was because ether anesthesia was usually preceded by a stage of "exhilaration"—the state associated with ether frolics. The association of ether with drunkenness and with mesmerism could not be eradicated as long as this entertaining phenomenon persisted; nor could the surgeon demonstrate complete control over the subject. Chloroform, on the other hand, generally bypassed the stage of exhilaration. As one surgeon put it, "The time of the surgeon is saved [and] . . . the patient has not the same degree of tendency to exhilaration and talking." One moment the patient was a conscious subject, the next, he or she was a body on the operating table.

As for the longer-term effects of anesthesia, Victorian hospital reports suggest that it did not result in an increase of successful surgeries. Death rates for surgery were still very high from loss of blood and from infection. The assumption that anesthesia must have caused such a revolution is a sign of the success of the campaign to create a perfect profile for ether anesthesia in the pages of the *Lancet* for 1847, a veil dropped over the messy controversy surrounding the emergence of inhalation anesthesia.

Dr. Robert Liston was professor of surgery at University College London and the acknowledged master of the early Victorian surgical stage.

The controversy over anesthesia cannot be explained in terms of a simple duel between the establishment and the fringe, since it was the construction of mesmerism as deviance that was at stake. Mesmerism, then ether, and later chloroform were seen as potentially important tools in the construction of a professional relationship between surgeon and patient; mesmerism, then ether, then chloroform were marginalized within a short space of time. One of the most general lessons of this story is that we tend to think of great scientific and medical discoveries as being independent of their original cultural and social contexts. When these contexts are reconstructed, the process of discovery can become less of a single, isolated, and sudden event, and more of a choice between competing alternatives, whose merits look very different when they are understood from the perspective of the people of the time. ☐

This article was adapted from Alison Winter's Seminar Day talk in May 1998. Winter, who earned her BA from the University of Chicago in 1987 and her PhD from the University of Cambridge in 1993, has been assistant professor of history at Caltech since 1994. Her book Mesmerized: powers of mind in Victorian Britain, *published by the University of Chicago Press, will appear this fall.*



Addison-Wesley, 1998, 122 pages

by David Goodstein

If you go to the science section of your local bookstore, chances are you'll find a shelf full of books by or about Richard Feynman. He seems to be endlessly fascinating to scientists and nonscientists alike. I confess to having coauthored one of those books, intended as a tribute to my friend and colleague at Caltech for more than 20 years. Before I knew him personally, however, in April 1963 he came to Seattle to give three public lectures under the general title "A Scientist Looks at Society," part of a series of guest lectures at the University of Washington known as the John Danz Lectures. I was at the time a graduate student in physics at U-Dub (as we called the U. of W.) and Feynman, although he had not yet won his Nobel Prize, was already a legendary figure. A visit to U-Dub by the great man was a very exciting occasion.

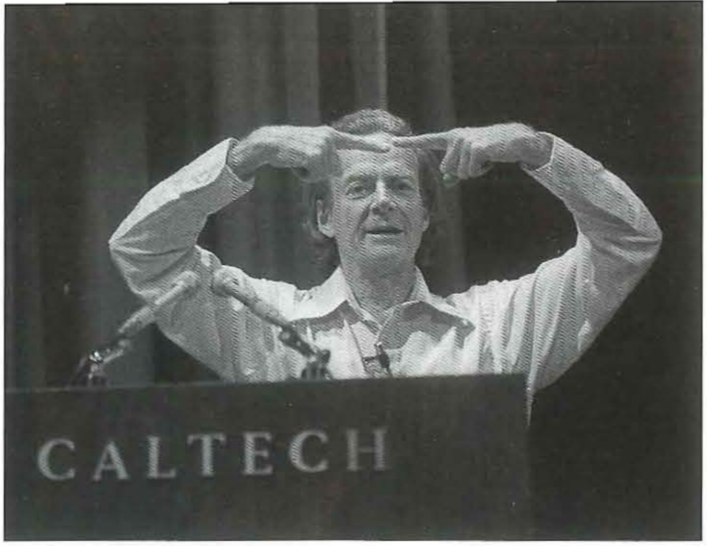
Addison-Wesley has now published Feynman's Danz Lectures under the inappropriate title *The Meaning of It All.* I read through the review copy that was sent to me, eager to find those vivid moments that, even after 35 years, stand out in cherished memory. One was the point at which, much to the delight of Feynman and the rest of his audience, the entire psychology department stood as one and marched out in a huff (of course it may not have happened that way. This is a 35-year-old memory we're talking about). I found it in the third lecture when Feynman referred to psychoanalysts and psychiatrists as "witch doctors," because all their complicated ideas about ids and egos and so on, accumulated in almost no time at all, couldn't possibly be right. He also said that, if he were a member of a tribe and he were sick he would go to the witch doctor, because the witch doctor knows more about it than anyone else, but, if memory serves, that was after the psychologists were already gone. In the next few pages he also savages professors of English pretty thoroughly, but probably there were none of those present in the first place.

Another zinger I've repeated often (I have spent an entire career shamelessly stealing ideas from Richard Feynman): While making a point, often lost even on scientists, that you can't verify a theory using the same data that suggested the theory (if only epidemiologists would catch on to this!), he suddenly seems to change the subject and says: "I had the most remarkable experience this evening. While coming in here I saw license plate ANZ 912. Calculate for me, please, the odds that of all the license plates in the state of Washington I should happen to see ANZ 912." So much for a priori probabilities of unlikely events.

Feynman had been invited to give a series of public lectures. In his mind, "public" meant nonscientists, even though most of his audience probably were scientists (like me for example). His general idea was first to try to explain what science and scientific thinking were about, and then to say what a person who thought in that way might have to say about matters like government and religion. So far, so good.

The first lecture, the one that was supposed to explain what scientific thinking is about, he called "The Uncertainty of Science." The uncertainty he had in mind was not that of Heisenberg, but rather that of Karl Popper: that scientists should be skeptical of their own theories, or, in other words, have an open mind. As with most scientists who profess to follow Popper, he consistently refutes himself throughout his lectures.

To Feynman, science has three parts: the facts or body of knowledge, the method or process that we use to establish those facts, and the applications of science, that is to say, technology. To him it's an article of faith that technology follows science. He would regard technology arising on its own as something akin to the Virgin Birth (we'll get to religion shortly). But his real point is that technology is only incidental to the importance of science. He vents his fury on journalists who report (poorly) each new advance in biology, then declare that it



Feynman at Seminar Day in 1978.

He could say more with body language alone than most people can extract from the *Oxford English Dictionary*.

"While coming in here I saw license plate ANZ 912. Calculate for me, please, the odds that of all the license plates in the state of Washington I should happen to see ANZ 912."

will lead to a cure for cancer.

In the second lecture, called "The Uncertainty of Values," he sets out to apply the scientist's open mind to conventional religion (he swears off "fancy theology"; he's interested in everyday religious belief) and to the Cold War struggle between East and West. In each case, in spite of repeated protestations of uncertainty, he winds up firmly taking sides. For example, on Khrushchev's comment that "modern art" looks like it was painted by the tail of a jackass, Feynman's comment is, "He should know."

Feynman on religion is interesting mainly because he clearly feels the need to tread very carefully for fear of offending too many people. He divides religion up into three parts (he seems to like dividing things into three parts): the metaphysical (creation myths, etc.), the ethical, and the inspirational. His analysis is that science undermines the metaphysical part, but has no effect at all on the ethical, because, in fact, scientists have pretty much the same ethical values as everyone else. He laments the fact that the undermining of the metaphysical takes a lot of air out of the sails of the inspirational part, but his view is that the picture of the universe presented by science is pretty inspirational itself. On the delicate question of whether we are justified after all in believing in God, he gives us the one paragraph in the entire book that justifies the title *The Meaning of It All:*

"It is a great adventure to contemplate the universe, beyond man, to contemplate the universe without man, as it was in a great part of its long history and as it is in a great majority of places. When this objective view is finally attained, and the mystery and majesty are fully appreciated, to then turn the objective eye back on man viewed as matter, to see life as part of this universal mystery of greatest depth, is to sense an experience which is very rare and very exciting. It usually ends in laughter and a delight in the futility of trying to understand what this atom in the universe is, this thing—atoms with curiosity—that looks at itself and wonders why it wonders. Well, these scientific views end in awe and mystery, lost at the edge in uncertainty, but they appear to be so deep and so impressive that the theory that it was all arranged as a stage for God to watch man's struggle for good and evil seems inadequate."

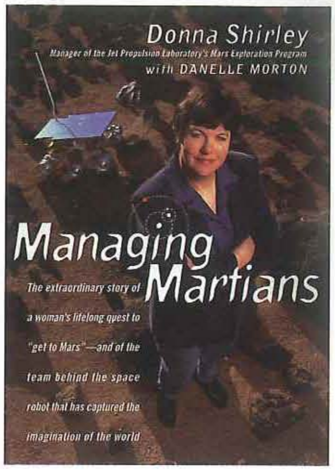
He starts the third lecture, "This Unscientific Age," with the announcement that he had used up all his organized ideas in the first two. There are a number of other points that bother him, however, and those he will discuss here. This is the lecture that had in it both of the moments I remembered, and along the way another that I don't know how I could have forgotten. He tells the story of a snake-oil salesman he heard speak in Atlantic City, selling bottles without the legally required warning labels. By the end of his talk he's gotten his gullible audience to affix the labels to the bottles. "This," Feynman announces, "is what I did in the second Danz lecture." He had started out by claiming an open mind on, for example, politics, but by the end, there was a label on his bottle.

There are, then, some nuggets of pure Feynman gold in this book. So why did it take so long to get published? The answer is that, according to the University of Washington Press, they tried strenuously at the time to get Feynman to permit them to publish, but he wasn't having it. And he was right. Feynman in person was electrifying, no matter what he spoke about. He could say more with body language alone than most people can extract from the *Oxford English Dictionary*. But on paper, dealing with matters far from his comfort zone, Feynman is quite another matter.

The book is badly dated and atrociously edited. Many pages make the reader squirm with embarrassment. Sometimes he's just a little off the point. He trashes those English professors not for the relentless banality of most literary criticism, but for not producing a rational scheme of spelling for the English language. At other times it's just not the right stuff. Feynman doing battle with the earnest ladies of the Altadena Americanism Center has some of the same spirit as the famous stories he liked to tell about himself, but it's neither racy nor funny, just quaint and somewhat silly. And there are many references to names or things that had meaning in 1963 but not anymore. Who was Mr. Nakhrosov? Mr. Anderson? (He was somehow mistreated by the American military). Do you remember what "the farm problem" was? The editors do nothing to help us in these matters. Addison-Wesley's attitude is, clearly, take the money and run.

The publication of this book now, with Dick Feynman no longer here to defend himself, does not honor his wishes, and it does not honor his memory. You'll find this book on the Feynman shelf in your bookstore. Don't buy it.

This review first appeared in the July–August 1998 issue of American Scientist. *David Goodstein is professor of physics and applied physics, the Gilloon Distinguished Teaching and Service Professor, and vice provost at Caltech.*



Broadway Books, 1998, 276 pages

by Al Hibbs

" 'We have a signal!' the flight engineer yelled.

"The team exploded in cheers and hugs and impromptu jigs. Even though I was in full view of the CNN audience, broadcasting live, I did my own modest victory dance. Pathfinder hadn't crashed or burned! It was on the surface of Mars—and alive. My Christmas package had arrived. I restrained myself from hugging the nearest available person—the CNN reporter.

" 'Did I just see you wipe away a tear?' he asked with astonishment."

This excerpt is from the first chapter of Donna Shirley's memoir—a chapter that gives a fast-reading account of the development of the Mars lander at Caltech's Jet Propulsion Laboratory (JPL), and the climactic events of July 4, 1997, when the spacecraft, called Pathfinder, completed its seven-month journey from the surface of Earth to the surface of Mars. Pathfinder was carrying a small roving vehicle named Sojourner Truth. (In a later chapter we learn how and why it got that name.) This rover, although firmly strapped down to the Pathfinder for its interplanetary trip, was developed as a completely separate project—a project managed by Shirley, the first female spaceflight project manager at JPL, and, I believe, for all of NASA. In the rest of the book we learn how this came about and what some of the consequences have been.

At the age of 10 she found her career goal—aeronautical engineering. In high school she got out of a home economics requirement in order to take a mechanical drawing class, wherein she was looked upon as a sort of joke by both her fellow students (all male) and her teacher. (An aside: I have known Donna for many years and worked with her from time to time at JPL. On more than one occasion one of our colleagues has quietly assured me that, as a woman, she shouldn't be taken seriously as an engineer.) But she kept to her goal of engineering and her interest in aeronautics. She learned to fly and soloed at 16.

The brief story of her early life carries us through her college years, sometimes difficult, but including winning a hometown beauty contest and becoming Miss Wynnewood (Oklahoma). Her early professional career brought her eventually to JPL. Here she was involved in a number of study and analysis projects. The main body of the book describes these activities and the engineering challenges they involved. Although she does a pretty good job of avoiding jargon (not a perfect job: for example, "six-degree-of-freedom equations" goes by without comment), we still get a heavy dose of technical explanations. If you're an engineer, you'll probably enjoy it.

For Donna, JPL seemed to offer the hope of fulfilling a dream held since childhood—flying into space, particularly to Mars; and, if not going personally, at least with a piece of machinery she could truly call her own. This required getting on a flight project, where the engineering requirements are much stricter than for study projects. But that goal was elusive. She had worked on studies of Mars missions and even the development of Mars-rover prototypes. When it was decided to turn prototypes into flight hardware, she applied for the job of running the project and was turned down with the Catch-22 excuse that, because she had no experience in delivering flight hardware, she couldn't get a job delivering flight hardware. When she finally did get the assignment, she suspected that it was because all the experienced candidates believed it would never be successful.

The Pathfinder spacecraft, which carried the rover to Mars, was developed and operated under the project management of Tony Spear, an engineer with many years of flight-project experience. It was inevitable that Donna and Tony would have a difficult relationship. Every project manager wants everything that might affect the success of his project under his control, so Tony was understandably dismayed at the setup with Donna and tried to get the rover either canceled or placed under his management. Donna recounts a meeting with Tony and his senior staff in which he demanded that she turn over her budget allocation to him. Of course she refused, and the ensuing shouting match was ended only when an engineer from an adjoining office complained that they were interrupting his meditation.

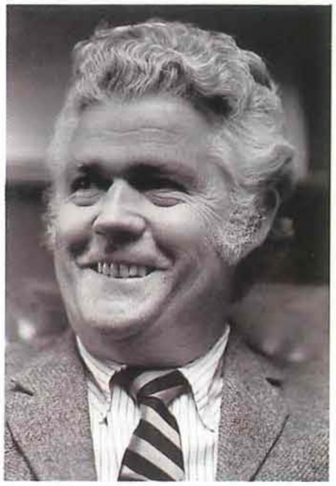
And how did Sojourner Truth get its name? After developments were pretty well started, Donna had her "bright idea," which was destined to get her in trouble: She would have a naming contest. Young students were asked to propose the name of a woman who had done much for humanity, and back up their choice with a 300-word essay. When a NASA bureaucrat got wind of the contest, he ordered it stopped, but it was too late. Essays were already pouring in. A few months later, the project got an official NASA reprimand for the undignified contest that had not "followed proper procedures." Four years later the same bureaucrat was publicly praising the young contest winner for her choice of the name!

Sojourner Truth was well along in development but still a couple of years from launch when Shirley was offered the job of Mars Program Manager for JPL. This meant responsibility for planning all the projects intended to explore Mars. In describing this job she reveals a certain lack of historical perspective, saying, "Certainly no one at JPL had any experience building a program." To the contrary, program plans were a regular output of JPL—and NASA. In 1959, shortly after joining the newly created space agency, JPL published a plan for the exploration of the solar system, including Mars. In 1976 NASA published a massive plan called "Outlook for Space" that involved program planners from every NASA center, including JPL. A planetary exploration plan was described therein, including a plan for Mars. This was followed two years later by the publication of a plan called "Exploration of the Solar System," put together at JPL. In 1983, NASA published "Planetary Exploration Through the Year 2000," again with a plan for Mars with inputs from JPL planners. Indeed, there has never been a lack of plans for exploring Mars and the rest of outer space. What has been lacking is consistent funding for this piece of the space program, and the determination of the NASA bureaucracy to stick to any plan. Perhaps Shirley's plans will be more successful. To quote the last line of her book, "Stay tuned."

Al Hibbs, BS '45, PhD '55, is retired from a long career at JPL, where he was senior staff scientist and manager of program planning and coordination. In his capacity of public spokesman for JPL, he was known as the Voice of Surveyor, the Voice of Mariner, the Voice of Viking, and the Voice of Voyager.

This rover, although firmly strapped down to the Pathfinder for its interplanetary trip, was developed as a completely separate project—a project managed by Shirley, the first female spaceflight project manager at JPL, and, I believe, for all of NASA.

Robert W. Oliver
1922–1998



Robert W. Oliver, professor of economics, emeritus, at Caltech, died Friday, July 17, of a heart attack in Pasadena. He was 75.

A native of Los Angeles, Oliver earned his bachelor's degree in international relations and economics from the University of Southern California in 1943. He then focused his attention solely on economics, earning his master's in that subject in 1948, also from USC. For his doctorate, he again concentrated on economics, earning his degree from Princeton University in 1957.

His academic career covered several institutions. Before coming to Caltech, he was a teaching assistant at USC from 1946 to 1947, an instructor of economics at Princeton in 1948, an assistant professor at USC from 1952 to 1956 and a research economist at the Stanford Research Institute from 1956 to 1959. He became an assistant professor of economics at Caltech in 1959 and a full professor here in 1974. During his time at Caltech, he was also an economist at the World Bank, and a consultant to the Brookings Institution and the Organization for Economic Cooperation and Development in Paris. While at Caltech he also served as Master of Student Houses from 1987 to 1988 and chaired the convocation committee as well as serving on several other Institute committees.

He held fellowships at the London School of Economics and the Rockefeller Foundation. He was a member of several professional associations, including the Royal Economic Society, the American Economic Association and the International Institute for Strategic Studies.

Oliver also served in several positions with the city of Pasadena, including the Pasadena Citizens Downtown Improvement Board, the Pasadena Board of Directors (the Pasadena City Council), the Planning Commission, and the Future Land Use Committee. He was a current member of the Pasadena Utility Advisory Committee.

J. Kent Clark, professor of literature, emeritus, spoke at a service at Glendale Forest Lawn July 23; parts of his talk are excerpted here. Oliver had delivered Clark's elegy on David Wood and the Caltech Stock Company (of which all three men were charter members) at Wood's memorial service in April. A campus memorial service for Oliver is planned for the fall.

A lovely and fundamental fact about Bob is that he had music in his corpuscles. He grew up with the Gershwins, Jerome Kern, Cole Porter, Ray Noble, Rudy Vallee, Duke Ellington, and Louis Armstrong. He helped to dance in the swing era with Benny Goodman, the Dorseys, Glen Gray, and Glenn Miller, and of course he danced miles to "In the Mood." In the process of soaking up the music he also soaked up song lyrics. There may be a few Crosby, Sinatra, or King Cole lyrics he didn't know, but they are probably not worth knowing. After World War II, he made a fine tape (with commentary) of the songs that had consoled and heartened GIs around the world and their women in the shipyards. To Bob, the songs were friends; and Bob did not forget his friends.

It is absolutely typical of Bob that he specified the music he wanted played at his memorial service—the music we are hearing today. And I hope there is no one here under the age of 55 who cannot recognize most of the tunes. And if there is anyone who doesn't recognize any, he is either tone deaf or he has wandered into the wrong service. For years, incidentally, Bob and I tried to make a definitive list of the 10 top songs of our century. We juggled the order from time to time, added one or subtracted one; but one song always stayed at the top of Bob's chart: "I Get a Kick Out of You." This is typical of Bob's taste in songs: buoyant, neatly crafted, and melodic, with the sentiment happily understated.

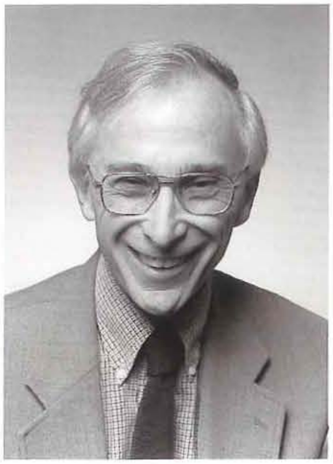
Again, it is typical of Bob that at his desert house at La Quinta, which his mother built, he kept a great collection of 78-rpm records, as well as an old-style radio phonograph and changer that would handle them. But although it is completely characteristic for Bob to hold onto his old records, it is also characteristic and symbolic that he made tapes of them and acquired CDs of well-engineered re-pressings. He kept his music available. One of the reasons Bob loved songs was that songs collapse time, and Bob wanted his past brought into his present. He lived very well in the present, as we all know, and he studied it with great skill, but he didn't forget how he got here or what great people had made the journey worth taking.

How he got here brings us, naturally, to the St. Louis Cardinals. The size of L.A. and the primitive state of air travel meant that Los Angeles didn't have a major league team. Bob adopted the Cardinals—not the ersatz Cards of the expansion days, but the genuine article. They won the World Series in '31 and '34, and they were a match for the redoubtable Yankees, whom they beat in '26 and '42. The Brooklyn Dodgers, on the other hand, were the boys of summer and the failures of fall. When they finally started winning league championships, they routinely lost the series. Their lone, puny victory in '55 couldn't atone for their flops.

Well, we all know Bob Oliver and loyalties. When the Dodgers moved to L.A. and changed their losing ways, they didn't change Bob's mind. He was with his old friends in sickness and in health, in 1967 and 1968, winning and losing. Granted that the Cards are an odd symbol of Bob's permanent attachments, they are a great one nevertheless.

And there is one more, which may be even odder and greater. That is the round table at the Athenaeum. Bob loved to lunch there with his longtime Caltech colleagues. They settled the state of the arts, the state of the nation, the economy, international politics, the condition of the cosmos, and the future of the Institute. They kept Bob and each other firmly grounded in the current world. Last Friday they almost settled the seismic future of Altadena, but there were data lacking and Bob told me a James Thurber story. I think Bob would not complain if, for now, we left him there laughing and talking with his marvelously bright friends. But let's not. Let's leave him at home with Jean, sipping a drink as they watch the TV, seeing Mark McGwire hit two home runs, and seeing Bob's Cardinals beat the unrecognizable Dodgers. □

TOMBRELLO NAMED DIVISION CHAIR



Thomas Tombrello has been named chair of the Division of Physics, Mathematics and Astronomy, succeeding Professor of Physics Charles Peck, who had been division chair since 1993.

Tombrello, also a physicist, and his research group are primarily involved in applying the techniques of theoretical and experimental physics to problems in materials science, surface physics, and planetary science. His ongoing research includes understanding the damage processes caused by megavolt ions in solids, characterizing the sputtering of materials by low-energy ions, and growing and studying novel light-emitting materials.

A native of Texas, Tombrello was born in Austin, grew up in Dallas, and earned his BA, MA, and PhD degrees at Rice University in Houston.

Tombrello came to Caltech in 1961, and except for a brief stint on the Yale faculty, has been here ever since. A full professor of physics since 1971, he also served as vice president and director of research at Schlumberger-Doll Research from 1987 to 1989. He was named William R. Kenan, Jr., Professor at Caltech in 1997.

Tombrello said that his immediate goals will be to strengthen the division's efforts in theoretical physics, mathematics, and observational astronomy. □

HONORS AND AWARDS

Thomas Ahrens, W. M. Keck Professor of Earth Sciences and professor of geophysics, has been selected as a Geochemistry Fellow for 1998 by the Geochemical Society and the European Association for Geochemistry, for his outstanding contributions to geochemistry.

Assistant Professor of Biology José Alberola-Ila has been named a 1998 Pew Scholar as part of the Pew Scholars Program in Biomedical Sciences. He studies signal transduction in the immune system.

Michael Alvarez, associate professor of political science, has been selected to serve as an executive council representative for the Western Political Science Association, 1998–2001.

Tom Apostol, professor of mathematics, emeritus, received the Trevor Evans Award of the Mathematical Association of America, presented to authors of exceptional articles that are accessible to undergraduates. Apostol's prizewinning article on the prime number theorem, published in *Math Horizons*, covered some of the same material as an article that originally appeared in *E&S* (1996, No. 4).

The face of Ahmed Zewail, the Pauling Professor of Chemical Physics and professor of physics, now graces two Egyptian postage stamps, issued in tribute to his scientific achievements. Zewail attended their unveiling at a June 14 ceremony in Cairo, Egypt. "I am particularly pleased as this honor comes from my country of birth, and that I could be in the company of stamps honoring the pyramids, Tutankhamen, and Queen Nefertiti," he said. "And it's nice that they do it while you're still alive." The one-pound stamp (yellow) is for international mail. The 20-piastre stamp (blue) is for first-class domestic mail.

HONORS AND AWARDS CONTINUED

Jacqueline Barton, Hanisch Memorial Professor and professor of chemistry, has been given the 1998 Weizmann Women & Science Award as "an innovative scientist, inspiring educator and eloquent advocate for basic research." The award is issued biennially by the American Committee for the Weizmann Institute of Science.

Peter Dervan, Bren Professor of Chemistry and chair of the Division of Chemistry and Chemical Engineering, has been chosen for the 1997 Remsen Award by the Maryland Section of the American Chemical Society.

Assistant Professor of Biology Bruce Hay has been named a 1998 Ellison Medical Scholar as part of the Ellison Medical Foundation New Scholars in Aging Program. The grant is aimed at identifying the proteins that control programmed cell death—a critical step not only in the aging process but in normal embryonic development.

JPL astronomer Eleanor Helin, principal investigator for the Near Earth Asteroid Tracking program, has been named to the Women in Science and Technology International Hall of Fame.

Professor of Applied Mathematics Yizhao Thomas Hou will share the 1998 Francois Frenkiel Award with Michael Shelley of the Courant Institute and John Lowengrub of the University of Minnesota. The award, given by the Fluid Dynamics Division of the American Physical Society, recognizes the best paper to appear in the journal *Physics of Fluids*.

Alice Huang, senior councilor for external relations and faculty associate in biology, has been named to the board of trustees of the Keck Graduate Institute of Applied Life Sciences, the newest component of the Claremont Consortium of Colleges.

Professor of Chemistry Barbara Imperiali has been awarded the 1998 Richard P. Feynman Prize for Excellence in Teaching.

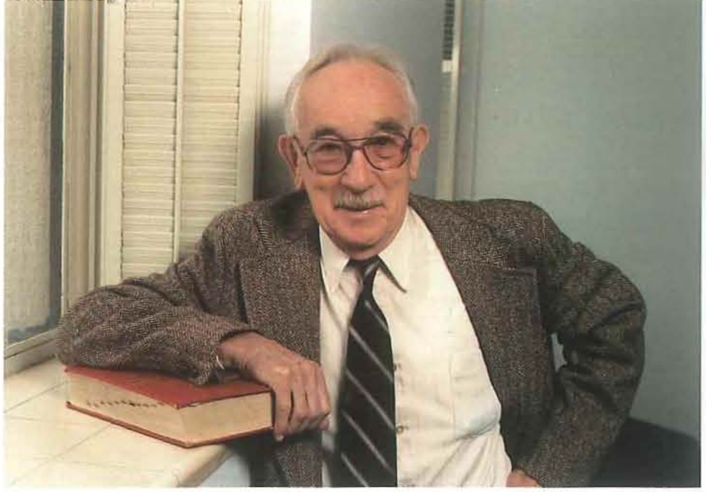
Richard Roberts, assistant professor of chemistry, has been named a Beckman Young Investigator by the Arnold and Mabel Beckman Foundation. The award will enable him to improve a technique he developed for the design and isolation of the signaling molecules involved in various biological processes.

Professor of Anthropology Thayer Scudder has been selected by the Royal Anthropological Institute of Great Britain and Ireland to be the first recipient of the Lucy Mair Medal for Applied Anthropology "in recognition of his application of anthropology to problems of sustainable economic development...his influence on governments and donor agencies has led the way for anthropologists contributing on policy issues." Meanwhile, the Society for Applied Anthropology has tapped him for the 1999 Bronislaw Malinowski Award, which honors "the application of the social sciences to contemporary issues"—in this case, "distinguished work on the social impacts of resettlement."

The organizers of the 15th International Conference on the Application of Accelerators in Research and Industry, to be held November 4–7 in Denton, Texas, have decided to dedicate the conference proceedings to Tom Tombrello, Kenan Professor and professor of physics, and recently appointed chair of the Division of Physics, Mathematics and Astronomy, for his "vast number of contributions to the ion beam community."

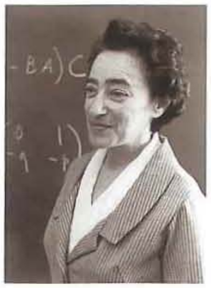
Amnon Yariv, Summerfield Professor of Applied Physics, has received the Esther Hoffman Beller Award from the Optical Society of America, "for outstanding contributions to optical science and engineering education."

Ahmed Zewail, the Pauling Professor of Chemical Physics and professor of physics, has been awarded the University of Würzburg's Roentgen Prize. ☐



MATHEMATICAL GIFTS

For much of the second half of this century, John Todd and Olga Taussky Todd were two of the leading figures of Caltech's mathematics department and among the most prominent mathematicians in the world. Olga Taussky Todd, who died in 1995, was an expert in number and matrix theories and was the first woman to achieve the rank of full professor at Caltech. Her husband, John, now professor of mathematics, emeritus, is a world renowned numerical mathematician.



Olga Taussky Todd in 1964.

Although the Todds were affiliated with several institutions during their lifetimes, their strongest bond was with Caltech. In the 1980s they decided it was time to give something back to the Institute, and they created the first in a series of charitable trusts that will help ensure Caltech's future in mathematics.

While the charitable annuity trusts provide Todd, who retired in 1981, with quarterly income and substantial income tax deductions, his and his wife's primary motivation was to benefit Caltech. "We enjoyed being here and thought that establishing charitable trusts was a good thing to do," Todd said. "Olga had no family and my family was flourishing. Effectively, Caltech was our family."

The Todds met in 1937 when they were both lecturers at the University of London. The following year, they were married. During World War II, Todd was a scientific officer for the British Admiralty, and immediately after the war he helped preserve a German mathematics research institute in the Black Forest that is still an active research center. "That was my best contribution to mathematics," quips Todd. "French troops were coming to get the mathematicians out and probably destroy the building. I told them that the British navy had taken it over, and they went away. The people there didn't really work on military problems. They wrote books."

In 1947, the Todds were hired by the National Bureau of Standards in Washington, D.C. Todd became chief of its Computation Laboratory and then Chief of Numerical Analysis. But they missed teaching, and in 1957, they both received appointments at Caltech; Todd as a professor and his wife as a research associate. Fearing the uncertainties of the stock market, the Todds put most of their money into certificates of deposit. When the certificates started to mature, they used the money to establish charitable gift annuities at Caltech.

"During our careers at Caltech, we benefited greatly from contact with visitors, in particular those who came under the Sherman Fairchild Distinguished Scholars Program," said Todd. "Our money was made from mathematics, and should go back to mathematics." When Todd dies, the funds will be used to establish a program in mathematics similar to a miniature Fairchild Program. His house will also be left to Caltech.

If you'd like more information about ways to benefit yourself and Caltech, please contact us.

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