California Institute of Technology

Engineering & Science

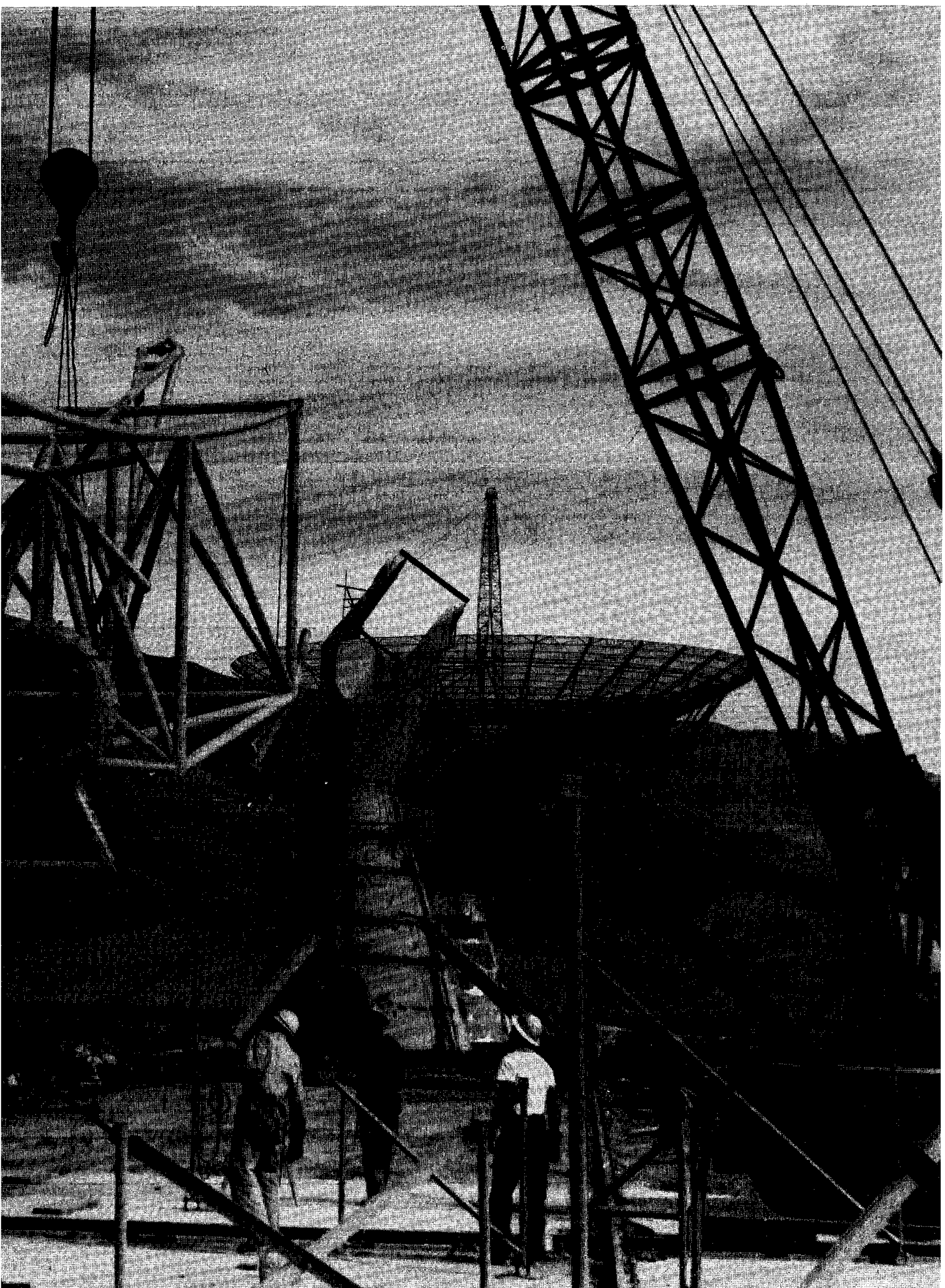
Spring 1994

In this issue

Plastics that Bend Light

A Brief History of OVRO

India's Forgotten Army

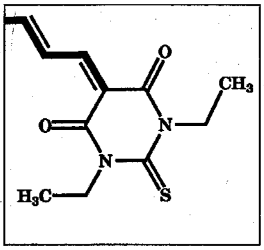


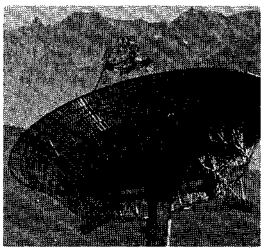
Caltech's first radio observatory begins to take shape in the Owens Valley in the late 1950s. One of the 90-foot antennas of the interferometer stands virtually complete in the bakground, while the pedestal of the second waits to grasp in its thumb-and-forefinger-like mount the other dish, whose frame is just getting started in the foreground.

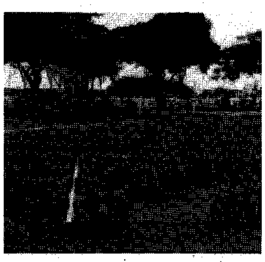
California Institute of Technology

Engineering & Science

Spring 1994
Volume LVII,
Number 3



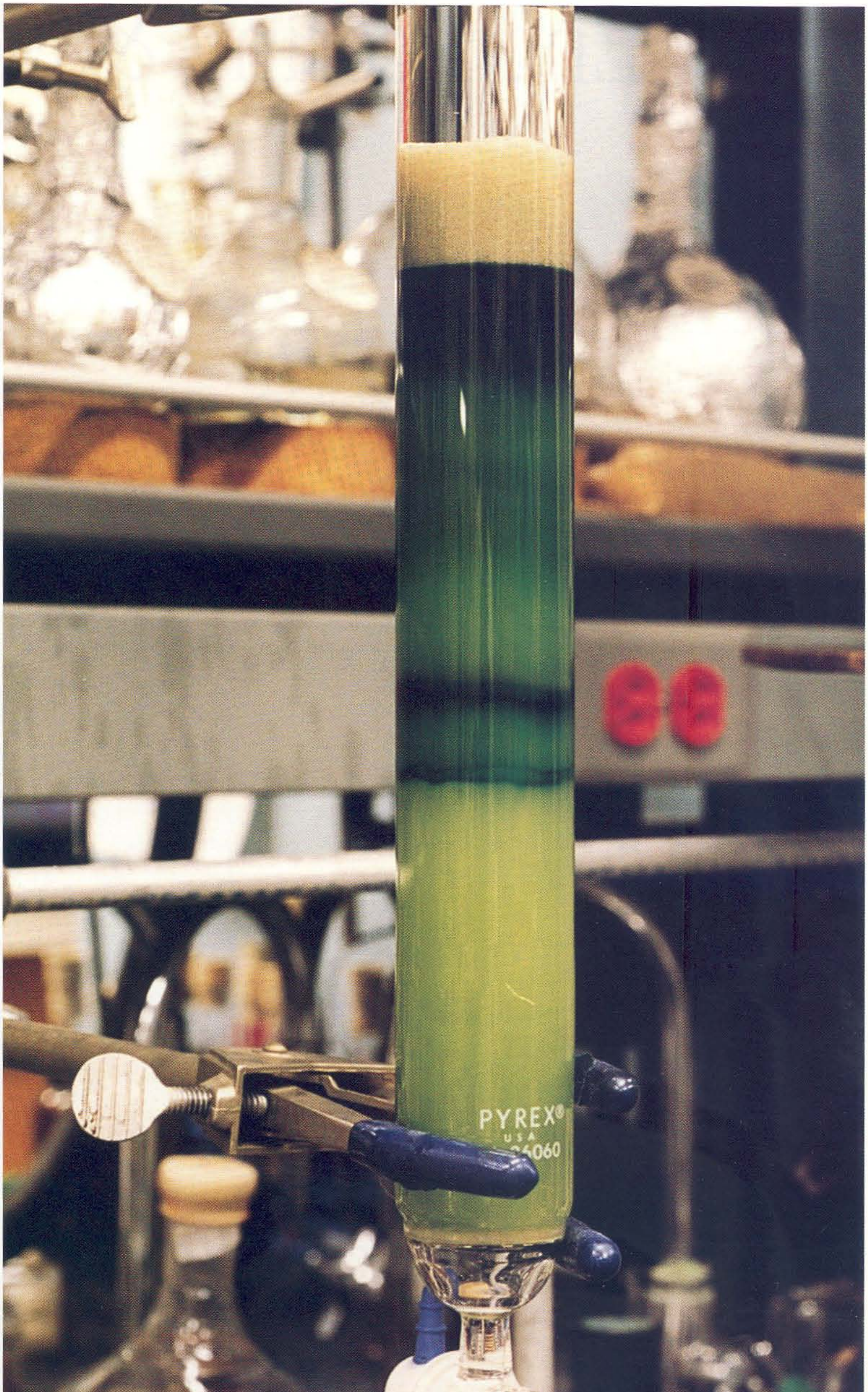




On the cover: These brightly colored vials are filled with solutions of synthetic organic dyes. These molecules' electronic structures allow them to absorb certain wavelengths of light very strongly, giving them their intense colors. Sometimes these structures also make the molecules interact with light in considerably more unusual ways that may be vital to the fiber-optic information superhighway of the future. Story on page 2.

Engineering & Science (ISSN 0013-7812) is published quarterly, Fall, Winter, Spring, and Summer, at the California Institute of Technology, 1201 East California Boulevard, Pasadena, California 91125. Annual subscription $8.00 domestic; $20.00 foreign air mail; single copies $2.00. Third class postage paid at Pasadena, California. Published by the California Institute of Technology and the Alumni Association. Telephone: 818-395-3630. Postmaster: Send change of address to Caltech 1-71, Pasadena, CA 91125.

PICTURE CREDITS: Cover, 2 – Bob Paz; inside front cover, 15, 16, 18 – John Bolton; 4 – Doug Smith; 5–7 – Seth Marder; 10 – R. W. Porter; 12 – Tom Harvey/Caltech Archives; 13 – Caltech Archives; 13, 16 – Dick Read; 17 – U.S. Navy; 26–28, 31 – Prem Sahgal; 32 – Linda Weavers; 33, 34, 38 – Michelle Owen; 36 – *Sky & Telescope*, Eric Wemhoff and Al Ratner; 37, 44 – NASA; 43 – White House; inside back cover – Paul Chodas and Don Yeomans


PYREX®
USA

Any Color You Like

On the fiber-optic lanes of the information superhighway, nonlinear-optic materials may one day provide the off ramps and cloverleaf interchanges.

by Douglas L. Smith

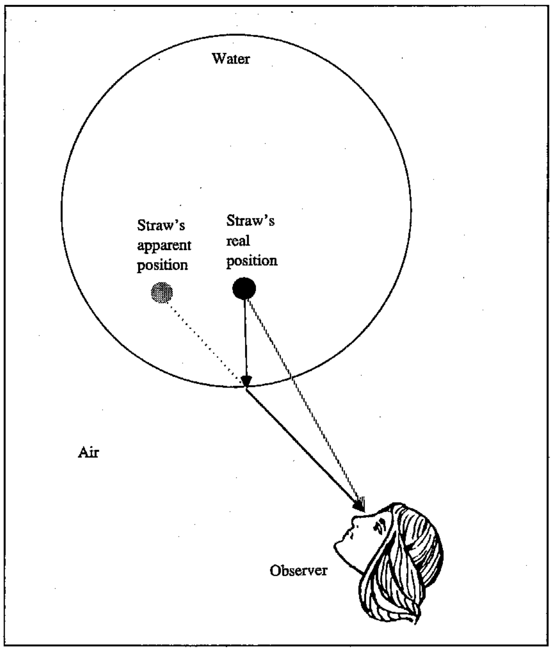
In Marder's lab, nonlinear-optical compounds are purified by running them through silica-gel-filled columns like this one.

If you shine a red light through a pane of glass, it comes out red. But shine that same light through a nonlinear-optic material, and it might come out blue. Or it might come out red, but at a funny angle. A nonlinear-optic material modifies light passing through it in a way that depends on the light itself, among other things. On the fiber-optic lanes of the information superhighway, nonlinear-optic materials may one day provide the off ramps and cloverleaf interchanges, routing data-laden pulses of light to their proper destinations. Seth Marder, a member of the technical staff at Caltech's Jet Propulsion Laboratory and a member of the Beckman Institute on campus, and Joseph Perry (PhD '84), leader of the optoelectronic materials group at JPL and a visiting associate at the BI, lead a group that has created a whole new class of substances with the highest nonlinear-optic properties yet found.

Light is an electromagnetic wave, so it's no surprise that it interacts with electrically charged things, including the electrons in a molecule. As light passes through a molecule, the molecule's electrons ride the waves like boats at anchor. As the electrons surge against their moorings, the molecule becomes polarized—it acquires a slight negative charge in the direction the electrons are moving, and a slight positive charge in the region they've vacated. In materials such as glass, the electrons move back and forth with equal facility, and the distance an electron moves in either direction is proportional to the wave's amplitude, at least to a first approximation. Thus a plot of polarization versus the applied field is a straight line—a linear, symmetric response.

But if the molecule is already polarized, applying an electric field in the direction of polarization can shove the electrons a considerable distance. Applying the same field in the opposite direction pushes the electrons against the tide, and they won't move nearly as far. Now the plot of net polarization versus the applied field is steep in one direction and flat in the other—a nonlinear, asymmetric response. This effect, called "hyperpolarization," is relatively weak, becoming noticeable only in the presence of strong fields. There, however, the effect becomes quite powerful, since it varies with the square of the field's strength. Only molecules lacking a center of symmetry are hyperpolarizable in this manner—a symmetrical molecule would have an equal desire to polarize in either direction. (This kind of hyperpolarization is what's called a second-order effect—there are other, higher-order hyperpolarizations beyond our scope here.)

The interplay of polarization and hyperpolarization has several useful consequences. You can bend light by manipulating the material's refractive index. When a beam of light hits a molecule at the edge of a chunk of matter and sets its electrons bobbing, the oscillating electrons themselves generate an electromagnetic field at the same frequency as the original wave. The new field travels to the next molecule, and the light propagates through the material. Each molecule imposes a tiny time lag as its electrons respond—in essence, the light slows down. This time lag, relative to that for light through air, is the material's refractive index. (This is why a straw sticking out of a glass of water appears bent: light



An overhead view of a glass of water. Light travels by the quickest path (solid arrows), and since light travels more slowly in water than in air, the quickest path is not the shortest path (shaded arrow). But we *know* that light travels in straight lines, so we see the straw in a different location from where it actually is.

travels one-third slower in water than in air. Light reaches your eye by the fastest possible route rather than the shortest possible route, and as a consequence takes the path that minimizes the time spent traveling through the slower medium.) Applying a second, nonoscillating field slows the wave further—the electrons have to fight harder to get back to their initial position to begin the next oscillation. As the second field intensifies, the refractive index gets higher, until eventually the light beam emerges from the material in a radically different direction. Thus, laser signals could be shunted from one fiber-optic line to another by applying a switching field to a nonlinear-optic material at the lines' junction. The switching field could be an applied voltage, or it could be another laser beam.

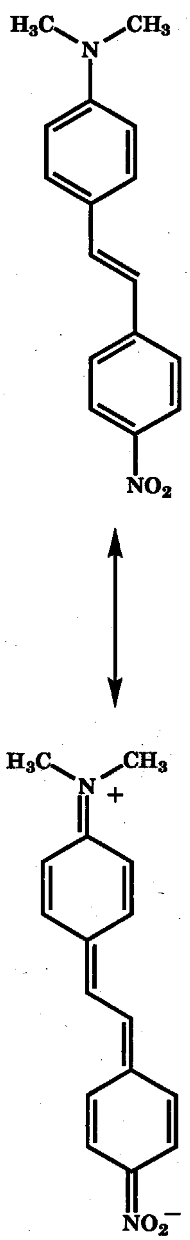
And you can change the light's color. The oscillating electrons not only rebroadcast the light at its original frequency, they also emit energy at twice that frequency. (These higher frequencies—harmonics—are analogous to the harmonics of sound waves. You can hear harmonic generation the next time you have a piano handy: Press down on the C above middle C slowly and gently, so that the note doesn't sound. While holding that key down, hit middle C hard. Let go of middle C, but continue to hold the other one. The ringing you hear is the second harmonic of middle C causing the string an octave above it to resonate.) Under the right circumstances, upward of 80 percent of the light can be rebroadcast as the second harmonic; 35 percent is routine for commercial devices. Additional frequencies can be generated by combining the fundamental frequency with other frequencies, an effect that comes in very handy when working with lasers. A laser emits light at one specific frequency that depends on what the laser is made of. But this frequency generally isn't the one that a researcher needs, so nonlinear-optic crystals are widely used to "tune" lasers to the right frequency—say, a particular spectroscopic resonance of some molecule. Outside the lab, surgeons need short-wavelength lasers to cut tissue, while longer wavelengths work better to cauterize the incision.

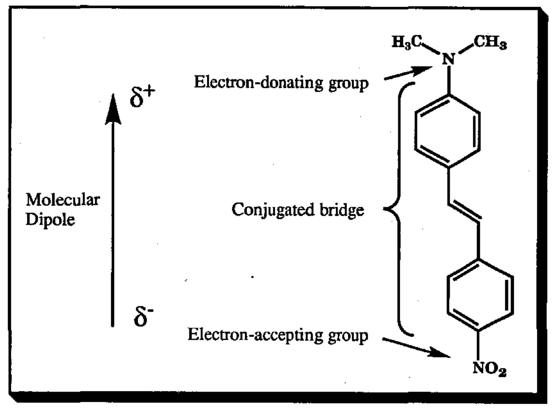
To date, all commercial nonlinear-optic materials have been inorganic compounds such as potassium dihydrogen phosphate and lithium niobate. But their nonlinear effects aren't that large, whereas some organic molecules are highly polarizable and thus should show strong effects. And while lithium niobate and its kin polarize in part by schlepping heavy ions back and forth, organic molecules polarize by whisking featherweight electrons around. Electrons are nimble enough to keep up with the highest frequencies—a very big advantage, says Marder, when you consider that the information superhighway's structural engineers want electro-optic switches capable of operating 10 billion times a second. "If you tried to do that with an inorganic," says Perry, "its efficiency drops way off. It becomes very power-hungry." Organics have other advantages—they can be formed into films and sheets, or molded into any shape you like; and best of all, the molecule's properties can be customized by altering its chemical composition. Lured by these promises, folks have been tinkering with organic molecules for the past two decades.

These molecules share a basic design: a cluster of atoms (the donor group) willing to give up an electron and acquire a positive charge; another cluster of atoms (the acceptor group) that attracts the electron and becomes equally negatively charged; and in between them a stiff bridge, several atoms long, that separates the charges to create a dipole. (The longer the bridge, the farther the charges are separated, and the greater the dipole—at least within limits. If the bridge gets too long—more than 13 atoms, say—the groups' influence fades.) In order to get the donated electron to the acceptor, the bridge contains what chemists call a conjugated π-bond system—a backbone of alternating single and double bonds along which a charge can flow.

Conjugated systems really contain *two* sets of electrons. One set actually holds the structure together, and lives in well-defined bonds between the atoms. The other set—the π electrons proper, and the second half of each double bond, as it

Right: DANS's anatomy. The line segments are carbon-carbon bonds; their intersections are the carbon atoms themselves. The δ^+ indicates a partial positive charge, δ^- a partial negative charge. Left: DANS can be thought of as existing in two forms: the electrically neutral form (top); and the charge-separated form (bottom), in which the nitrogen atom at the molecule's top end has given an electron to an oxygen atom at the molecule's bottom end. Note how all the double bonds have shifted as a result.





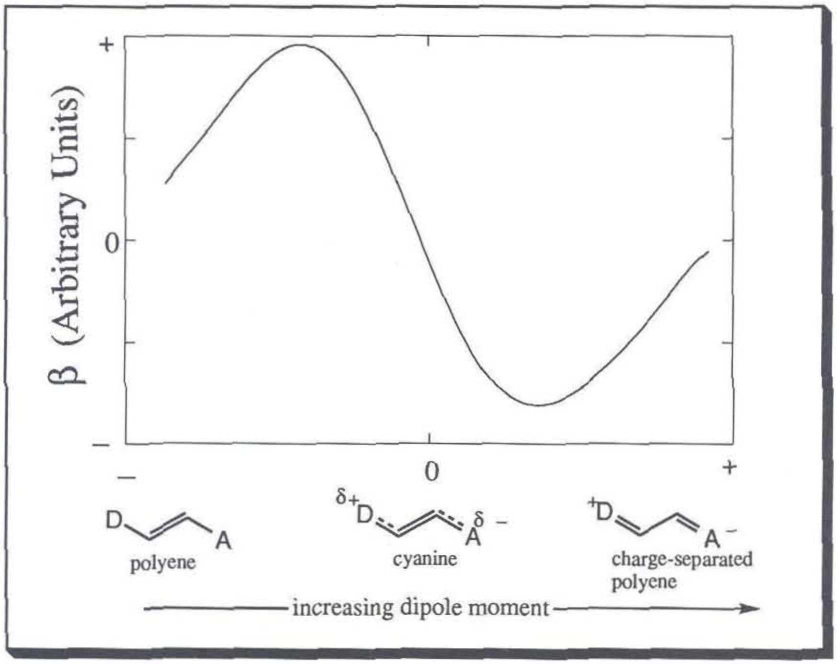
were—is more gregarious. If the opportunity permits, they spend as much time visiting neighboring atoms as they do at home. The π electrons will cruise up and down the system for as far as the alternating double and single bonds extend.

The archetypal nonlinear-optic organic molecule is called DANS (4-N,N'-dimethylamino, 4'-nitrostilbene to the chemically literate). DANS's structure is shown above. DANS has an electron-greedy nitro group—NO_2—and an electron-generous amine—$(CH_3)_2N$—and a bridge of 10 conjugated carbon atoms to carry the charge. Researchers had worked their way up to DANS by attaching progressively stronger donors and acceptors to the bridge. "These are among the strongest donors and acceptors known—at least, as far as things that don't combust spontaneously in air are concerned," says Perry. And, in fact, the nonlinear response was getting stronger, but nobody knew how high it could go. "They hadn't gotten to the end of the rainbow yet."

Marder and David Beratan (PhD '86, now at the University of Pittsburgh), decided to start afresh, working from chemical first principles. "All of the equations for nonlinear optics were derived by physicists," Marder explains. "They played around with things like DANS because they could buy it. You can buy a dye called Disperse Red in kilo quantities, and it's essentially the same stuff. The chemists learned from the physicists what molecules to make—were told the rules—and to a great extent didn't try to derive the rules for themselves. What we did was make the connection between the physics and the chemistry."

The chemistry told Marder and Beratan something very important. Conceptually, a molecule containing an electron donor and an acceptor exists in two forms (shown at left). In one—the electrically neutral form—the donor keeps its spare electron in its trouser pocket. In the other—the charge-separated form—the donor surrenders its electron completely, acquiring a charge of +1, and the acceptor acquires a charge of −1. But as long as the π electrons are loose, the molecule actually exists in a state somewhere between these extremes—a combination of the two forms that resembles the charge-separated one more closely as the donor and acceptor get stronger. If both forms contribute equally to the molecule's character, the donor and acceptor act as if they have charges of plus and minus one-half, respectively. At this balance point, a light wave's oscillating field finds it just as easy to push electrons in one direction as the other. In other words, the molecule no longer responds asymmetrically to the field (i.e., does not hyperpolarize), even though the molecule itself is polarized.

So Marder and Beratan, in collaboration with Lap-Tak Cheng from DuPont, embarked on a series of calculations to see just what combination of donor and acceptor would maximize the asymmetric response. The calculations spanned a continuum having very strong donors and acceptors on one end and very weak donors and acceptors on the other end, and tested the π electrons' ease of motion along the bridge in both directions. "No one had done this kind of detailed analysis before," says Marder. "What we found is that you don't want the *strongest* donors and



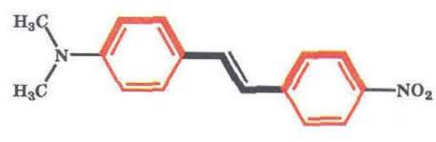


Figure of Merit = 470 x 10^{-48} esu

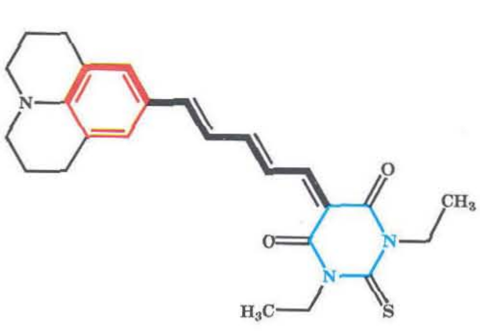


Figure of Merit = 7150 x 10^{-48} esu

Above: A plot of hyperpolarizability (β) versus dipole moment. In the generic molecules across the bottom, D stands for donor and A for acceptor. The "polyene" structure (left) is the neutral form. The "cyanine" structure (center) is the balance point. Here electron mobility is equal in either direction, as shown by the dashed lines, and the molecule's two ends have half a charge each. In the neutral form and the fully charge-separated form (right) the π electrons are pinned to the locations shown by the double bonds.

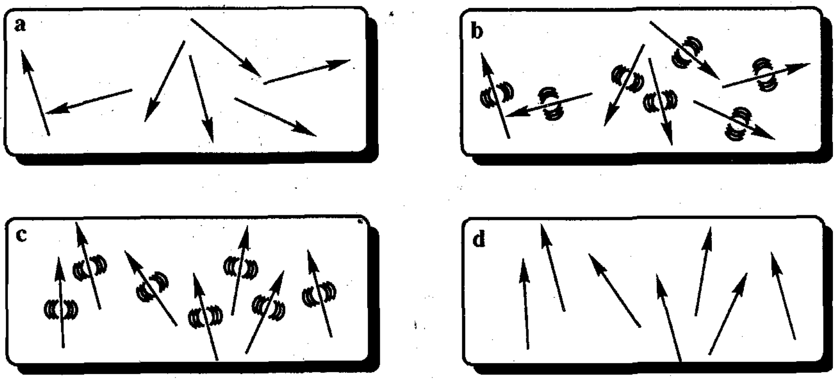
Right: The larger the figure of merit in esu, or electrostatic units, the more nonlinear the compound. Both compounds have 10-carbon bridges (bold lines), so the choice of donor and acceptor make all the difference. Top: DANS; bottom: a thiobarbituric acid derivative made in Marder's lab. Red rings lose aromaticity upon polarization. Blue rings gain aromaticity upon polarization.

acceptors you can possibly find; you want the *optimal* donor and acceptor."

The analysis showed that there's a balance between the electrons' lopsided reaction to the wave's alternating tugs and pushes that makes the molecule want to hyperpolarize, and the electron mobility that allows the hyperpolarization to occur. Plotting hyperpolarizability against the degree of charge separation, called the dipole moment, gives a double-humped curve. At the curve's very ends are molecules with very highly asymmetric responses, but whose π electrons are tied down. Molecules that are 100 percent in the neutral state lie at the curve's far left, and molecules that are fully charge-separated lie at the extreme right. In each case, there's only one direction the electron can be pushed—as skewed a response as you can get—but the π electrons are frozen in place because the opposite state does not contribute to the molecule. In the exact center lie molecules whose π electrons are free as the wind, but whose response is perfectly symmetric—the balance point. The molecule's hyperpolarizability is zero in all three instances. The curve's maximum occurs to the left of center. Here the response is asymmetric—you haven't yet gotten to the balance point, but you're far enough from the neutral state that the π electrons can move. On the curve's right-hand hump, which is actually a valley, the charge-separated form dominates. There's a similar hyperpolarizability, but with the opposite sign.

DANS, it turned out, was a bad choice in any case. Donor and acceptor groups aside, DANS suffered from having a bridge that actually impeded charge transfer. The bridge contains two benzene rings—carbon-atom hexagons containing six π electrons each. By a quirk of molecular orbital theory, rings containing six π electrons are especially stable, a beatified state known to chemists as "aromaticity." But in order for the charge separation to occur, each benzene ring would have to surrender some of its π electrons. The rings' reluctance to do so, and lose their aromaticity in the process, put the molecule on the very left-hand end of the curve, where the response was small.

Armed with this knowledge, Marder began synthesizing new molecules whose donors and acceptors had the right strength to put the molecule near the curve's peak. The bridges contained no aromatic rings, just chains of carbon atoms with alternating single and double bonds, and thus venturesome π electrons. In fact, the researchers were able to use aromaticity to their advantage. By choosing an acceptor group that contains a ring that becomes aromatic upon



**How poling works. (a.) The molecules and the polymer are dissolved in a solvent, which is then evaporated off. This leaves the molecules trapped in the polymer and oriented at random, as shown by the arrows representing their dipoles.
(b.) The polymer is heated above its glass transition temperature, allowing the molecules to jiggle about.
(c.) A strong electric field orients all the molecules in roughly the same direction.
(d.) The field is turned off after the polymer cools, freezing the molecules in their new orientation.**

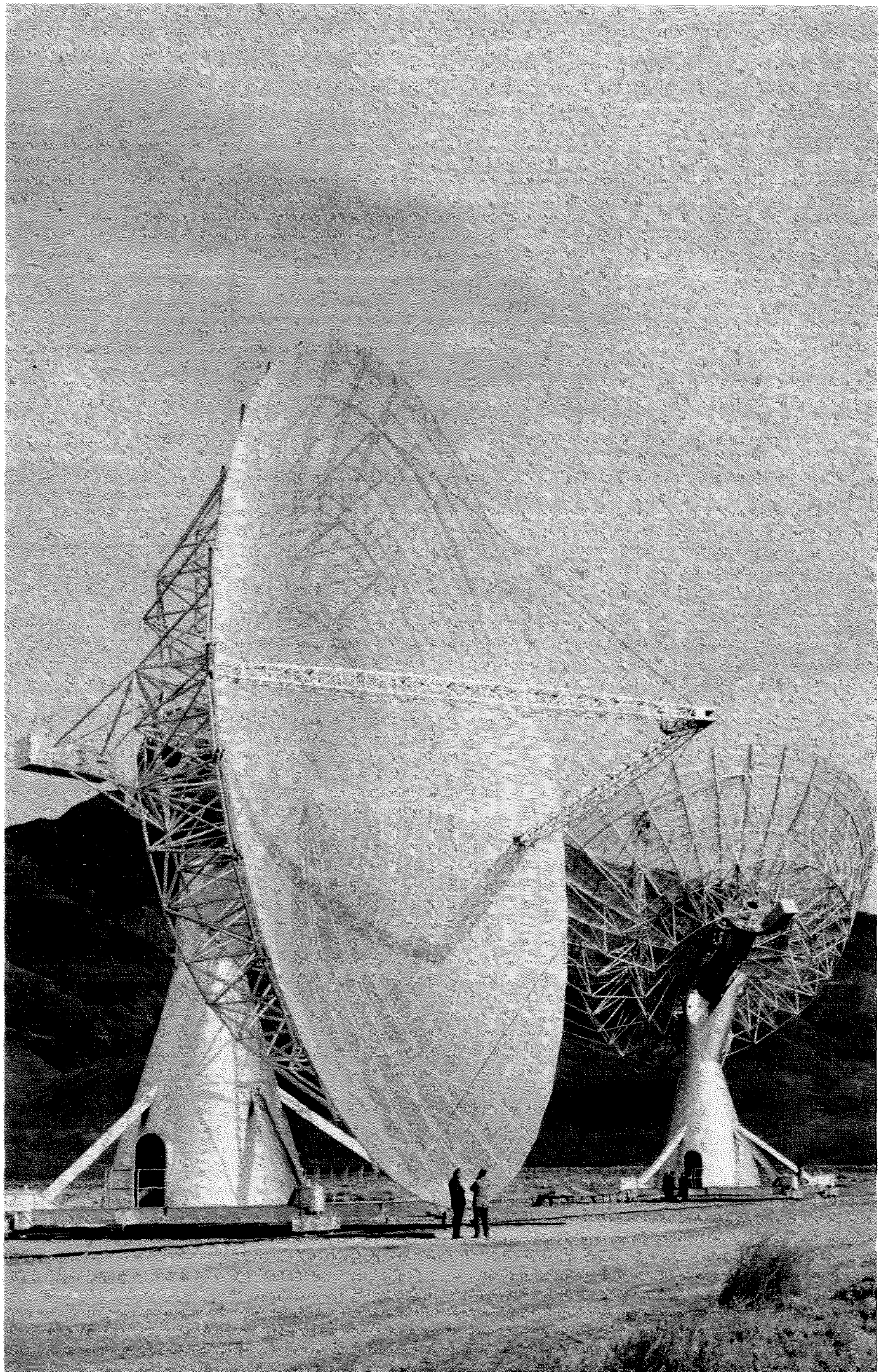
receipt of an electron, they were able to move the molecule farther to the right, toward the peak. "After we published that study, people jumped to the conclusion that aromaticity was bad," says Marder. "What we're really saying is that aromaticity is neither intrinsically bad nor good—it's just another way to position a molecule on the curve."

But getting the molecule right is only half the job. If the molecules assemble themselves into a crystal containing a center of symmetry, as Marder says some 75 percent of asymmetrical molecules are wont to do, then you're no better off than you were with a symmetrical molecule. It's the same old bugaboo—the asymmetric polarization response vanishes, and the bulk material will be inactive. But there are ways around this. One is a method called poling (which Marder and company did not invent). This method imprisons the active molecule in a polymer matrix. Then you heat the polymer until it goes rubbery, and apply a strong electric field. The active molecules' dipoles will line up more or less with the field. If you let the material cool back down while maintaining the field, the molecules will remain trapped in that alignment. The alignment isn't perfect, like a crystal's could be, and the molecules will slowly come unstuck again with time, but the technique is good enough for many applications.

Marder and Perry have made polymers using some of their molecules, and are measuring the polymers' "figure of merit"—a package of properties including the dielectric constant (which measures an individual molecule's polarizability), bulk susceptibility, thermal and chemical stability, and so forth, all of which have to be optimized in order to make real-world devices. The molecules they have made to date are far from optimal, but they still expect to beat lithium niobate's figure of merit for an electro-optic switch by a factor of four. This means that the polymer requires one-quarter as much power to operate and generates one fourth the waste heat to be dissipated, both important practical considerations. "They won't give you many watts on a spacecraft," says Perry. "Everything has to be lightweight and low-power. If a switch takes a watt to drive, you can't use it. But if it only takes 100 milliwatts, you might be able to. Technology like this lets us think about putting fiber-optic LANs [a type of communications network] on a spacecraft—you could run the whole switching network on one or two watts."

One of the first applications of these materials will probably be in sensors that react to changes in the voltage applied to them. "We could use these sensors to monitor, and perhaps eventually manage the national power grid," says Perry. "In areas of high demand, you want to equalize the load—the way the current is sucked through the lines. By evening out how the current is split, you minimize the power loss. Fully automating the distribution system might increase its efficiency by perhaps 0.1 percent, but that still translates into billions of dollars saved." The polymeric sensor would be applied to the lines at points near transformers and switching stations. "Like all technologies, we're chasing a moving target," says Marder. "Inorganic materials are constantly getting better, too. The goal is not to displace inorganics, but to complement them."

Perry agrees. "I think that we won't make a big dent in frequency conversion, but waveguides and polymer voltage sensors are where the cost benefits should allow these materials to find a real commercial life. Like plastics, they're really easy to process. You can mold them, you can spin them onto silicon wafers, and then you just mask, etch, and metallize them like any other circuit component." Waveguides are to light what wires and switches are to electricity, and could replace much of the soldered metal that shuttles information between the chip's microcircuits. The chip's logic elements would still be electronic, but they would communicate via tiny diode lasers and photodiodes. Data would travel faster and generate less heat. "This is the limiting step in chip design now—getting the waste heat off the chip. The more stuff you put on a chip and the faster it goes, the more heat you make." And with that, we're back to the information superhighway. □



The Owens Valley Radio Observatory: Early Years

The early radio astronomy students were an exceptional group. They lived for long periods at OVRO and participated in the exciting task of building a large, novel scientific instrument.

by Marshall H. Cohen

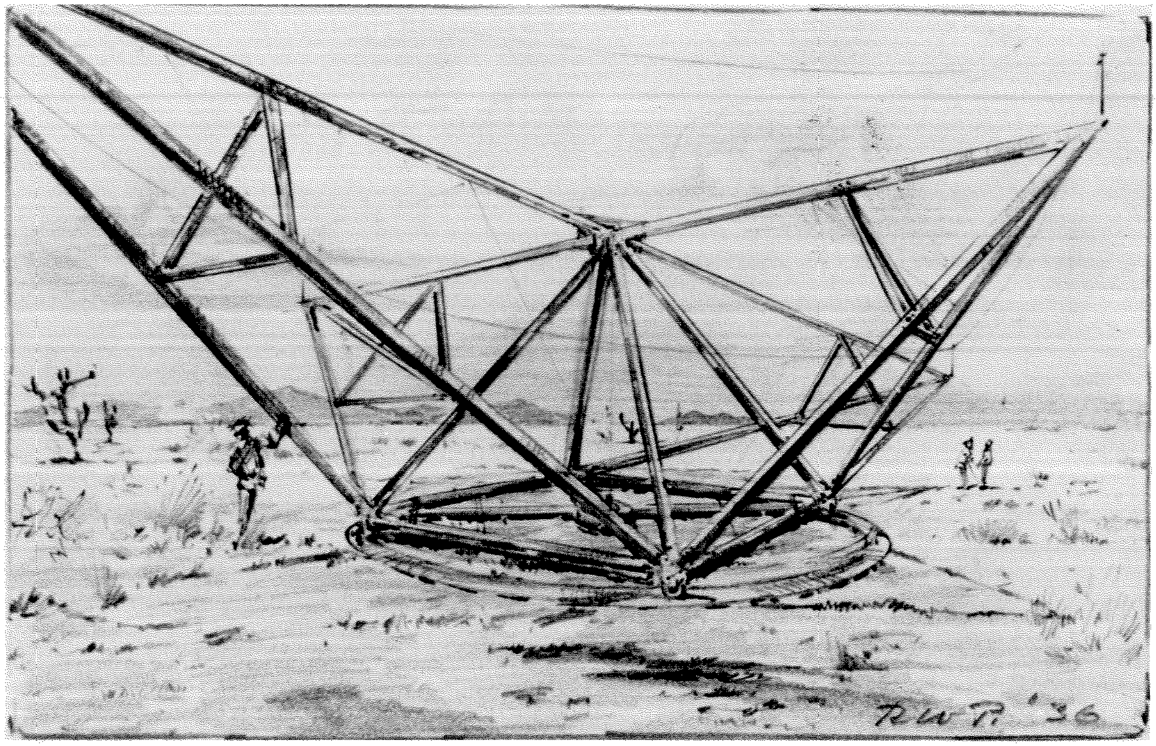
The two 90-foot antennas of the radio interferometer at Owens Valley, shown here after the completion of the dishes in 1959, formed one of the largest and most sensitive radio telescopes in the world. Each dish alone was larger than any in the United States at that time.

Heinrich Hertz demonstrated the existence of radio waves in 1888. Scientists immediately thought of looking at the sun for radio waves, but the early experiments, between 1894 and 1900, were hopelessly lost in noise. Thirty years later the sensitivity of radio receivers was enormously improved, mainly by the invention of vacuum-tube amplifiers, and cosmic radio waves were discovered in 1933. This discovery came by accident, and, moreover, the waves were not coming from the sun. As Karl Jansky at the Bell Labs was studying interference, he found a component of static that was fixed among the stars; that is, it came four minutes earlier on successive nights. Jansky recognized it as radiation from the Milky Way. During the thirties he and Grote Reber, a radio amateur from Illinois, mapped the source of this radiation, but for the most part it remained a minor curiosity.

Caltech, however, did have an abortive program in those early years of radio astronomy. Professor of Physics Gennady W. Potapenko, who was interested in Jansky's work, got his student Donald Folland (MS '36) to build a receiver to detect the cosmic radiation. In 1936 they tried observing with loop antennas on the roof of East Bridge, but it was too noisy. They then moved into the Mojave Desert, where they succeeded in confirming Jansky's results. Although their work was crude and never was published, it was promising and showed the need for a much larger antenna. They made a rough design for a rotating rhombic antenna, 90 by 180 feet, and persuaded R. W. Porter, the artist who made all the wonderful cutaway drawings of the 200-inch Palomar Telescope, to sketch it. Some question exists over how much money was needed to fund the project—Potapenko is quoted as asking for $1,000, but Fritz Zwicky, who also was involved, later said that it was only $200. In any event, Millikan refused to fund the project, and that was the end of radio astronomy at Caltech for 20 years.

During those years Jesse Greenstein, as a graduate student at Harvard, became interested in the problem of the origin of the cosmic radiation. He tried to understand Jansky's radiation in terms of thermal radiation from dust, and in 1937, with Fred Whipple, wrote the world's first scientific paper on interpretations of radio astronomical data. In that paper they showed that dust failed to produce the observed radiation by a factor of 10,000, but they had no alternatives. It was many years and many incorrect explanations later before anyone understood that the radiation actually was due to the synchrotron effect, from relativistic electrons gyrating in a magnetic field. Greenstein went on to play a major role in establishing the radio astronomy program at Caltech.

During World War II the sensitivity and reliability of radio systems again improved enormously, and after the war a number of radar engineers turned to the study of cosmic radio waves. The sun and the Milky Way came under immediate observation, but then many small sources also began to turn up. They were called "radio stars" but they clearly were not stars like the sun. In order to find and study these objects, radio astronomers in England and Australia built



In 1936 R. W. Porter sketched Potapenko and Folland's design for a rotating rhombic antenna, 90 by 180 feet, with which they wanted to detect cosmic radio waves. The project was never funded, and Caltech waited two decades before taking up radio astronomy again.

interferometers, antenna pairs that act together to give higher angular resolution than either antenna alone. In its simplest form, an interferometer comprises two antennas which behave exactly as a 2-slit interference experiment in elementary physics, producing fringes (a pattern of strong and weak bands), whose angular spacing is λ/s radians (λ is the wavelength, s is the linear spacing between the antennas). The spacing s can be large, so the angular resolution can be much greater than that available from one antenna alone, which is merely λ/d, where d is the diameter of the antenna.

In the early 1950s the most important interferometers or antenna arrays were at the Universities of Cambridge and Manchester in England, and at the Commonwealth Scientific and Industrial Research Organization (CSIRO, then called CSIR) in Australia. These groups produced lists of radio sources, but there were substantial disagreements that ultimately were traced to "confusion," that is, to artifacts produced when many weak sources are seen simultaneously. Not until the early sixties was the entire sky reliably surveyed. At that point several thousand radio sources were known, but most of them were "unidentified"; that is, they had no known optical counterpart. Identifying the sources—matching them up with something visible, such as a star or a galaxy—was laborious, and proceeded slowly. This work was important because the radio objects were very mysterious—their workings were unknown, and even their distances were unknown until the identifications were made.

Two of the main players in the identification work were John Bolton and Gordon Stanley of CSIR. In 1949 they made the first three optical identifications of compact, or discrete, radio sources: a galactic supernova remnant called the Crab Nebula, and two galaxies in Virgo and Centaurus—M 87 and NGC 5128. Interestingly, in their 1949 paper Bolton, Stanley, and Slee noted that although M 87 and NGC 5128 were generally called external galaxies, they had not yet been resolved into stars. If their proposed identifications were correct, their radio luminosities had to be unrealistically large, and they probably were nearby galactic nebulosities rather than distant galaxies composed of stars. This idea was wrong, but is only one of the many failures of the human imagination to grasp the size and complexity of the universe. Fifteen years later similar weak arguments were used to suggest that quasars were not at the enormous distances implied by their redshifts.

In 1951 Palomar Observatory got into the act when Graham Smith of Cambridge sent accurate positions for the strong radio sources in Cygnus and Cassiopeia to Walter Baade and Rudolph Minkowski, who were on the staff of the Mount Wilson Observatory (operated jointly with Palomar by Caltech and the Carnegie Institution of Washington). With the 200-inch telescope they found that Cassiopeia was a pale galactic nebulosity, the remnant of a supernova explosion, but the Cygnus source astounded them and everyone else, because the radio signals appeared to come from a very distant pair of galaxies in collision. (We know now that it is only one galaxy, but it has a dust lane which makes it look double.) The

The Observatory Committee . . . decreed that radio observations were outside the charter for Palomar Observatory, which spoke of using "light" to study the universe.

source had to have a million times more radio luminosity than is produced in the Milky Way to be so bright from such a distance, so this was indeed a remarkable object. Furthermore, astronomers then realized that some of the many weaker discrete sources should be even more distant, and radio galaxies might provide a powerful tool for probing the distant universe and studying cosmology. The potential of radio astronomy for exciting extragalactic research now appeared very high.

Jesse Greenstein, here in 1957 with a more famous Porter drawing, was one of the prime movers behind the establishment of radio astronomy at Caltech.

Radio Astronomy at Caltech

In the late 1940s three people with an interest in radio astronomy arrived at Caltech: Lee DuBridge, the new president; Robert Bacher, professor of physics and chairman of the Division of Physics, Mathematics and Astronomy; and Jesse Greenstein, professor of astrophysics and founder of the astronomy department. Greenstein had continued the interest in radio astronomy that originated at Harvard, and had tried, but failed, to establish a radio astronomy program at the University of Chicago, when he went to Chicago's Yerkes Observatory in Williams Bay, Wisconsin, after graduation. DuBridge had been director of the Radiation Laboratory at MIT during the war, and Bacher had worked there for two years before moving to Los Alamos; they were familiar with the wartime developments in radio astronomy. An important connection for both DuBridge and Bacher was the Australian Edward G. "Taffy" Bowen, who had worked in British radar since 1935 and had been a frequent visitor to the Rad Lab. After the war Bowen became chief of the radiophysics division at CSIRO in Sydney, where so much of the postwar development of radio astronomy occurred.

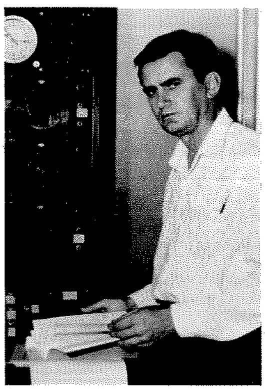
At first, most of the conventional astronomers at Caltech (and elsewhere) regarded radio astronomy as unimportant and even uninteresting, but that view changed when the exotic nature of the radio sources became appreciated. Around that time it also was generally recognized that the United States, where so much radar development had gone on, was rapidly falling behind Australia and Europe in this new field. Bowen and senior American scientists he knew from his radar years, including DuBridge and Bacher, met over this problem on a number of occasions. They considered the possibilities for large projects in both the U.S. and Australia, and conceived of the U.S. project as being at Caltech. In 1952 Bowen even wrote a "Draft Programme for a Radio Observatory" in which he stated that the next great advances in radio astronomy would come from associating radio and optical measurements, and that a very large dish, 200 to 250 feet in diameter, would be needed.

Meanwhile, Greenstein, along with the Mount Wilson astronomers Baade and Minkowski, was lobbying the Caltech administration to set up a radio astronomy program. DuBridge, however, had what in current officialese is called "programmatic concerns." Astronomy was organized under the joint Palomar and Mount Wilson Observatories, but radio astronomy was foreign to the astronomers—the techniques and style were completely different. The Observatory Committee, which advised the director, Ira "Ike" Bowen, on important matters, decreed that radio observations were outside the charter for Palomar Observatory, which spoke of using "light" to study the universe. Radio astronomy would have to be a separate operation in the Division of Physics, Mathematics and Astronomy.

In 1953 Greenstein organized a conference on radio astronomy to demonstrate the vitality and prospects of the field, and to prod the administration. It was held at the Carnegie Institution of Washington in January 1954. All the major radio astronomers of the world were there, and DuBridge underscored the importance of radio astronomy to Caltech by attending, along with Greenstein and Minkowski. This meeting was a great success, for not only did it impress DuBridge and thus set the stage for the Caltech project, but it also catalyzed the founding of the National Radio Astronomy Observatory (NRAO).

In 1954 it still was necessary to look to

Right: John Bolton with a model of the 90-foot dish. Below: Gordon Stanley, the receiver expert, another member of the Australian team imported for OVRO.



Australia or Europe for someone with experience in large radio astronomy projects, and DuBridge naturally consulted Taffy Bowen. (The Old-Boy Network was in full flower.) Bowen recommended the Englishman John Bolton, a prominent radio astronomer at CSIRO. Bolton had graduated in 1942 from the University of Cambridge with a physics honors degree, and then had immediately gone into military radar work. Settling in Australia after the war, he joined the CSIRO radiophysics group, a team that included a number of very talented but also very determined people. Bolton had differences with them, especially group leader Joe Pawsey, which led to his leaving radio astronomy temporarily in 1952 and joining the cloud physics group at CSIRO—with a promise from Bowen of a major role in the large telescope planned for Australia.

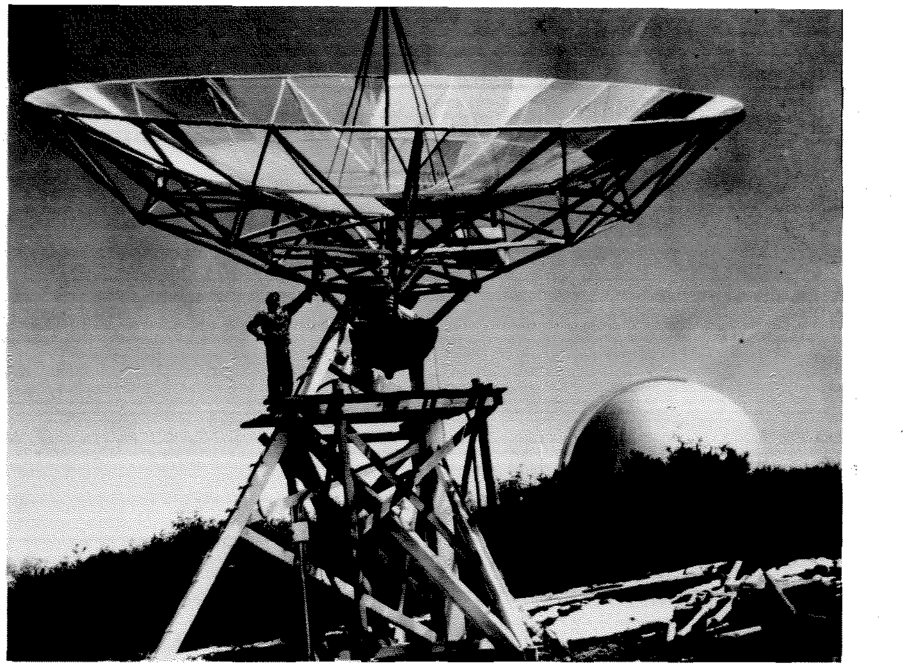
The Australian project started in early 1954, when the Carnegie Corporation, using funds restricted to projects in the British Commonwealth, granted $250,000 to CSIRO for the construction of a large dish. This was followed by a similar grant from the Rockefeller Foundation and these grants led to what became the 210-foot telescope at Parkes, in the countryside 200 miles west of Sydney.

So Caltech's timing in mid–1954 was right. The Institute wanted to start a program, and the talented and ambitious Bolton was available. A three-way discussion evidently took place, and Bowen later wrote, ". . . I arranged for Bolton and Stanley to be seconded to Caltech. This was to prove the starting point for radio astronomy in California." DuBridge offered Bolton a job (a two-year term appointment as senior research fellow in physics and astronomy, with a commitment to discuss a long-term association at the end of the term), and Bowen urged him to take it, adding (according to Bolton) that he could come back and run the new Australian dish when it was built. Bolton accepted the offer, and in January 1955 arrived in Pasadena with his wife, Letty, and their two sons. He was 32 years old, and famous for his work on discrete radio sources. Two years later he was promoted to professor of radio astronomy, and six years after he arrived, after establishing the Owens Valley Radio Observatory (OVRO), he did return to Australia, to the regret of the Caltech community. It is not clear how much the Caltech astronomers knew of the deal that had been devised to attract Bolton, but Greenstein and others were surprised and disappointed when he left.

Establishing OVRO

Bolton's war service and his work at CSIRO gave him an exceptionally wide background, and he was a talented jack-of-all-trades. But he needed help for the Caltech undertaking, and he brought along Gordon Stanley, who arrived a few months later. Stanley, a radio astronomer and receiver expert, had worked with Bolton at CSIRO, and was part of the team that made the first identifications of discrete radio sources. By the time Stanley arrived, Bolton had already decided to build a two-element, variable-spacing interferometer. This was not a new idea; Bolton and Stanley had considered a similar system a few years earlier, but it never got beyond the planning stage at CSIRO. For identification work the important goal was precise positions, and the high frequency and versatility provided by an interferometer were vital. DuBridge had already lined up support from the Office of Naval Research (ONR), and the project got under way immediately. (Getting new projects started was far, far easier in those days than it is today.)

Design and construction of the interferometer would take a few years, and Bolton and Stanley decided to use that time to build a modest prototype antenna. Its main purpose would be to test the advanced concepts they planned for the main instrument. Ike Bowen agreed to let them build it on Palomar Mountain, and so, in 1956 Caltech got its first radio telescope, a 32-foot dish outfitted with a 21-cm receiver. Interference was bad, but scientific observations were carried out anyway while receiving systems were tested. The study of hydrogen clouds by their radiation at a



Right: Bolton shows off Caltech's first radio telescope (here still unfinished)—a 32-foot antenna constructed on Palomar Mountain in 1956. In 1958 it was moved to Owens Valley (left) and re-erected with a solid fiberglass surface. A corner of the low-frequency array, the second instrument completed at OVRO, is visible at left.

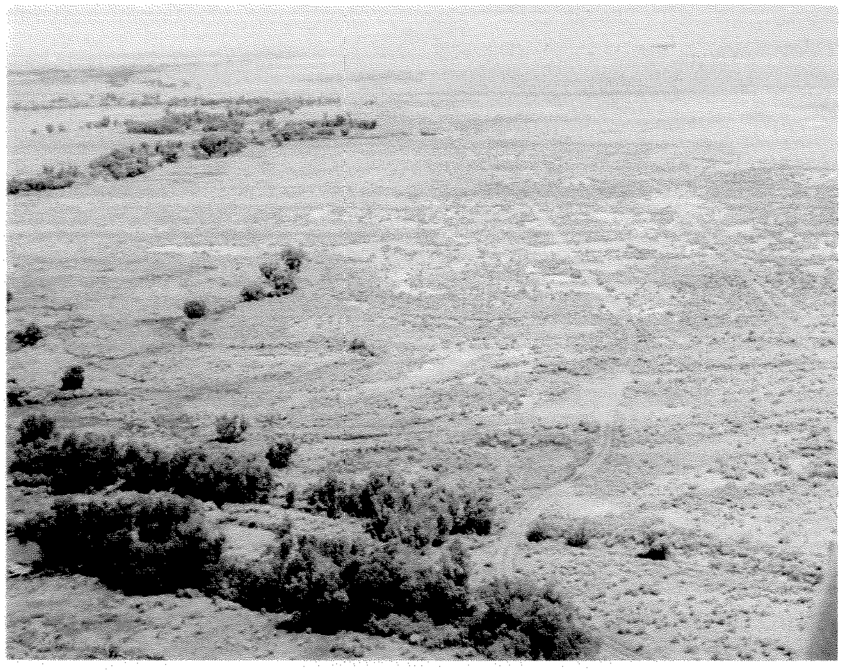
wavelength of 21 centimeters was still in its youth, and from Palomar the southern Milky Way was available for exploration. The first paper from the Caltech radio astronomy group, "A 21-cm Line Survey for Galactic Longitudes 294° to 328°, Latitudes ±8°," by Bolton, Stanley, and Harris, appeared in the *Publications of the Astronomical Society of the Pacific* in December 1958. The third author, Dan Harris (MS '57, PhD '61), was the first Caltech graduate student in radio astronomy. He arrived in June 1956 and spent much of the summer observing at Palomar.

In those days data reductions were often done by an assistant (called a "computer") using a mechanical calculator. Mildred Matthews, daughter of the famous Harvard astronomer Harlow Shapley, was a computer for Jesse Greenstein, and her daughter June, who had just graduated from high school, similarly helped the radio astronomy group by working on the data from the 32-foot telescope. June Matthews is now professor of physics at MIT.

All that remains of it now are some fragments at OVRO and pieces of concrete in the ground at Palomar, across the road from the museum.

The 32-foot telescope (shown above) was primitive by modern standards. It had an hour-angle drive controlled by the difference between two gear trains, one driven by a sidereal clock and the other set to the desired right ascension. The declination was set by moving the antenna with a large wrench. (Right ascension and declination are the same as longitude and latitude, only with respect to the stars rather than to the earth. As the earth turns, a star at a fixed right ascension appears to move across the sky, and the antenna must track its motion. The sky "rotates" once per sidereal day, which is 23 hours and 56 minutes: the missing 4 minutes is made up by the motion of the earth around the sun.) The receiver was of the single-channel, double-comparison variety (two comparison bands), with a bandwidth of 25 kc/s, and a receiver noise figure of 3. This was good for its time, even if recent generations of students will not know what these units mean. The telescope was dismantled in 1958 and installed at OVRO with a better surface. The astronomers intended to connect it as part of the interferometer, but it was never used that way. All that remains of it now are some fragments at OVRO and pieces of concrete in the ground at Palomar, across the road from the museum.

The site-selection process for the main observatory began soon after Bolton arrived. The chief requirements were for a large flat area and low radio interference. The latter meant that it had to be rather isolated, since the radio-noise level is roughly proportional to the local population density; it comes from noisy motors (or anything that sparks) and from communication devices of all kinds; taxis, police cars, and airplanes are a particular nuisance, as well as fixed strong sources such as radio stations. Nowadays the interference is worse, because of cellular telephones and other modern devices, and it is impossible to hide from satellites; but even in the mid-fifties the San Gabriel Valley was far too noisy for radio astronomy. (Indeed, it had been too noisy for Potapenko in 1936.) The Palomar area was desirable but also was too noisy, and the best nearby location had the ominous name of Earthquake Valley. So the search turned to the valleys to the north.



The Owens Valley site of the radio observatory (shown here "before") was, and still is, a ranching area. Before the DWP discovered it, there was extensive farming here also. The tree-lined Owens River, which winds around to the left, can be seen along the lower edge of the "after" photo on page 21.

By the time Stanley arrived, Bolton had already selected a location near Ojai, but that turned out to be a military site and unavailable. They then looked into more remote areas and in the summer of 1955, while Bolton was off at the general assembly of the International Astronomical Union in Dublin, Stanley and Temple Larrabee began investigating Owens Valley and other desert areas. Larrabee, a Caltech mechanical engineer, had helped build the 32-foot telescope and supervised much of the early construction at OVRO, especially ground clearance and the erection of buildings.

Stanley and Larrabee went through Owens Valley to the Mammoth Lakes area. They also tested the Saline Valley, which was very quiet but too remote; the nearest towns were 50 miles away on a bad road across the Inyo Mountains. Owens Valley was more civilized. The southern part was too close to military bases, but the northern part of the valley formed a good compromise between interference and accessibility. The Sierra Nevada shielded it from Fresno and other cities to the west and south; Los Angeles was 250 miles away, beyond the mountains; and, best of all, the Los Angeles Department of Water and Power (DWP) owned most of the land up and down the valley and was unlikely to allow much development to occur. The astronomers finally selected a site a few miles north of the Zurich railroad station, on the east side of the valley, five miles north of Big Pine. The east side was chosen because every 20 years or so the valley experiences exceedingly high winds, which decrease in strength from west to east.

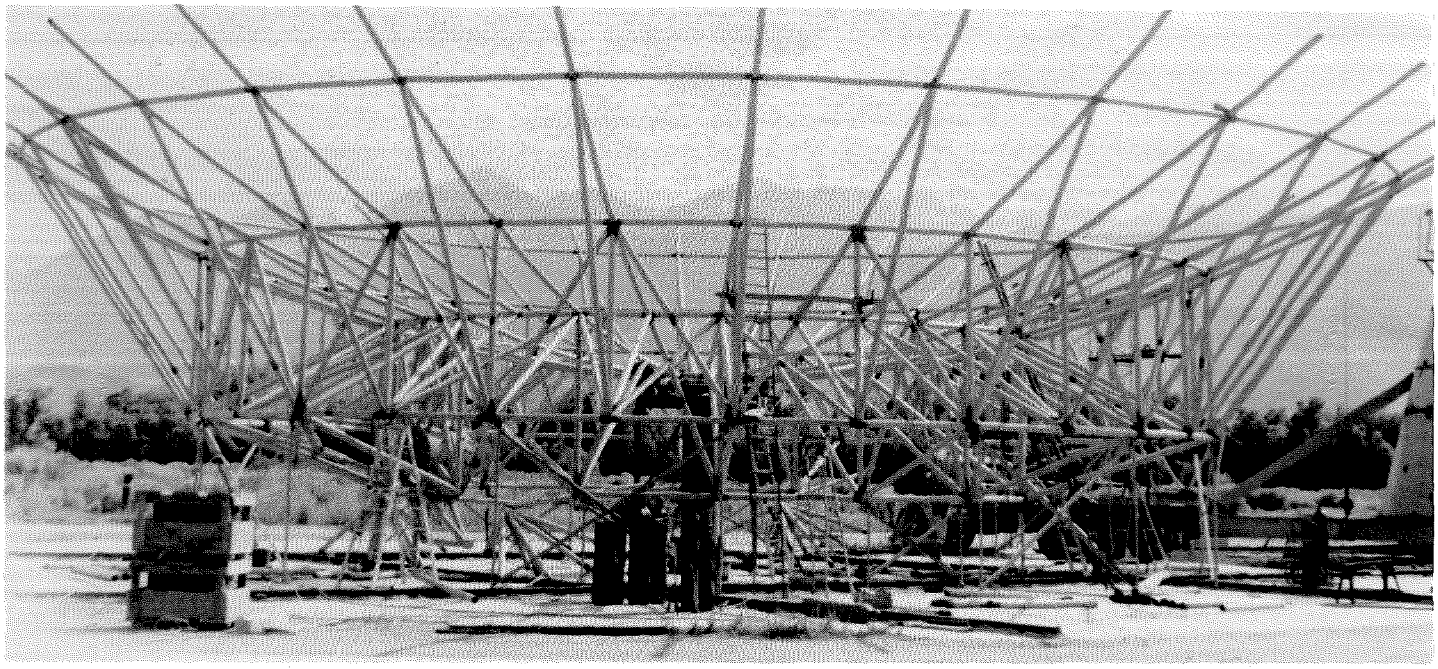
In 1956 Caltech arranged a 300-acre lease with the DWP. The lease was renegotiated in 1965, and an extensive area was added in anticipation of a large array that was never built. In 1988 a sub-lease was arranged with the NRAO, which then built an antenna for the Very Long Baseline Array.

The DWP is the biggest employer in Owens Valley, but there still is contention between the valley's citizens and the city of Los Angeles. Particular problems include the low water level in Mono Lake, the trout population in Rush Creek, groundwater pumping in the Owens Valley, and especially the dry condition of Owens Lake, which ranks as the worst dust pollution source in the United States. (On bad days alkaline dust from Owens Lake falls on the city of San Bernardino.) The current chief of the Great Basin Unified Air Pollution Control District (Inyo, Mono, and Alpine counties) is Ellen Hardebeck, who came to Caltech in 1969 as a research fellow in radio astronomy, and moved to Bishop in 1972, when her husband, Harry (another radio astronomer), accepted a job as an engineer at OVRO. The population ratio between Los Angeles and the Owens Valley is rather extreme, but Ellen is aided by the Clean Air Act, and manages to win many points against the city.

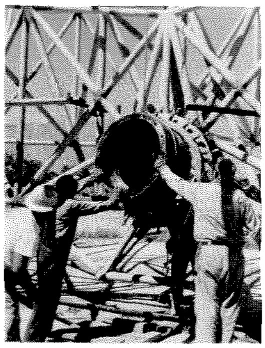
Control of Owens Valley water and land by the DWP may make life difficult for people who live close to Owens Lake, but it has kept development away, and the observatory remains one of the quieter sites still being used for radio astronomy. The contrast could not be stronger than between the regions around OVRO and the Palomar Observatory. A half million people now live in the valleys east and north of Temecula, and their communities creep to within 15 miles of Palomar Mountain, but the Owens Valley has changed only slightly in the 39 years since the observatory was founded.

Building the Interferometer

Bolton's goal was to make a two-antenna interferometer to be used mainly for determining the precise positions of radio sources. The antennas would be 90 feet in diameter, a size representing a compromise between cost and sensitivity. The antennas, like the 32-foot prototype, were polar-mounted; the modern efficient design with azimuth and elevation axes was awkward before computers became available. The mount had an axis parallel to the earth's pole (the polar axis) and another at right angles (the declination axis). The polar motion was limited to ±4 hours, or ±60°. This feature was economi-



Above: The skeleton of one of the 90-foot dishes rises against the Sierra Nevada, which borders the valley on the west. Below: Workers position the hub at the center of the dish.

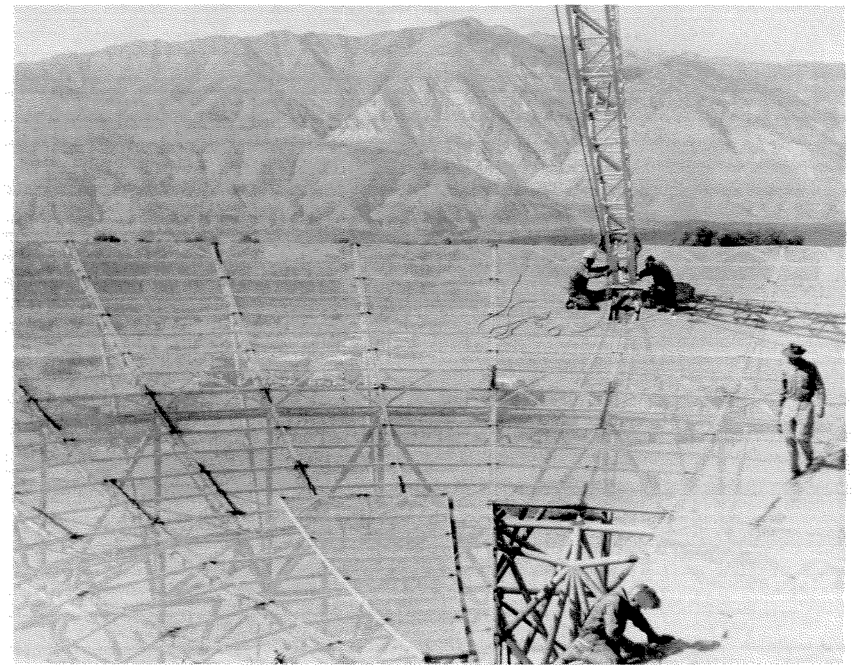


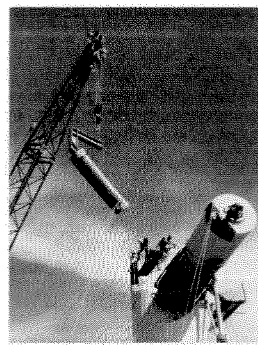
cal because the axes did not have to be cantilevered; thus there were no off-axis wind loads, and less stress to be resisted. This was a compromise between cost and versatility, and has sometimes been a liability; for example, it currently limits observations of the sun to eight hours per day. A more dramatic design feature was the railroad track: the antennas were movable on tracks extending in an L-shape 1,600 feet east and north from a central station. At a wavelength of 75 cm this would give a minimum fringe spacing of 5 arcminutes, and on a strong point source the positional accuracy would be a few seconds of arc. This was adequate for the planned optical identification work.

The objective, of course, was more general than simply pinpointing the sources and associating them with optical objects. It also was important to find their sizes and spectra so that the physics of the radio sources could be studied. This required a range of angular resolutions, which is why the antennas had to have variable spacing. The idea is that for each spacing between the antennas there is a characteristic angular scale, and measuring the interferometer output (the "fringe" amplitude) gives information for that particular scale. By measuring at many spacings, the full information on the source can be developed. (Technically speaking, the fringes are sine waves which modulate the brightness distribution of the source, and a measurement of the complex fringe amplitude is an integration over this modulation, which yields one component of the Fourier transform of the source. After measurements are made at all the spacings, the inverse transform is calculated; this is the desired image.) The antennas had to be exceptionally stable when in use, and were set at stations on the tracks, which had electrical connections and also provided precise support and positioning. There was a station at the center of the L, and stations were set at 200, 400, 800, and 1,600 feet both east and north of the center. This gave many differential spacings, but most could not be used because the outlying stations were connected only to the center and not to each other. In later years more stations were added, and the track was extended to 600 feet west of the center.

The original intention was to operate the interferometer at 400 MHz, with a buried waveguide connecting the dishes. During the design and construction years, however, higher frequencies became both desirable and convenient, and the first system installed was at 750 MHz, with a buried cable rather than a waveguide. That system in turn was quickly replaced with 960 MHz, which was in a protected space-communication band. The JPL systems on the Explorer satellites operated at that frequency, and they used receiver front-ends designed by Stanley at OVRO. This was one of the first of many mutually beneficial close collaborations between JPL and the Caltech radio astronomy group.

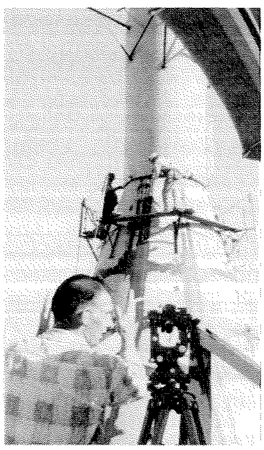
Bruce Rule, Caltech's chief engineer, who had already distinguished himself in telescope design through his major role in the 200-inch Palomar project, designed the antennas, with the substantial assistance of Charlie Jones (BS '32) , who ran a Pasadena engineering firm. The reflector was made of steel mesh with 1-cm holes—sufficiently





The reflectors (far left) were made of steel mesh with 1-cm holes. The 45-foot pedestals, or towers (below), were constructed separately and the dishes hoisted onto them later.

Right: It took three cranes, manned by a rigging crew of 12, to lift one of the huge dishes onto its pedestal. Securing it in position involved meshing the pedestal gears with the driving gears on the dish, and welding two counterweights of 18 tons each onto the dish before the cranes could release their grip.



Bolton, with a theodolite, checks the installation of the top section of the tower for one of the antennas in 1958. Bolton spent a lot of time at OVRO, watching the details of construction and doing some of it himself.

smaller than the wavelength, first 75, then 40, then 31 cm. Still shorter wavelengths were steadily introduced, however, and today the short-wavelength limit of the antennas is 2 cm. To keep up with the shorter wavelengths, the surface was substantially improved in 1964. A quadrupod replaced the original feed support bipod, the backup structure was strengthened, and the reflector's mesh changed to solid aluminum (perforated near the rim).

Stanley supervised the electronics for the interferometer, and he designed and built the receivers. At the beginning these had crystal mixer front ends, using the wartime-developed 1N21 crystals. The local oscillator used a planar triode that had been developed for use in rockets. For stability and tuning this was secured inside a rugged brass cylinder with various protruding rods; it looked like a 19th-century steam engine and so, of course, was always known as a Stanley Steamer. These receivers were probably the best in the world at the time, with system temperatures in the range 300–400 K. They were retired in the 1960s as parametric amplifiers became available, and yet later the paramps were mercifully replaced with transistor amplifiers, which had lower noise, more stability, more bandwidth, and also were cheaper and much easier to work with. Maser amplifiers also have been used; although tricky and expensive, they give the lowest noise.

Charlie Jones oversaw construction at OVRO, which took place between 1956 and 1960. A Los Angeles contractor prepared the site and built the east arm. The money for the north arm, however, began to disappear too soon, and Bolton himself became the site contractor. He involved himself with every detail of the construction and, indeed, had immersed himself in nearly every facet of the project from its beginning.

The total cost of preparing the site, including roads, buildings, utilities, and the tracks and antenna caissons for the east arm, amounted to somewhat over $100,000. This money came from the Institute and was the first of many generous contributions that Caltech has made to radio astronomy. ONR provided the rest of the construction cost, and the operating costs. The capital construction costs through 1960 were about $900,000, while the yearly operating costs increased from about $90,000 in 1956 to $136,000 in 1961.

After the war the ONR was an important source of funding for basic scientific research in the U.S. and helped many programs get started. This continued even after the National Science Foundation (NSF) was founded in 1950, and in the late sixties both NSF and ONR supported OVRO. Congress, however, grew unhappy with what it saw as misspent defense funds, and in 1970 passed the Mansfield Amendment, which restricted Defense Department funds to projects with military applications. At that point ONR had to drop OVRO. The NSF picked up the slack and since then has provided most of the support.

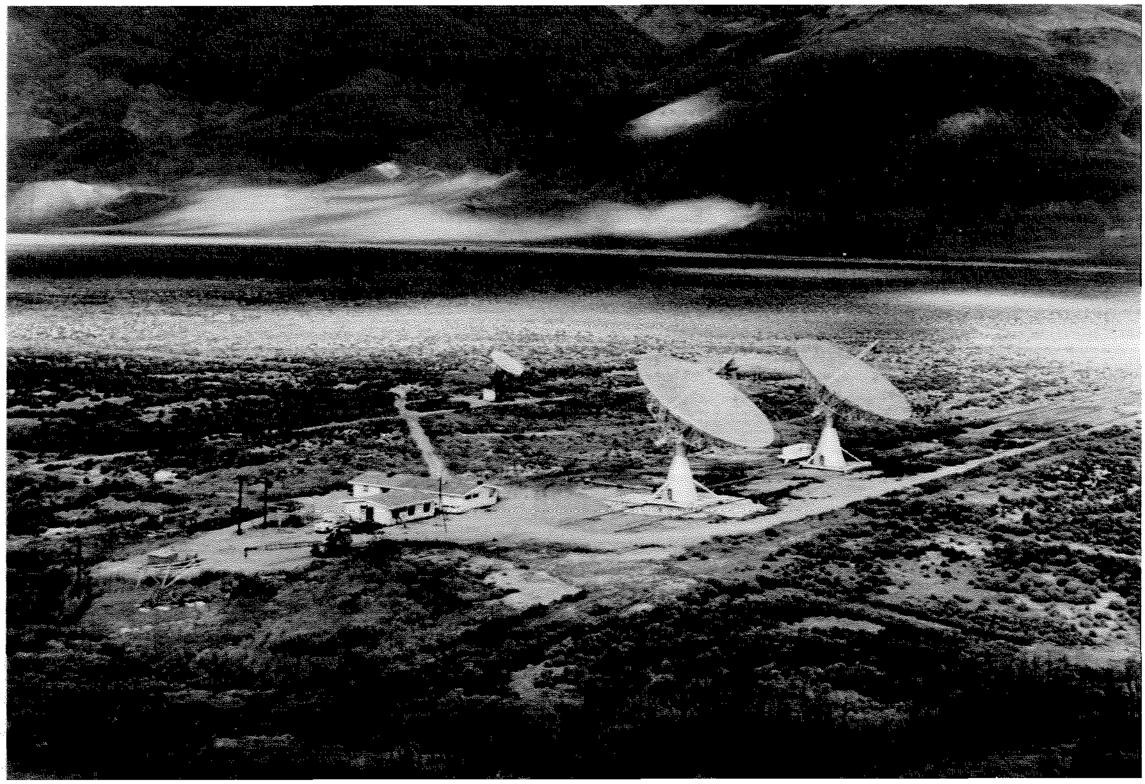
Arnold Shostak was the genial program manager at ONR, and for many years he helped promote radio astronomy throughout the United States. Surplus military equipment was available

We can only speculate as to how such elderly German rails ended up at a naval station in Southern California.

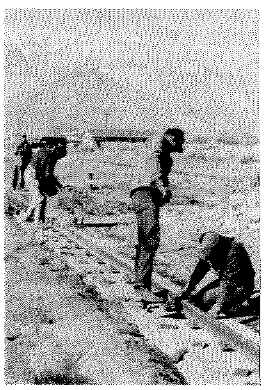
to those with the right connections, and Shostak scrounged the world over for OVRO. Most important, he found lead weights from the submarine base at Subic Bay in the Philippines and railroad tracks from the Navy depot at Port Hueneme. Most of the rails were stamped "Krupp 1880"; we can only speculate as to how such elderly German rails ended up at a naval station in Southern California.

The interferometer was dedicated in December 1958, although the north arm had not yet been built. DuBridge attended, as well as Albert B. Ruddock, chairman of the Caltech Board of Trustees. Rear Admiral Rawson Bennett II, chief of naval research, represented the ONR. The second reflector had been hoisted up on its pedestal a few weeks earlier, so that, superficially, the system looked complete, but the distinguished group of visitors probably was not fooled. As always at such affairs, the system was a long way from being operational.

Observations with one antenna started in April 1959, and the first fringes with the complete east-west interferometer were obtained at the end of the year. Bolton and research fellow Dave Morris completed the final linking of the two dishes at Christmas, and they endured an agonizing four days during which they tore apart and rebuilt the whole system; every component worked correctly but no fringes showed up. Finally, they discovered that the IF cables, still on their drums as provided by the manufacturer, were not identical but differed in length by 500 feet. Once found, that problem was readily cured, and the interferometer worked as expected.



Right: OVRO from the air before the north arm was begun, probably in 1959. The living quarters and the shop are visible in the foreground and the 32-foot dish just above it. Between the 90-foot dishes, behind them, the low-frequency array of dipoles is also faintly visible. Below: Workers construct the tracks of the east arm in 1957.



The North Arm

The observatory staff itself, in particular the graduate students, built most of the north arm of the interferometer in 1960. Dick Read (BS '55, PhD '62), who still works with the interferometer as a senior research engineer in solar physics at Caltech, did the survey work. Glenn Berge (MS '62, PhD '65), currently a senior scientist in planetary sciences, did much of the welding of the reinforcing rods in the sleepers under the tracks. A local construction company poured the cement for the sleepers; one of its employees, then-18-year-old Chick Lackore later got a permanent job at OVRO and still works there, the only current OVRO employee involved in the original construction. Bolton himself and "Big Al" Munger also helped with the welding. Big Al worked at OVRO for 10 years as a general technician and handyman.

He and Ken Kellermann (PhD '63) strung many miles of wire for the north arm; Barry Clark (BS '59, PhD '64), with Bolton and George Seielstad (PhD '63), did the same for the east arm. Clark had worked summers at OVRO since 1957, helping with the construction of the low-frequency array and the installation of the 32-foot dish. Clark and Kellermann joined the National Radio Astronomy Observatory after graduation, the latter after a stint as postdoc at CSIRO, and they, along with later graduates, played major roles in the development of the various arrays at the NRAO. Kellermann has returned to Caltech twice as a Fairchild Scholar, in 1981 and again in 1992–93.

Seielstad also operated the crane and moved the rails about. He went to the University of Alaska when he graduated, but after a year returned to OVRO, where he remained for many years. In 1984 he left for NRAO to be the assistant director for Greenbank operations. He is currently an associate dean at the University of North Dakota. Another early graduate student was Alan Moffet (PhD '61), "Little Al," who as a graduate student first worked in nuclear physics, but joined the OVRO team in 1959. After a year in Germany as a Fulbright Scholar, he returned to Caltech as a postdoc and ultimately became radio astronomy professor and director of OVRO.

The most senior student, Dan Harris, had already done a good share of work on the 32-foot antenna, but still had to help on the north arm. Harris worked at a number of radio observatories after graduation (in Argentina, Italy, the Netherlands, Puerto Rico, and Canada), and currently works at the Harvard-Smithsonian Center for Astrophysics. Another early student was Fritz Bartlett (MS '61), whose chief interest lay in computers and programming; he introduced modern computing into the observatory. Bartlett never finished his doctorate, and after a number of years he left OVRO and worked as a programmer in high-energy physics at Caltech.

Several of the students worked with Stanley on the electronics, in particular Moffet, Read, and Bob Wilson (PhD '62). Wilson joined Bell Labs after graduating, and a few years later, in a painstaking search with coworker Arno Penzias for spurious noise sources in their receiving system, discovered the cosmic background radiation left

Bolton started what became a vital tradition of students spending long periods at the observatory, first building and then using the telescopes.

over from the Big Bang. There are many parallels between this discovery and that of Jansky 32 years earlier. Jansky did not live to get the Nobel prize, but Wilson and Penzias did, in 1978.

Work on the north arm stretched through the hot summer of 1960. Living quarters at the observatory were very crowded, and Bolton established a strict set of rules and duties, sorted by seniority. In particular, first- and second-year students were warned about neatness: "All clothing, shoes and other personal effects are to be stored in drawers or closets and not left lying around." Third- and fourth-year students were merely enjoined to keep their rooms as tidy as possible. The cook and housekeeper was a warm-hearted, if formidable, woman named Rachel Gates (now deceased), and she ran the kitchen with an iron hand. She prepared three meals a day: lunch, afternoon tea, and dinner. The custom of morning coffee and afternoon tea is still rigidly observed at OVRO.

The early radio astronomy students were an exceptional group. They lived for long periods at OVRO and participated in the exciting task of building a large, novel scientific instrument. They worked closely with Bolton and Stanley and with a talented group of research fellows, including Jim Roberts and Kevin Westfold (actually, a visiting associate professor) from Australia, Dave Morris from England, Tom Matthews from Canada, Per Maltby from Norway, and V. Radhakrishnan (Rad) from India, who returned to Caltech in 1980–81 as a Fairchild Scholar. Some of them were fairly adventurous. Rad bought a trimaran and, with Morris and Harris as crew, sailed from England to Australia in 1962–63. On the way they called at Puerto Rico, where Harris left the boat and took up a radio astronomy position working with me at the Arecibo Observatory.

Bolton started what became a vital tradition of students spending long periods at the observatory, first building and then using the telescopes. They also had to work in the electronics lab in Robinson. Under the tutelage of technician Johnny Harriman, each new student had to build a power supply for the interferometer, and some of them did a great deal of electronics work. Caltech students have always been thoroughly trained in the tricky art of interferometry. They have provided much of the expertise needed to build interferometers at NRAO and at other institutions. Caltech still provides students with hands-on experience at building telescopes and their instrumentation, but there are only a few institutions left in the world where such opportunities now exist. It is much more difficult to provide this experience to graduate students now than it was 30 years ago, because radio observatories everywhere have become more expensive, more formal, and more automated. Most forefront research now requires large and expensive telescopes, and most of these are run by professional staffs. Graduate students (and professors) are welcome as users but participate little as builders, and this split between "user-astronomers" and "builders" becomes ever wider. At Caltech we always have regarded this as unhealthy and have worked to maintain an environment where students use their hands for more than typing on a keyboard.

The Early Science Program

The first instrument working at OVRO, apart from the 32-foot dish, was a low-frequency interferometer consisting of two large arrays of dipoles. In the summer of 1957 this device monitored the Crab Nebula as it passed near the sun. Two years earlier radio astronomers had discovered that the flux from the Crab decreased during this period, and suggested that it was due to scattering in the solar corona. The OVRO work aimed to extend this study by measuring at a new wavelength, 12 meters. In 1957 solar interference in fact was too strong for the OVRO group to make the measurement, but they achieved success in 1958. This generated the first paper in the "yellow jacket" preprint series: "A Solar Occultation of the Crab Nebula at a Wavelength of 12 Meters," by Bolton, Stanley, and Clark, published in December 1958. (The yellow jackets have yellow covers, of course, but the name derives from the nasty ground wasps that seriously annoyed the early workers at OVRO.) The arrays really were built, however, to study the fluctuations imposed on a radio signal as it passed through the earth's ionosphere (ionospheric scintillations), a phenomenon that also interested the Thompson Ramo Wooldridge Corporation (TRW). Some of the data given to TRW were classified because they were useful in tracking ICBMs, or so TRW thought. These low-frequency arrays were used until early 1959, when the first 90-footer began to work, and they were dismantled a few years later.

When the first antenna came on line early in 1959, observations started immediately, even as the second antenna was being finished and the interferometer electronics perfected. The scientific staff included six students and four research fellows in addition to Bolton and Stanley; within a year these numbers rose to nine and six, respectively. As design and construction were a group

Observing in the 1950s, without a computer, was real observing, with continuous manual setting of selsyn dials, interpolations in calibration curves, filling the pens of ink recorders, adjusting the voltages, and occasionally chasing away a cow.

effort, so were scientific observations. Different observing projects were defined and became the property of a small group—one or two people—but all observed for each other as they worked their shifts. Many of the students worked with a postdoc. Observing in the 1950s, without a computer, was *real* observing, with continuous manual setting of selsyn dials, interpolations in calibration curves, filling the pens of ink recorders, adjusting the voltages, and occasionally chasing away a cow.

In 1959 a single 90-foot antenna, operating at a frequency of 960 MHz, was all by itself a world-class instrument. Other large dishes in the U.S. (at the Naval Research Laboratory, the University of Michigan, and NRAO) had diameters of 84 or 85 feet. The OVRO 90-foot telescope was also bigger than any instrument in the Netherlands where much of the world's hydrogen line studies of the Milky Way had been made. The only one in the world that had more sensitivity and comparable versatility was the 250-foot telescope at Jodrell Bank in England.

A group from the Convair Corporation, led by Gail Moreton and Bill Erickson, started a program of monitoring the sun on the first 90-footer. They had a swept-frequency receiver and did simultaneous radio and optical observations (the latter with a 4-inch Questar). The Convair group included Chuck Spencer, an engineer who stayed on after the solar program stopped, and worked at OVRO until his retirement in 1985. This program operated during the International Geophysical Year (1958–59) and was one of many that followed the sun and its interactions with the terrestrial atmosphere.

In the spring and summer of 1959 Stanley and postdoc Jim Roberts measured the radiation from Jupiter at a wavelength of 31 cm and showed that its apparent temperature indeed was high (5,500 K) and that Jupiter had a Van Allen belt like Earth, as had been suggested by observations at shorter wavelengths. Bob Wilson and Bolton made maps of the galactic plane and cataloged point sources in the Milky Way; many of these would turn out to be supernova remnants. Wilson's PhD thesis grew out of this work.

Using a 21-cm receiver in the second 90-foot dish, when it became available, Radhakrishnan studied self-absorption in hydrogen clouds, and proved that the temperature of clouds near the supernova remnant IC 443 could be no more than 60 K. At the time the consensus had been that these clouds were at a temperature of about 125 K (although there had been suggestions that some were colder) but Rad's work showed that the clouds came in greater variety and were more complex than had been assumed.

Much of the work with the single antenna, however, was directly related to the prime motivation behind the whole project—identification of the discrete radio sources. By 1959 the Cambridge and Sydney catalogs had been refined well beyond the early work, which had contained many errors and false sources. But there still were discrepancies, and errors in position of minutes of arc, 10 to 100 times worse than required for identifications. Harris and Roberts studied about 100 objects from the third Cambridge catalog, in order to prepare an accurate finding list for later studies with the interferometer.

The CTA (for Cal Tech List A) catalog, the result of the work by Harris and Roberts, also contained a number of new objects discovered by chance. One of these, CTA 102, was seen to fluctuate by Sholomitsky in the USSR in 1965. Unfortunately, this observation was coupled with the notion of external civilizations sending us radio signals, and a popular song was even written about CTA 102. Although Sholomitsky's work was not believed and indeed "proved" wrong, that view has changed and now it is realized that Sholomitsky could well have been correct. CTA 102 and other small sources do fluctuate at decimeter wavelengths, because irregularities in the interstellar medium cause "seeing" much like the twinkling of stars.

When the east-west arm of the interferometer became operational in February 1960, it immediately began to measure accurate right ascensions (RA) of known strong sources, to get at the identification problem that had been driving the project from the beginning. The most spectacular success came with the object 3C 295, which was thought to be a distant analog of the Cygnus A radio source. A reasonably good position had been supplied by the Cambridge group in 1959. The improvement in RA provided by the Caltech interferometer then enabled Rudolph Minkowski, using the Palomar 200-inch telescope, to identify 3C 295 with a distant galaxy with a redshift of 0.461. This made it about eight times farther away than Cygnus A, and for many years it was the most distant galaxy known.

One of the first programs was a study of galactic clouds composed of neutral hydrogen. Such studies in the previous decade (made with a single dish) had led to a mapping of the Milky Way, but no one had a clear idea of the size and structure of the clouds themselves. The interferometer, with its high resolution, could give detailed information on the size, density, and temperature in the clouds. Barry Clark's thesis grew out of this work, and several later students

The completed L-shaped interferometer in the early 1960s, looking east (top) toward the railroad and the White Mountains, and north (bottom) along the floodplain of the river valley.

also wrote theses on interferometric studies of hydrogen clouds. In recent years this has become a major industry at OVRO, with many people studying interstellar clouds with the new millimeter interferometer. Most radio astronomers nowadays, however, study exotic molecules such as carbon monoxide, hydrogen cyanide, or formaldehyde, rather than neutral hydrogen atoms.

Kellermann and Harris conducted another early program with the east-west interferometer, observing 739 sources from the Sydney catalog in order to check its reliability. The OVRO interferometer was well suited for this, as it operated on a much higher frequency than the system used at Sydney (960 MHz vs 86 MHz) and had better positional accuracy. Checking the existing catalogs was important, not only to improve positions but also to establish the degree of completeness, to settle a statistical argument. The data suggested that there was a strong cosmological evolution in the density of extragalactic radio objects—that they had been much more numerous in the distant past. This conclusion depended critically, however, on having complete and accurate data. By 1960 the main discrepancies between the Cambridge and Sydney data had been eliminated; but many uncertainties remained and there were fierce arguments over the correct interpretation.

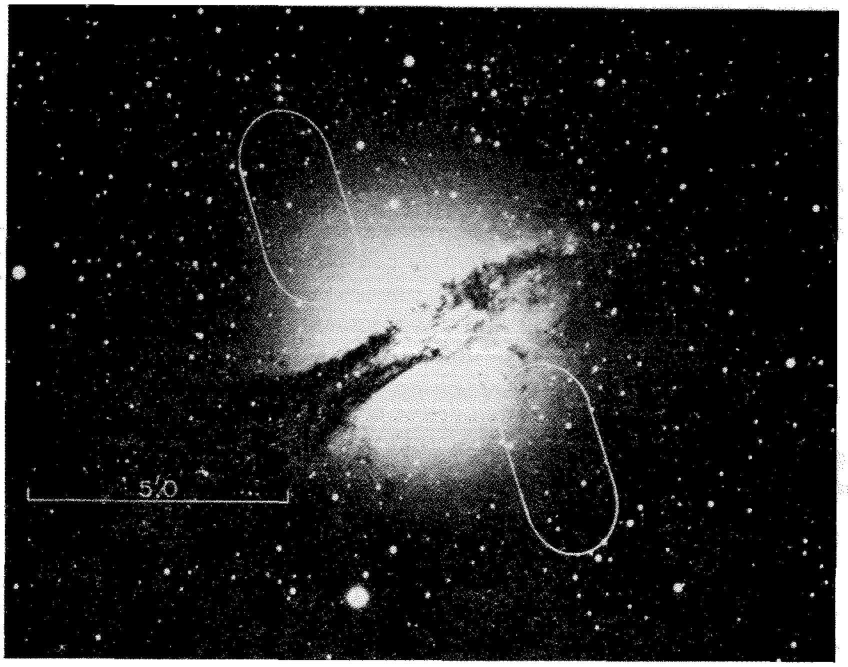
Kellermann and Harris found approximately three-quarters of the Sydney objects. Some of the missing sources were too weak to be seen at the high OVRO frequency, but many apparently had substantial position errors. Further, some of the Sydney sources listed as "extended" actually were blends of two or more independent objects. The result of this work was that the major catalogs were corrected, but still the arguments over evolution persisted. Indeed they have not totally died down now, 30 years later. Although the evidence for an evolutionary universe is regarded as overwhelming by nearly all astronomers, a small but ingenious band of astrophysicists still returns regularly to the idea of a steady-state, nonevolving universe.

The Full Interferometer

In late 1960 the north-south arm also started functioning. Dick Read was able to use it to determine accurate declinations for many sources, and this formed part of his thesis. His work reduced the error boxes for the location of many of these objects, and allowed the important identification program to accelerate. This involved a major collaboration between the radio astronomers, who measured positions with the interferometer, and the optical astronomers, who tried to find the corresponding objects with the Palomar telescope.

With two-dimensional measurements, two-dimensional models or even crude maps could be made, and a new level of investigation began. The maps were the best the world had seen to that time, although primitive by modern standards, and a number of striking results were obtained in the first few years. The procedure was developed in Cambridge and Sydney in the 1950s and is called "aperture synthesis" or "earth-rotation synthesis." In this scheme a simple interferometer is used at many spacings, and the measurements are combined to give the result of the entire set working at once, that is, a large aperture is synthesized. Observations out of the meridian plane give more data because the earth's rotation gives a variety of projected spacings (as seen from the sky) without physically moving the antennas. This is not as easy as it sounds, however, because the differential delay in the two sides must be compensated by extra cable inserted into the short side, and the delay change must be tracked with high precision. At first the OVRO interferometer was used only in the meridian plane; then observations were made at fixed-hour angles, and, after a few years of technical development, continuous tracking became possible. Fritz Bartlett and Dick Read, following Stanley's suggestions, built a wonderful analog computer to accomplish part of this. It was full of interesting wheels and cams, and it was a sad day when it was replaced by a microprocessor.

Barry Clark recently wrote of this device, "I



This picture of the galaxy NGC 5128, photographed with the Palomar 200-inch telescope, is taken from an article published by Per Maltby in *Nature* in 1961. Superimposed is a model of the radio emission (the two ovals) made with east-west and north-south observations at two frequencies with the interferometer. The radio source is double and lies outside the dust region (the black bands along the equator). NGC 5128 also has a gigantic outer radio source that stretches for a full 10° across the sky.

Right: This circa-1960 view of the OVRO variable-spacing interferometer looks from the northeast toward the Sierra Crest skyline. The system has been through many modifications and improvements, and currently is used exclusively to study the sun.

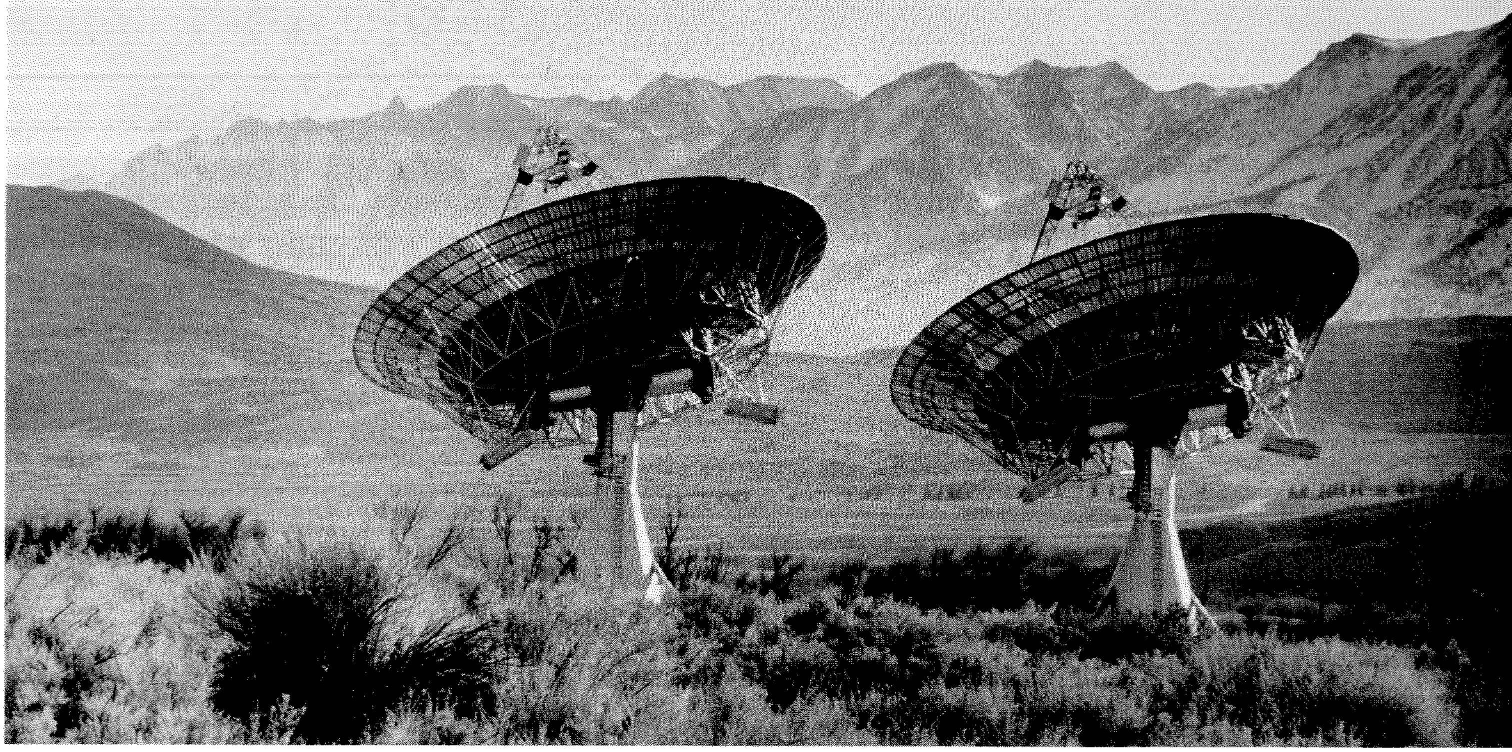
have fond memories of the ball-and-disk analog computer. My memory is that the realization that the integral of the fringe function was the delay was an afterthought, and that the integral was then brought out on a pulley, over which a string ran to another pulley mounted on the shaft of a lumped-constant delay line. For years the final piece of the apparatus was a four-inch crescent wrench tied on the end of the string to provide the motive power to turn the delay line. It was while watching that machine, and thinking 'there oughta be a better way' that I thought up the three-level lobe rotator used in the Mk II correlator, and calculated its signal-to-noise ratio." (The last refers to developments in computerized interferometry that Clark made several years later while at NRAO.)

Alan Moffet, Per Maltby and Tom Matthews did the first synthesis program on the complete interferometer: a study of a number of radio sources, essentially all those that had been identified with optical objects. While many of these objects had been identified earlier, the Caltech program was able to identify others because of its superior positional accuracy. The east-west portion of these observations began in February 1960, and together with the north-south observations lasted into the winter of 1960–61. Their results were published in 1962 in an important series of four papers; part of the work also formed Moffet's PhD thesis.

The image restoration procedures used in these papers are familiar to the modern student of radio interferometry, but they were carried out in the old-fashioned way. Digital computers were not common in 1961, and the authors took the trouble to inform the reader that the visibility functions ". . . were numerically inverted using an electronic digital computer." They performed the interpolation and gridding operation by plotting the visibility points by hand, joining them with a smooth hand-drawn curve, and reading off the appropriate values at the grid points.

The most significant fact discovered in these observations was that most extragalactic radio sources are double; that is, they consist of two well-separated clumps (lobes) of radio emission. The centroid of this brightness distribution often coincides with a faint galaxy, even when the radio lobes are well outside all the visible light. Maltby, Matthews, and Moffet noted further that the visible "radio galaxies" typically have unusual shapes: they are distorted, or have jets, or are located in a small group of galaxies. This story is still valid, although, of course, it has been enhanced by later developments.

During that period there was intense but friendly competition in the study of the structure of the extragalactic radio sources. The double nature of the majority of extragalactic radio sources is regarded as a major discovery and has generally been attributed to Maltby and Moffet. The idea, however, circulated around at the time, and in their first published note in *Nature* in 1961, Moffet and Maltby acknowledge that Palmer and Brown at Manchester also had evidence that many sources were double. The Manchester interferometer had stations much farther apart and so had more angular resolution than OVRO, but did not give detailed two-dimensional information. The first signs of double structure had actually come from Manchester nearly a decade earlier, when Jennison and Das Gupta showed that the Cygnus radio source was double. By 1961 it even was known, from the work by Lequeux in France, that the lobes in Cygnus were brightened at their forward edges, causing speculation about shock waves. But in



1961 the OVRO interferometer was superior to all others and the full survey carried out by Moffet and Maltby became the standard reference.

Another important early program was the study of the polarization and size of the radio-emitting cloud around the planet Jupiter. In several papers published between 1959 and 1961, Roberts and Stanley, Radhakrishnan and Roberts, and Morris and Berge reported increasingly improved observations of Jupiter. Two-dimensional measurements at two wavelengths, 31 and 22 cm, showed that the cloud was elliptical, with equatorial diameter three times the polar diameter, and the radiation was strongly polarized in the equatorial direction. This was a sure sign of synchrotron radiation from electrons trapped in a Van Allen belt around Jupiter. The observations even showed that the magnetic axis was offset by 9° from the rotation axis. Glenn Berge extended this work to shorter wavelengths to get a more detailed picture, and wrote his thesis on this topic. All this was confirmed many years later when Voyagers 1 and 2 flew by Jupiter and directly measured the particles and fields.

John Bolton

This article is dedicated to John Bolton. At the end of 1960, six years after establishing the Owens Valley Radio Observatory, Bolton returned to Australia to supervise the commissioning of the 210-foot telescope at Parkes. (Stanley was appointed his successor at OVRO, and the next installment of this story will describe how new instruments and new people changed its direction.) Bolton became director of the Parkes Observatory when the telescope became operational, and remained in that position for 10 years. In 1969–1973 he directed the Parkes program to receive signals from the moon for the NASA Apollo program. He remained interested in identifications of radio sources during his entire career, and also did extensive optical work in that area. Bolton retired to Buderim, Queensland, on the Sunshine Coast north of Brisbane in 1981, for reasons of health. I was fortunate to have visited John and Letty at their home in June 1993, to talk with them about the observatory and many other projects. He died a week after my visit.

Since he didn't come to Caltech until 1968, Professor of Astronomy Marshall Cohen was not actually on the Owens Valley scene during the years he has described above, and is grateful to Jesse Greenstein, Gordon Stanley, Robert Bacher, Glenn Berge, Barry Clark, Dan Harris, Ken Kellermann, Dick Read, Jim Roberts, and George Seilestad for their discussions with him and their comments on the manuscript of this history. Cohen studied electrical engineering (BEE 1948) and physics (PhD 1952) at Ohio State University, and began work in radio astronomy in 1954 when he joined the School of Electrical Engineering at Cornell as an assistant professor. In the late fifties he helped plan the large reflector that Cornell built at the Arecibo Ionospheric Observatory, and participated in its commissioning in 1961-62. After two years at UC San Diego, he came to Caltech as professor of radio astronomy. Cohen then worked at OVRO for many years, but to his regret has been there only infrequently since the late eighties. His most recent interests involve optical polarimetric studies of active galactic nuclei at the Palomar and Keck observatories.

Farrer Park

An army for an independent India, a true Indian Army, was offered that afternoon at Farrer Park.

by Peter Ward Fay

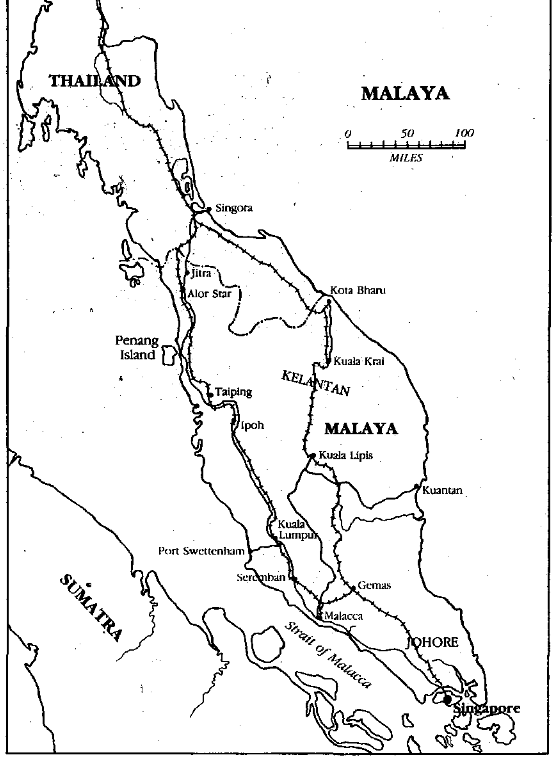
Professor of History Peter Fay's book, The Forgotten Army, *from which this chapter is excerpted, had its start in Kanpur, India, where for two years in the middle 1960s Fay taught at an institute of technology just getting under way with Caltech's help. In Kanpur, Fay met Prem Sahgal and his wife Lakshmi. He managed a mill; she had an obstetrical practice. Two decades earlier, however, both had played important roles in the "forgotten army" of the title. Their stories fascinated Fay. Years later he returned to Kanpur and taped their recollections, work that led eventually to a broader study of that army—why it was formed, how, and with what result.*

It has long been widely believed, particularly in the West, that India did not fight for her freedom from the British Empire—because she did not need to. The nonviolent tactics of Gandhi and Nehru sufficed, and on August 15, 1947, Britain "transferred power" in the manner of a father handing the car keys to his son. But in the persons of Prem and Lakshmi, Fay encountered Indians who believed that struggle had, in fact, been necessary; they were among those who seized an opportunity the Japanese offered and took up arms. (Prem commanded a regiment in the field, and Lakshmi had organized a women's unit.) More than 10,000 Indian soldiers made prisoner when the Japanese overran Malaya early in 1942, together with some 5,000 recruited on the spot and trained, moved early in 1944 to Burma, which the Japanese also held. There this Indian National Army (INA), commanded by a charismatic, sometime Congress Party leader named Subhas Chandra Bose, prepared to break into India on the flanks of the Japanese and ignite rebellion. Prospects were good—anti-British feeling was rising. Then in mid-1944 the Japanese were stopped dead on the border. By early 1945 they had lost Burma itself, and the INA lay beaten and scattered. Bose died in a plane crash. Prem (who, like Lakshmi, was close to Bose's high command) was brought back to India and, with fellow officers Gurbaksh Singh Dhillon and Shah Nawaz, put on trial for treason. But by this time the war was over, censorship had been lifted, and the trial (in Delhi's Red Fort) was public. All India, learning for the first time what the INA had attempted, rocked with excitement and indignation. Even the Indian Army—the old British Indian Army, ultimate defender and enforcer of the Raj—was shaken. Officers and men alike grew restless. Headquarters concluded that they were ceasing to be reliable. And that, Fay argues, not prior British commitment, made the prompt granting of independence inevitable. It was a defeated INA that gave freedom its decisive push.

When "Farrer Park" (chapter 4 of the book) opens, in mid-February 1942, Malaya has fallen. Prem, having fought the Japanese the full length of the peninsula, has just been captured by a ruse. And Lakshmi is still with the clinic that had brought her to Singapore 20 months before.

***The Forgotten Army: India's Armed Struggle for Independence, 1943–45*, by Peter Ward Fay was published by the University of Michigan Press, 1993.**

Kate Caffrey's *Out in the Midday Sun* is a vivid account of how Malaya fell, one of the best I've seen, based largely on the recollections of men who fought in that miserable campaign. There is something odd, however, about the names. You notice a Braddon and a Brereton, a James, a Morrison, a Russell-Roberts. The units are the Bedfordshires and the Cambridgeshires, the Royal Norfolks, the 18th Australian Division.



Early on the morning of December 8, 1941, in a strike timed to coincide with the December 7 attack on Pearl Harbor, the Japanese went ashore at Kota Bharu on the northeast coast of Malaya, fought their way inland (against Prem's 2/10th Baluch battalion, among others), and began the campaign that by mid-February gave them Singapore.

You come across "Painter's men"—it is only when you look closely that you realize these are actually the Sikhs and Garhwalis of the 22nd Indian Brigade, whom Caffrey prefers to call after their British commander. Indeed, all through the fighting Indians *as Indians* rarely appear. And when the fighting is over, when Singapore has surrendered and Caffrey starts to tell us what happened to the survivors, they become utterly invisible.

In her account of the campaign we do meet a few Indian units. It will please any veteran of the 2/10th Baluch, and also startle him since the regiment had long ceased to recruit from that part of the subcontinent, to read that "heavy fighting went on . . . around the village of Nee Soon, the Imperial Guards hammering away at a regiment from Baluchistan that gave a good account of itself in spite of being faced by tanks. . . ." The fighting over, however, units and men alike quite disappear. "The British and Australian troops," Caffrey writes, "were given until five in the evening of February 7—44 and a half hours from the official cease-fire—to assemble in the Changi area." Changi being at the eastern tip of the island, the men would have over a dozen miles to walk. So on that Tuesday, which Caffrey says dawned clear and hot, "the long, long column set off, headed by at least four files of brigadiers and full colonels, with here and there a lorry on which some soldiers hitched a ride for part of the way." What she does not say is that many of these brigadiers and colonels had commanded Indians. (There were more Indians in Percival's army than British and Australians combined.) Key, for example, whom Prem thought so well of, and whom Caffrey describes as "a short, thick-set, hearty man with a round face" and a very determined manner—Key must have been in those files, and Key had commanded the 8th Indian Brigade, and later the 11th Indian Division. But none of Key's jawans (common soldiers) were in the long, long column. None of his VCOs (Viceroy's commissioned officers, invariably Indian) and ICOs (Indian commissioned officers) were in it either. Prem himself, had he surrendered with the others instead of being tricked into captivity a day early, would not have trudged off behind Key. Virtually no Indian did.

This was not because they did not wish to. The Japanese gave them no choice. Already Prem had discovered, when his English second in command was beheaded and he was not, that the Japanese intended something special for their Indian prisoners. In fact, the process of distinguishing Indians from the British and the Australians had begun early in the Malayan campaign. It had begun with a Sikh captain of much personal and political restiveness named Mohan Singh.

Japanese tanks had shattered Mohan Singh's 1/14th Punjab Battalion at Jitra, on the west side of the peninsula, early in the fighting. After a day spent wandering in jungle and swamp, and several days hiding while the fighting moved farther and farther away, Mohan Singh was in a frame of mind to listen (it owed something to his longstanding dissatisfaction with the way the Army had treated him) when by chance he was picked up by the Japanese Army *kikan,* or agency, charged with making friendly contact with Indians. A certain Pritam Singh, expatriate Sikh and founder in Bangkok several years before of an Indian Independence League, did the talking. But the driving force in this *Fujiwara Kikan* (sensibly, the Japanese called these agencies after the men who led them) was Fujiwara himself.

By all accounts Major Fujiwara Iwaichi was a remarkable man. Young, newly promoted, hardly two months on his assignment, with no Hindustani, little English, and supported by only a tiny staff, he had nevertheless already managed to set up a joint *Kikan*-Indian Independence League office at Alor Star, near Jitra, and to collect several hundred Indian stragglers. He had the confidence of Pritam Singh and the other Indians from Bangkok, though he had met them only in October. More surprising, he believed in the overtures he was instructed to make with a sincerity not to be doubted. (Years later he was to refer to himself as the Lawrence of the Indian

National Army.) Japan must capture the hearts of the Indians, a thing she had signally failed to do with the Chinese. Japan must help them obtain their freedom. And she must do so for the reason that it was right to do so, not simply to advantage Nippon.

The fighting had left Alor Star behind, there was a good deal of looting, Fujiwara lacked the means to stop it—and turned to Mohan Singh. Mohan Singh assembled a few score fellow Indians and did the job. The two hit it off well (they were both 33), and in a short while Mohan Singh was organizing Indians all over northern Malaya. At the end of December, after meeting the Japanese commanding general and receiving assurances that Fujiwara spoke for more than himself, Mohan Singh agreed to raise an army to fight alongside the Japanese. Though it might eventually draw upon Indian civilians, for the time being it must be recruited from captured Indian soldiers. A headquarters was established and volunteers called for. As they came forward they were issued rifles, given arm bands bearing the letter F, and sent south to collect more of their kind.

In the evening an order was received—a British order—that all Indians were to march the following day to Farrer Park, a sports ground a few miles away.

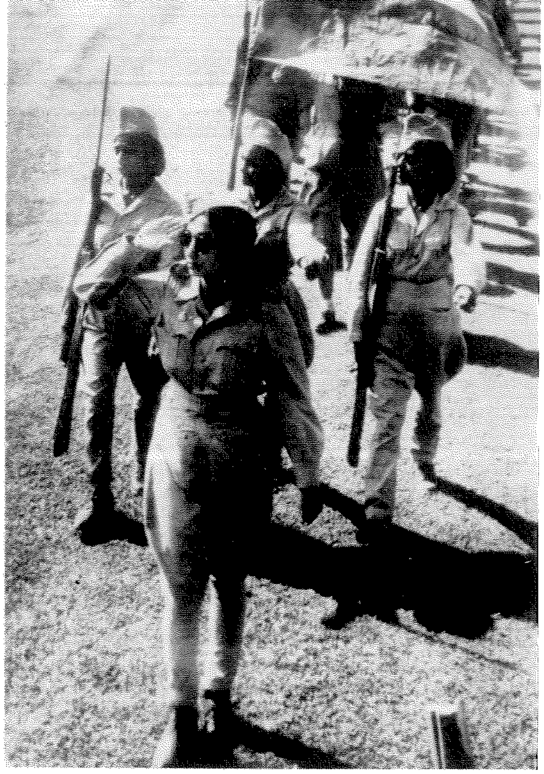
Of all this Lakshmi had some inkling; "at the aid post our position became very awkward, because some of these Britishers said, 'oh, we've been let down by the Indian troops, they've gone over to the Japanese.'" But she did not really believe it. As for Prem, he was quite unaware of the rumor. Much later, it is true, he allowed himself to wonder whether the message that had brought him up the fatal *nullah* (ditch) into the arms of the Japanese had not been the work of Fujiwara Volunteers. If it had, it had worked only because it concealed a ruse. Units that avoided being trapped or broken and retained their confidence and fighting spirit, as Prem's had, offered few stragglers and therefore few prospective recruits for Mohan Singh's roving parties. And so long as the fighting continued, the men in such units had little time or inclination to question the politics of the war, or to ask themselves what India and Indians should do.

With the fall of Singapore, however, things changed. Prem was identified as an Indian, which saved his life. He was separated from the British officers of his battalion. His captors of the Imperial Guards Division kept him with them, though more as a guest than as a prisoner. And when, after several days, he grew restless and asked to rejoin his battalion, they gave him a vehicle and let him go find it himself.

It was while he was the guest of the Guards Division that the Farrer Park meeting took place, the meeting that more than any other single act set the Indians in Malaya on the road to active war against Britain-in-India.

Singapore surrendered on Sunday, February 15. Next morning the 1/14th and 5/14th Punjab, amalgamated as one battalion because of the losses suffered at Jitra and Slim River, piled their arms near Bidadari, a mile or so south of Paya Lebar. In the evening an order was received—a British order—that all Indians were to march the following day to Farrer Park, a sports ground a few miles away. That meant jawans, noncommissioned officers, VCOs, ICOs, the lot. The 14th Punjab had no British other ranks or NCOs. But it did have British officers—and they were not covered by this order, they had their own. It directed them not southwest to Farrer Park but east to Changi.

To Shah Nawaz, a captain in the 1/14th, this was disturbing. Shah Nawaz came from a large family in Rawalpindi, close to the Northwest Frontier; a Muslim family, but one conscious of Rajput origins; an old military family of the sort that generation after generation had sent its sons to be jemadars [equal to lieutenants] and subadars [captains] and subadar-majors [majors] in the Indian Army. Shah Nawaz's father had served for 30 years. Shah Nawaz himself was later to say that not one of his able-bodied male relatives had failed to wear the King-Emperor's uniform in one World War or the other. Indianization permitted Shah Nawaz to lift himself above the VCO level. In 1933 he entered Dehra Dun. Though three years Prem's senior (he was the 58th Indian cadet to receive a commission, Prem the 226th), circumstances kept him at the



Left: Prem Sahgal (standing, right) with an unidentified British battalion in India some time in 1940.

Right: Lakshmi Swaminadhan (later Sahgal) leads her Rani of Jhansi Regiment on parade—an INA regiment of women named after a heroine who fought in the 1857 Sepoy Rebellion against the British.

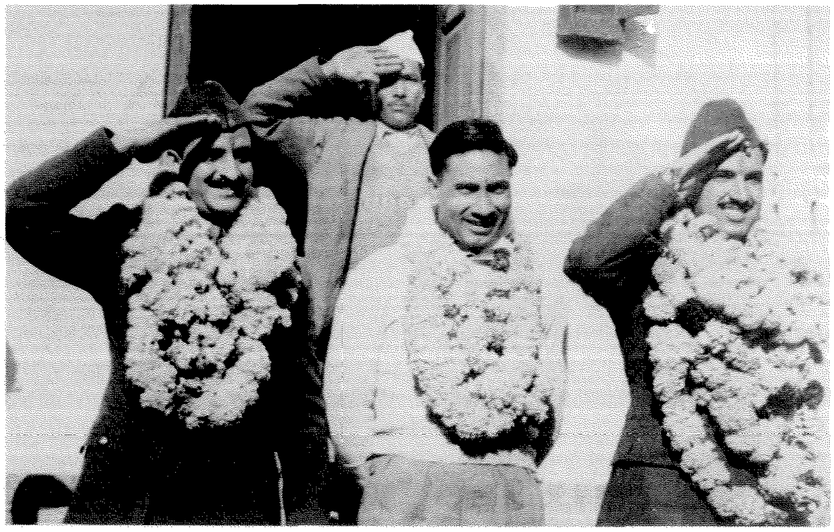
regimental depot long after Prem had gone overseas. Asked for at last by his British battalion commander, he reached Singapore at the end of January in time to join the amalgamated battalion on the island's north shore and withdraw with it to Bidadari. The experience galled him. "To have brought me to Singapore so late in the fight, only to be ordered to lay down my arms and surrender unconditionally, I considered to be extremely unjust to myself and to my sense of honour as a soldier." But what bothered Shah Nawaz now was the order to proceed to Farrer Park. For "according to the laws of civilized warfare, all captured officers, whether Indian or British, are kept together, and separate from rank and file," and this the order proposed not to do.

Another officer of the 1/14th, Lieutenant Gurbaksh Singh Dhillon, was similarly bothered. Dhillon, too, came from a military family, a Sikh family of Lahore. His father was a veterinary surgeon at a cavalry remount depot, one brother was a jemadar in the Service Corps, another was an army clerk. Dhillon himself was marked for medicine. Failing the entrance examination to medical college, however, and with the cloud of family disappointment heavy about his head, he enlisted in the army as an ordinary recruit. By this time he had a wife. Married life on a sepoy's pay (he himself uses the term sepoy instead of the current jawan) was difficult. Dhillon was overeducated for the men he rubbed shoulders with, and overqualified for the tasks he was set. For a time he thought of quitting. But his wife's encouragement, and his own determination and exuberance, drove him instead to search out every possible avenue of training and advancement, with a commission his goal.

Inclined from his youth to imagine slights and fancy himself insulted, his path upward was by his own account marked by scuffles. Nevertheless he rose, qualified for the two-year course at Kitchener College, and went on to the Military Academy at Dehra Dun. Emerging in 1940 number 336 on the ICO list, at a time when the war had put a stop to the practice of placing graduates temporarily with a British battalion, he was posted to the 1/14th Punjab at Lahore "in the very lines where I had stayed as a Sepoy." When the battalion went south to Secunderabad, he went with it.

His recollections of his first months as an officer, however, read very differently from Prem's. At Lahore he was refused admission to the swimming club. Of Secunderabad he remembers not polo (he did not play), not mess nights (he was probably ill at ease), but how some senior British officers ignored him socially. "When I told my feelings to some of my brother officers," he adds after making this brief but bitter observation, "I was surprised to learn many more stories of discrimination." In March 1941 the 1/14th sailed for Malaya. As he went aboard, Dhillon exchanged angry words with an English sergeant-major of the embarkation staff. "The result was the C.O. did not talk to me throughout the voyage." In Malaya, where the battalion was quartered first at Ipoh, later at Sungei Patani, there was further unpleasantness: when the Indian officers tried to introduce Indian food into the mess; when they protested emergency-commissioned tea planters being given companies over their heads. Like Mohan Singh—a fellow Sikh, and in the same battalion, who had also worked his way up from the ranks—Dhillon carried with him a considerable baggage of resentment, a load he was temperamentally unable to lighten by riding with these Britishers, drinking with them, compelling them by the sheer force of his assurance (as Prem could) to amend their British ways.

Shortly after the battalion reached Malaya, the adjutant, an early Dehra Dun graduate named Zaman Kiani, fell ill. Temporarily, Dhillon took his place. Later he was sent back to India to do a signals course. Leave followed. He rejoined the 1/14th a few days before the Japanese attack. At Jitra his experiences were much like Mohan Singh's, but turned out differently, for with others he managed to escape down the coast by boat and rejoin what remained of the battalion. That, however, was for him the end of the fighting. He fell sick and went into hospital in



The three defendants in the first Red Fort trial—from left, Shah Nawaz, Gurbaksh Singh Dhillon, and Prem Sahgal—were found guilty of waging war against the king-emperor and sentenced to dismissal from the service, forfeiture of pay and allowances, and transportation (in effect, imprisonment) for life. The last, however, was remitted and they were released. Here, only hours later, Shah Nawaz and Prem salute INA (also Royal Navy) style, while the unidentified man behind them salutes as the British do.

Singapore, where characteristically he noted and protested the habit of refusing Indian troops access to British canteens. Though he was discharged before the final battle began, he had not managed to rejoin his battalion when the surrender came.

So when, on the morning of Tuesday, February 17, the combined 1/14th and 5/14th paraded under Zaman Kiani and marched off without their British officers to Farrer Park, Shah Nawaz marched with them. Dhillon marched with a unit to which he had been temporarily attached. Prem did not go at all. Of the three men who not quite four years later would be brought to trial in Delhi on a charge of treason, of the three defendants in the first Red Fort trial, two would remember (because they were there) the symbolic act by which the bond binding Percival's Indians to the King-Emperor's service was severed. Not that the guilt or innocence of Prem Sahgal, Dhillon, and Shah Nawaz would necessarily turn on the issue of allegiance. In the concluding address that old Bhulabhai Desai, chief counsel for the defense, delivered in late 1945 at the court-martial of these three, the point was early made that allegiance was not the issue. "Here is a case in which I venture to say—and the evidence supports it—that it is not at all a case of what you might call three individuals waging war against the king." It was not a case of personal allegiance at all. "I quite agree," Bhulabhai continued, "that if ten persons in a village declare war on Britain, they are rebels," and may be dealt with individually. But here was another matter. Here was a provisional government and an army. Sahgal, Dhillon, and Shah Nawaz were members of that army. It was not they who were on trial, it was that army, the Indian National Army. More exactly, what was on trial was the right of the Indian National Army to wage war for the liberation of India.

Of course those who waged a war of liberation, Bhulabhai admitted, began that war still bound by the previous allegiance, "the *prima facie* allegiance if I may so call it." And this allegiance could not wholly disappear until the war was won and liberation achieved— which, in this case, had not occurred. But neither had it occurred in the case of the American South, whose soldiers had nevertheless not been charged with breach of allegiance and put on trial for their lives. Win or lose, a war for the liberation of a people, if properly declared and conducted, gave to men fighting that war the rights and immunities of belligerents. Bhulabhai would demonstrate this with examples drawn from international law and history. And he proceeded to do so, in parts of an address that lasted ten hours and consumed two days.

Had not these rights and immunities passed, at England's insistence no less, to the South American rebels of Bolivar's day? to the Greeks for whom Byron died? to Garibaldi and The Thousand? In the European conflict just ended, fleeing remnants of Dutchmen, Poles, and Yugoslavs had taken refuge in England and constructed governments in exile there, governments possessing "not an inch of territory they could call their own. . . . And the fact that they were deprived of their territory temporarily, or the fact that the Indians were deprived of their territories for 150 years, makes not the slightest difference to the point that we are submitting to the Court." Belligerent rights had been successfully demanded for the one and could not reasonably be denied the other. Even the fragmented and frequently furtive French Resistance had qualified. Indeed, "if the Maquis," Bhulabhai pointed out, "were entitled to all the privileges and immunities of a fighting force," as Eisenhower himself had warned the Germans they were, "I cannot see how you can fail to accord a similar treatment to the Indian National Army." Allegiance was irrelevant. It was for the court simply to determine whether there had been "a de facto political organization sufficient in numbers, sufficient in character, and sufficient in resources to constitute itself capable of declaring and making war with an organized army." If the court found that such there had been, Sahgal, Dhillon, and Shah Nawaz must go free.

Bhulabhai knew, however, that the prosecution would not agree with him. Bhulabhai knew that the seven officers of the court, three Indians and four Englishmen, were not going to dismiss from their minds all considerations of loyalty. They were not going to rule inadmissible the question, were these three renegades? For the principal charge against the three was treason.

To be exact, they were charged with waging war against the King-Emperor contrary to Section 121 of the Indian Penal Code. But that in common parlance was treason, it being understood by every Englishman that to set oneself against king and country was a traitorous act. The good subject was loyal to king and country both. It was a common, an undivided, allegiance.

Suppose, however, that king and country did not coincide, so that a person's allegiance, if he was not to be utterly faithless, must desert the one and attach only to the other. Such cases existed. From his historical stock Bhulabhai chose a celebrated one. It was the case of America. Bhulabhai had already borrowed the Civil War to demonstrate that even a rebellion that fails confers belligerent rights. Now, to seven officers perhaps a little tired of history anyway and certainly not eager to receive further instruction from the far side of the Atlantic, he proceeded to recite the Declaration of Independence, from the self-evident truths and inalienable rights of the opening paragraphs to the colonists' solemn repudiation of allegiance to the British crown at the close. Seventeen seventy-six, he submitted, was "a classical instance of a case where the choice between allegiance to the King and allegiance to the country was presented to the world, and men of honour chose allegiance to their own country." And who were these men of honor? The same Americans whose descendants were now the friends and allies of England, "and if I may say so, their warmest and greatest supporters in the task of saving civilization. You could not have a stronger instance than that."

It was at Philadelphia, in 1776, that the Americans resolved the dilemma of their divided allegiance. At Farrer Park, on the 17th of February, 1942, the Indians of the Indian Army in Malaya did the same. . . .

It was at Philadelphia, in 1776, that the Americans resolved the dilemma of their divided allegiance. At Farrer Park, on the 17th of February, 1942, the Indians of the Indian Army in Malaya did the same—or discovered that willy nilly it was being resolved for them. What happened was this.

On that Tuesday morning the combined 1/14th and 5/14th paraded at Bidadari. The British commanding officer shook hands with Kiani and the other Indian officers, remarking (Shah Nawaz remembers him saying) "I suppose this is the parting of the ways." The battalion moved off. Across the island, in all the places where the fighting and the surrender had deposited them, battalions, companies, and smaller packets of the defeated did the same.

Captain R. M. Arshad of the 5/2nd Punjab remembers that his battalion, reaching Farrer Park shortly after nine o'clock, found a considerable number of men already there. By noon the ground was thick with uniforms. Had every Indian soldier alive in Malaya that day answered to the roll, an observer might have counted some 55,000 men. As it was, though there were jawans on the island who did not receive the order, and jawans on the mainland to whom it was not sent, perhaps 40,000 had collected on the great open space that in better days had been used for horse racing when, early in the afternoon, word went around to assemble before the stadium building on one side of the park.

Officers (there were not many of these, less than 250) came to the front. The men stood behind, grouped some say not by units but by classes, Dogras here, Punjabi Muslims there. On the second floor of the stadium building there was a sort of balcony on which loudspeakers and a microphone had been set up. A number of Japanese and Indians were on this balcony. Some of the Indians wore white arm bands bearing in red the letter F.

"When the parade was ready," continues Arshad, "a British officer—later I learned his name was Colonel Hunt from Malaya Command Headquarters—came in front of the microphone, brought us to attention, and addressed us." Exactly what Hunt said is uncertain. Though he survived the war, he was in England on medical leave when the Red Fort trials began, and so was not asked. Arshad first testified that he told the Indians, "from now on you belong to the Japanese Army" and would have to obey its orders. Later, under cross-examination, he decided that he had said no more than that they were all prisoners of war, and that he was turning them over to the Japanese. But whatever Hunt said he said briefly, in a simple, almost perfunctory manner, with no indication that he was bothered or uncomfortable. "After that," remembers Subadar-Major Baboo Ram of the 1/14th, who was near the front with the other VCOs, "he handed over certain papers to Major Fujiwara, a Japanese officer." (As each unit arrived that morning it had given its strength in writing to Hunt. These, presumably, were the papers.) "Then he saluted him and went back. And after that Major Fujiwara came to the microphone and made a speech in the Japanese language which was translated into English and then retranslated into Hindustani." Fujiwara said a number of things with great and obvious sincerity. He ended by announcing that he was turning the officers and men over to Mohan Singh. Then Mohan Singh came to the microphone and made a speech too. So each handed over to the next, not as one speaker making way for another speaker, but as one command surrendering men to another command.

Much later, in the chorus of anger and embarrassment that rose among Englishmen on the subject of the INA, no one was heard to suggest that Percival should have refused to let himself and his colleagues be separated from their brothers in arms, the Indian officers. No one was heard to suggest that Hunt, on coming to the microphone that February afternoon, should have declined to announce what he was instructed to announce, or should at least have told the men that he spoke because compelled to, and with a heavy heart. At the Red Fort no one charged Hunt with anything (of course the British were not in the dock). No one even asked, of the affair, what had those who sent him intended?

Yet it is perfectly clear that the purpose in addressing those thousands of officers and men just two days after the fall of Singapore cannot have been simply to tell them that the battle was lost and that all of them, British and Indians together, were now prisoners of the Japanese. They knew that well enough.

Yet it is perfectly clear that the purpose in addressing those thousands of officers and men just two days after the fall of Singapore cannot have been simply to tell them that the battle was lost and that all of them, British and Indians together, were now prisoners of the Japanese. They knew that well enough. There had to be another purpose. And the purpose that was perceived, conveyed not just by the words "and I hand you over to the Japanese authorities" but by the arrangements that had gone before, and particularly by the separating without protest of the British officers and other ranks—the purpose perceived by these men whose discipline and loyalty Malaya Command had no reason to doubt, and who had fought bravely, some of them the full length of the peninsula, was the deliberate, formal, one might almost say ceremonial, abdication of a responsibility. In good times, in victorious times, the two races of the Indian Army (to use the traditional term) were bound to each other in "a matter of honour." Now times were bad. So the British were backing out.

"I had a feeling of being completely helpless, of being handed over like cattle by the British to the Japs and by the Japs to Mohan Singh." That was how Shah Nawaz later remembered it. Dhillon, too, felt "like one deserted." Yet at this very moment these men, so recently defeated, so thoroughly abandoned, so far from home, were being offered (by the speakers who followed Hunt) the means of reversing that defeat, of overcoming that abandonment, even of returning to India. Speaking slowly because what he said had to be translated first into English and then into Hindustani, Fujiwara welcomed the soldiers, by this time seated on the ground. They were, he said, to consider themselves not prisoners but friends. In Malaya the British had been thoroughly trounced. In Burma they soon would be. Through her victories, Japan was creating for the peoples of East Asia a co-prosperity sphere based on amity and equality. That sphere would not be secure without an independent India on its western flank. So Japan wished India to be free. To that end she was cooperating in the formation of an army that should liberate her, an army he hoped all would join. And Mohan Singh, following Fujiwara to the microphone, put the new loyalty, the fresh allegiance, squarely to his listeners. "We are forming an Indian National Army that will fight to free India. Are you all prepared to join the Indian National Army?" Were they? As Baboo Ram remembers it, "the audience lifted up their arms, threw their turbans in the air, and showed great pleasure."

Just how great the pleasure, and by how many experienced, it is impossible to tell. Dhillon reports "a feeling of hope and joy by all of us present," and Fujiwara himself says that Mohan Singh's short speech left some men weeping. He spoke in Hindustani, so that almost everybody understood him instantly. And he was an effective speaker. Lakshmi, who of course was not present, remembers of the time she first heard him, months later, how impressed she was: "He had an emotional way of talking. He seemed convinced he had taken the right step, he didn't have any doubts of any kind." At Farrer Park that certainty of tone and manner was directed at men who were beginning to realize that in this part of the world the Raj was finished, and that acquiescence in Japanese wishes was probably their only choice.

For that the British might some day return to Singapore did not seem remotely possible—or particularly desirable, either.

We in the West are so used to regarding the Second World War as a critical contest for the possession of civilized society's body and soul; a contest nearly lost by mismanagement, kept alive by England's stubborn refusal to capitulate, and at last won when America and Russia met in the center of Europe—we in the West are so used to looking at the war in this way, that its Asian dimension never rises in our eyes above the level of a sideshow, an enormous and shameless irrelevance. Germany began the conflict. It would be over, and the world safe again, when Germany was smashed. Meanwhile Japan's entry, like Italy's earlier but on a much larger and more dangerous scale, was an act of the grossest opportunism, a monstrous and unforgivable diversion,

Lakshmi and Prem Sahgal in later years. Prem died suddenly in 1992.

for which she would be duly punished when the work of saving civilization in Europe was done.

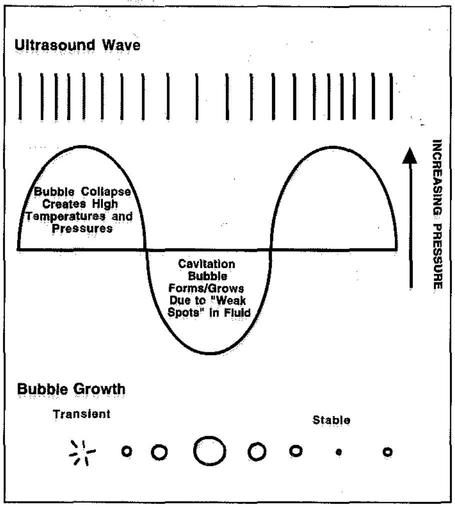
That is the way we view the war now; that is the way we of the North Atlantic community looked at it then. And seeing it thus gave to military events east of Suez a decidedly lower level of significance than attached to events west of it. Even the most chilling disasters in the Pacific theater, even Pearl Harbor, Singapore, and Bataan, struck us as inconclusive. Thrown out of this place or that by the Japanese, we, all of us, like MacArthur, knew that we should return. When the real business of the war, the European business, was finished, we should come back to Asia. And then everything would be as it had been before. Or if there had to be changes, as for example in India where some form of independence would have to be arranged, it would be seen that the war had nothing to do with it. Japanese victories had nothing to do with it. They were an interruption, a damned nuisance of an interruption, which, far from initiating or accelerating those changes, had actually prevented us from getting on with them.

Things, however, did not look this way to Asians. Perhaps Bhulabhai Desai meant it when he complimented the British and their "supporters" the Americans for undertaking "the task of saving civilization," but he was speaking after the war had ended, from Delhi which the Japanese had never occupied, and to a British court he was determined to sway. Indians at the time cannot have thought the task urgent, or Britain the indispensable agent. Whose civilization, anyway, were the British trying to save?

East of Calcutta the Japanese did not appear as usurpers of lands, lands to which the British, French, Dutch, and Americans must triumphantly return when more pressing business elsewhere had been attended to. The Japanese were fellow Asians, with as much right to those lands as westerners had. They offered their fellow Asians, if not equality of status, at least a secure and honorable place in an ordered hierarchy of Asian peoples. As for triumphant return, it was one thing for an Englishman with the Battle of Britain behind him, or an American from a continent that had never been invaded and never could be, to believe in its inevitability. It was quite another for someone on the spot—for an Indian or Malay, say, observing from Singapore's waterfront one April morning a great fleet of battleships and aircraft carriers lying in the roads, the same battleships and carriers that had delivered the crushing blow at Pearl Harbor four months before, and that had given Japan mastery of the waves from Hawaii all the way to the Indian Ocean.

Never much interested in Europe's war except, perhaps, as it might advance a military man's professional career. Accustomed to seeing, where Britain was concerned, not gallant little England of Dunkirk and the white cliffs of Dover but the great, unbending Empire of the Gateway, the Viceroy's Palace, and the Jallianwala Bagh. Confronted, suddenly, with the swift and complete collapse of the eastern portion of that empire and its replacement by a new imperial power both Asian and irresistible. And then invited, with apparent warmth and sincerity, to join in a march westward that should expel the British, obtain purna swaraj [full independence] for Mother India, and (no small point) bring themselves home—how could officers and men fail to be swept away by such a prospect? An army for an independent India, a true Indian Army, was offered that afternoon at Farrer Park. What is surprising is that, even so, there were some who had doubts and held back.

Peter Fay's own World War II experience, in Italy, was limited to a few months' action with 8-inch howitzers just before VE day, and occupation duty thereafter. Returning to Harvard (AB 1947), he won a Rhodes Scholarship to Oxford (BA 1949), and received a Harvard PhD in 1954. In 1955 he joined the Caltech faculty, where, except for the two years in Kanpur, he has taught ever since. His previous book, The Opium War 1840–42, *was published in 1975.*



As an ultrasound wave propagates (top), it generates regions of alternating high and low pressure (middle). A bubble (bottom) can form in a low-pressure zone, and will expand and contract with successive pulses until it grows too big to support itself and implodes, creating tremendous heat and pressure within.

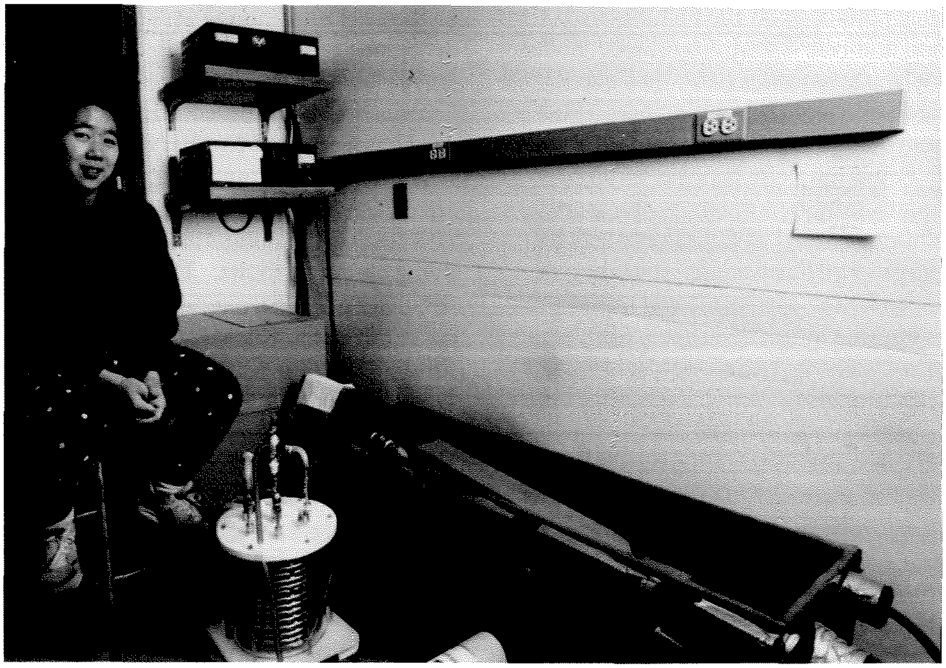
But Only Dogs Can Hear It

In 1927, Alfred Loomis noticed that ultrasound did strange things to dissolved chemicals, but ultrasound was hard to make and nobody got very excited about his discovery. There matters stood until the early 1980s, when cheap and reliable ultrasound generators became available.

In *The Hunt for Red October*, a Soviet nuclear-missile submarine is outfitted with a revolutionary propulsion system that virtually eliminates cavitation noise. In the words of the novel's Oliver Wendell Tyler, ex-submariner turned U.S. Naval Academy teacher, "When you have a propeller turning in the water at high speed, you develop an area of low pressure behind the trailing edge of the blade. This can cause water to vaporize. That creates a bunch of little bubbles. They can't last long under the water pressure, and when they collapse the water rushes forward to pound against the blades. That . . . makes noise, and us sub drivers hate noise." *Red October* would have made it to North America undetected, had it not been for the U.S.S. *Dallas*'s Sonarman Second Class Ronald "Jonesy" Jones, "one of the ten best sonar operators in the fleet," who "had been asked to leave the California Institute of Technology in the middle of his junior year. He had pulled one of the ingenious pranks for which Cal Tech [sic] students were justly famous." But a short in a bad switch started an electrical fire that "burned out a lab, destroying three months of data and fifteen thousand dollars of equipment." So Jonesy joined the Navy, where he was saving up to finance his return.

More broadly, cavitation is the formation of vapor bubbles in any liquid, even molten metal, caused by a pocket of reduced pressure within the liquid. "Cavitation is the death knell of subs," agrees Professor of Environmental Chemistry Michael Hoffmann, who never taught the fictional Jonesy. In Hoffmann's laboratory, cavitation also means curtains for pollutants lurking in water, but it's salvos of ultrasound, not depth charges, that deliver the lethal blow. There is a resemblance between the two methods, however—both rely on pressure waves for their effectiveness. The United States alone generates more than 540 million metric tons of hazardous solid and liquid industrial waste annually, according to Hoffmann, who estimates that some 10 percent of the liquid portion might be treatable with ultrasound. That 10 percent would fill a 21-mile-long train of tank cars daily.

Sounds become inaudible to humans somewhere above 15 kilohertz (kHz), or 15 thousand cycles per second. The threshold decreases with age—few sixty-somethings can hear tones higher than



Hua sits next to the flow-through reactor. The reactor itself is invisible within the coffinlike soundproofing, but the top of one of the ultrasound generators can be seen. The water recirculates through a cooling coil at her feet. The boxes on the shelves behind her are the ultrasound generators' power supplies.

Fifty-five hundred degrees is just a wee bit cooler than the sun's surface, and it's plenty hot to destroy any organic chemical you care to name.

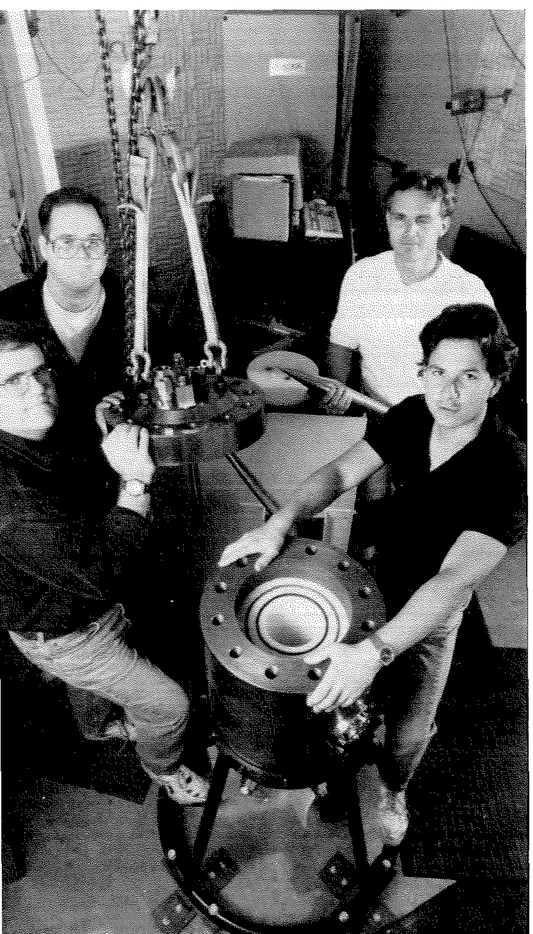
8 kHz. (A piano's eight octaves run from 16 Hz to 4,186 Hz.) Above audible sound lies ultrasound, ranging from 16 kHz to several million hertz. In 1927, Alfred Loomis noticed that ultrasound did strange things to dissolved chemicals, but ultrasound was hard to make and nobody got very excited about his discovery. There matters stood until the early 1980s, when cheap and reliable ultrasound generators became available. Kenneth Suslick (BS '74), now at the University of Illinois, began experimenting with ultrasound as an initiator of chemical reactions, and others, particularly in Germany, followed his lead. A German grad student, Claudius Kormann (PhD '89), got Hoffmann interested in using ultrasound-driven reactions to destroy waterborne pollutants.

Sound waves are pressure waves. They travel through a medium by alternately squeezing and stretching it. As the molecules move apart from one another, the local pressure drops, and—presto!—cavitation occurs. Any gases or volatile liquids dissolved in the water contribute their vapors to the bubble as well. Once a bubble forms, it grows and shrinks with each successive wave. Each time it grows, its expanded surface area and lowered internal pressure allow more vapor to diffuse into it. The influx of new molecules prevents the bubble from collapsing quite as far during the compression that follows, and allows it to grow even bigger on its next expansion. In less than 100 billionths of a second, the bubble reaches a size whose natural resonance frequency is the ultrasound's frequency—for example, 20 kHz equals a bubble roughly 150 microns in diameter. The bubble absorbs the ultrasonic energy like mad, swells like an exploding depth charge, and collapses.

In the crush that follows, the bubble's gas molecules slam together at pressures hundreds of times that of sea level, as measured by the shock waves they produce. Intense pressure generates intense heat. Suslick and others have measured evanescent vapor temperatures of 5,500° C during those last few nanoseconds, while the bubble's liquid skin can reach 2,100 degrees. (An acetylene torch burns at around 2,400° C.) But because the bubbles are so small, the bulk liquid heats up only very slowly.

Fifty-five hundred degrees is just a wee bit cooler than the sun's surface, and it's plenty hot to destroy any organic chemical you care to name. You can get direct pyrolysis—combustion in the form of microscopic underwater flames that cause the liquid to glow the color of a gas-stove burner. Furthermore, the heat dissociates water molecules, and the OH radicals thus formed are powerful oxidants. Such radicals are dangerous terrorists in the wrong places, because

From left: Willberg, Lang, Kratel, and Hoechemar gather around the pulsed-power reactor. Willberg is steadying the reactor's lid, which weighs several hundred pounds all by itself. The nine-inch spike on the lid's underside is one electrode. Kratel holds the other, which is the general size and shape of a mortar round. The countertop behind them hides the capacitor bank. Note the soundproofing on the walls.



they pounce upon the first molecule they find and tear it limb from limb. If these molecular assassins don't find a soft target within the bubble, they diffuse out into the liquid to do their wet work.

Something else happens in ultrasound-irradiated water. At temperatures of more than 374° C and pressures in excess of 221 atmospheres, water goes supercritical—its vapor and liquid phases become indistinguishable from each other. This is a whole new substance, with properties completely unlike the garden-hose stuff. The density drops by a factor of five, and the viscosity by a factor of almost 1,000. Consequently, things diffuse much faster. And the pK_a drops to six, giving water the acid strength of soda pop. (Ever leave a nail sitting in a jar of Coke?) These changes greatly accelerate chemical reactions.

These processes continue as long as the ultrasound is left on, until there's nothing left of the pollutants but simple substances like nitrate, sulfate, carbon dioxide, and water. How much of something gets destroyed by which method depends on the pollutant's initial concentration—the more concentrated, the more pyrolysis.

With such made-to-order mayhem available, ultrasound could be a really good way to get rid of some really nasty stuff. But so far, Hoffmann's group is the only one exploring the chemistry of ultrasound reactions as they apply to pollutants. Says Hoffmann, "We are doing small-scale experiments, trying to understand how the processes work and how to optimize them." The group uses several experimental setups of varying sizes and designs.

In one early effort, Anastassia Kotronarou (PhD '92) and German Mills (then a postdoc in Hoffmann's group and now a professor of chemistry at Auburn University) used a 50-milliliter cell to destroy parathion, a now-banned pesticide. Parathion tends to migrate into water and groundwater, where it lingers. Under natural degradation, half of the dissolved parathion remains after 108 days, according to Hoffmann; its equally toxic metabolite, paraoxon, has a half-life of 144 days. Ultrasound delivering 75 watts per square centimeter at 20 kHz got rid of all the parathion in a saturated solution in two hours. The decomposition products broke down with a half-life of 30 minutes.

Another reactor, used primarily by grad student Inez Hua, has four stainless steel plates that form a rectangular pipe eight centimeters by one centimeter by 100 centimeters long, through which the yucky water flows. The two sets of parallel plates generate ultrasound independently, at different frequencies—a design developed to extract oil from shale. Combining two frequencies gives better results than either one alone, says Hoffmann. "You might set one to 16 kHz, which favors pyrolysis, and the other to a higher frequency, which favors OH radical production." The plates are driven by magnetorestrictive transducers that vibrate in response to a fluctuating electromagnetic field. "This is a particularly efficient setup, because the ultrasound energy is evenly distributed over a large area."

The research group's latest apparatus, run by grad students Ralf Hoechemar, Axel Kratel, Patrick Lang, and postdoc Dean Willberg (PhD '94), is a pulsed-power reactor that uses the discharge from a capacitor bank to zap the water (via a submerged spark gap) with up to 45 kilojoules and up to 25,000 volts at a shot. (At a discharge energy of a mere 12 kilojoules, the group has measured a peak power of 3 million watts, according to Hoffmann.) The blast carries so much juice that the entire $2^1/_2$-ton apparatus, which includes a power bus the size of an I-beam in a freeway overpass, leaped six inches off the floor the first time they let 'er rip. (It has since been bolted down.) Each 20-microsecond burst explosively vaporizes the water around the electrodes. This forms a rapidly expanding plasma bubble that generates a shock wave packing several thousand atmospheres' worth of pressure. The reflected shock waves cause cavitation. The plasma also fries pollutants directly, as does the ultraviolet light it generates. "In the bulk solution, the reaction pathways are similar to those induced by electron beams, gamma rays, or X rays," says Hoffmann. "We're essentially harnessing the effects of a nuclear explosion." One might call it the neutron bomb of the toxic-waste business. Despite its spectacular mode of operation, this machine may be able to achieve high energy efficiencies, says Hoffmann, who hopes that the pulses will provide more bang for the watt than continuous-power systems can.

Although Hoffmann's lab works on a modest scale, his group has done pilot projects for industry. The clients include drug and electronics manufacturers, both of whose effluents contain complex mixtures of chemicals that are difficult and expensive to treat by other methods currently in use. Some of the chemicals the lab has conquered include triethanolamine, p-aminophenol, and carbon tetrachloride. This latter result implies that ultrasound should be able to treat chlorinated compounds such as TCE and PCE, not to mention PCBs. Hoffmann's lab has even destroyed TNT for the U.S. Army. "Ultrasound is proving itself useful against a wide spectrum of contaminants under a whole range of conditions," says Hoffmann. "It's a very general method. That's the beauty of it—unlike most other methods, you don't have to tailor the treatment process to the details of what you're treating." Even when pollutants run silent, run deep underground, if the water can be brought to the surface, they can be treated. It may even be possible to bolt an ultrasound generator to the wellhead and treat the water in situ. ☐ —*DS*

The Gamma Gambit

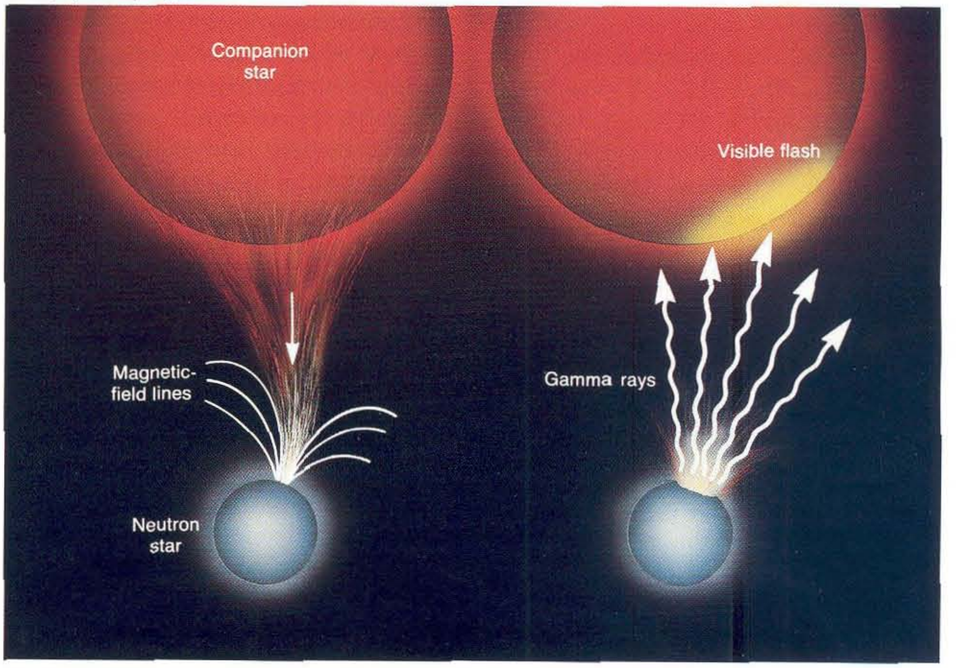
Unseeable from the earth, the neutron star devours its companion, slurping material from the normal star.

In the late 1960s, military satellites on the lookout for clandestine atmospheric nuclear tests discovered something else instead. Like tropical cloudbursts, torrents of gamma rays would suddenly appear, trailing off to background levels in less than a minute. (See *E&S*, Winter 1992.) These celestial outbursts—roughly one per day—don't reach the ground, thanks to the earth's atmosphere. These gamma rays are some of the most energetic astrophysical phenomena ever witnessed, carrying from tens of thousands of eV (electron volts—the energy an electron picks up while traveling across a one-volt potential) up to ten million eV. (For comparison, a photon of visible light has an energy of about two eV.) Ever since the news of their existence was declassified in 1973, astronomers have been trying to figure out where these bursts come from, but telescopic searches of the bursts' apparent points of origin show nothing unusual.

Until the launch of the Gamma Ray Observatory (GRO) in 1991, according to Moseley Professor of Astronomy Maarten Schmidt, astronomers had assumed the bursts came from within our galactic neighborhood. But GRO data show that the bursts come from anywhere in the sky, and never twice from the same spot. (In fact, this is not strictly true—there are three known repeating gamma-ray bursters. Two of these have been identified with supernova remnants—one just this past March, by Professor of Astronomy Shrinivas Kulkarni and colleagues.)

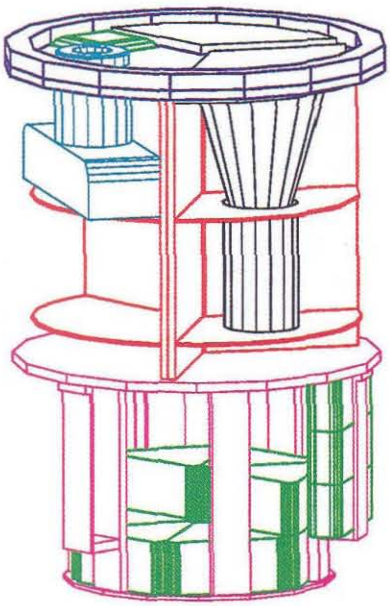
Since the nonrepeating bursts aren't confined to the galactic plane, their sources must either be near enough to lie entirely within the confines of the Milky Way, or far enough away to be outside our galaxy altogether. (The Milky Way is about 2,000 light-years thick and 100,000 light-years in diameter.) Furthermore, the data show that there aren't as many faint bursts as one would expect to see from the observed number of bright ones. This means that the number of sources per unit volume of the universe decreases with distance. That's fine if the sources are extragalactic, but if they're local, it means that we're in a special place in our galaxy where there are lots of sources. Cosmologists don't like this on philosophical grounds—ever since Copernicus dethroned the earth as the center of the universe, scientists have been loath to stipulate that there is anything extraordinary about our galactic neighborhood. "It's a very high-stakes game," says Schmidt. "Either the bursts are a few thousand light-years away, or billions

Courtesy *Sky & Telescope* magazine.



Right: One possible explanation for gamma-ray bursters. A neutron star slowly siphons gas from a companion star. The gas coasts down the magnetic field lines to the neutron star's magnetic pole. Once a critical density accumulates, the hydrogen fuses to helium in a thermonuclear explosion, emitting gamma rays. Some of the gamma rays hit the companion star, where they are absorbed and the energy reemitted as visible light.

Below: GAMCIT's guts. The lid (purple) contains the camera system (blue), GPS receiver (green), and two housings (black) for crystals that scintillate, or emit light, when hit by gamma rays. The superstructure (red) supports the camera and electronics (not shown) and the light cone-photomultiplier tube assembly (black), which captures and records the scintillation light. The battery box (magenta) is filled with battery packs (green), some of which aren't shown for clarity.

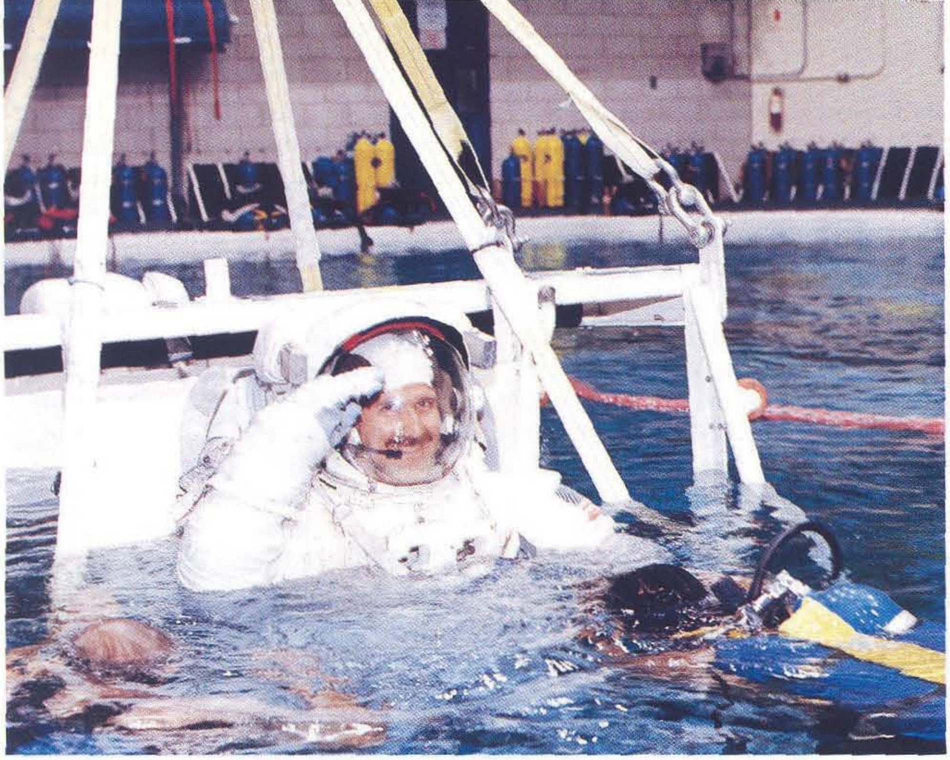


of light-years away. If they're billions of light-years away, they must be from enormously energetic events—perhaps coalescing black holes or colliding neutron stars."

If the bursts are coming from nearby, they could be generated by binary stars that consist of an ordinary star and a neutron star orbiting each other. Unseeable from the earth, the neutron star devours its companion, slurping material from the normal star. The material accretes on the neutron star's surface, where it slowly compacts itself under the star's crushing gravity. The stuff eventually reaches critical density and detonates in a thermonuclear explosion. That would explain why bursts never come from the same spot twice—it takes a good while (perhaps a thousand years) to bunker enough ammunition for a second explosion, and we've only been watching for about 30 years. If this theory is correct, every so often the gamma rays emitted by the explosion should hit the companion star's atmosphere and be reemitted as visible light, causing an optical flash coincident with the gamma-ray burst. However, no one has yet built a detector capable of seeing both at once.

Now, however, some 30 Caltech undergraduates are building a space-shuttle-borne instrument to do just that. The instrument, called GAMCIT, pairs a gamma-ray detector and a 35-millimeter camera. The camera's fixed stare will take in a fair amount of sky as the shuttle orbits. The detector is merely a trigger—when gamma rays hit it, it tells a computer, which must then decide whether a burst is happening. Solar flares also spew floods of gamma rays, electrons, and protons, so GAMCIT carries a charged-particle detector. If the computer logs simultaneous doses of gamma rays and charged particles, it holds its fire. But if the charged-particle detector remains mute, the computer tells the camera to shoot five exposures of one minute each. When the shutter does trip, the computer records the camera's position (but not orientation) to within 10 meters, using data from the Global Positioning System (GPS) satellite network. The computer also records the burst's arrival time, duration, intensity, and energy spectrum from 10,000 eV to 1,000,000 eV.

Once the students retrieve and develop the film, they hope to be able to localize a flash—if they find one—to within one minute of arc. The orientation data comes from the camera itself—each photo should show enough bright, easily recognizable stars to pin down the direction in which the camera was looking. It will take a bit of luck to catch the flash, even if one really occurred, because the gamma-ray detector sees half the sky, but the camera's field of view is



Attended by a retinue *of scuba divers,* astronaut candidate Grunsfeld gives a titanic salute as he's lowered into a pool at the Johnson Space Center for weightlessness *training.*

only about 60 degrees wide. The detector cannot pinpoint sources, so the burst's precise location will be determined by other spacecraft.

GAMCIT is what NASA calls a Getaway-Special Canister, or GAScan. GAScans provide a way for small, experimental payloads to be sent into space relatively quickly and cheaply. Thus undergrads can design, build, and fly a real payload—and analyze the data from it—in less than the time it takes to graduate. (About 100 GAScans have flown to date.) At $19^{3}/_{4}$ inches diameter and $28^{1}/_{4}$ inches high, GAScans are bigger than wastebaskets but smaller than oil drums, and can hold 200 pounds each. They get stowed in odd corners of the shuttle's cargo bay as a mission's size and weight requirements permit. They are self-contained and self-sufficient—GAMCIT gets its juice from 282 size-D batteries. All the astronauts do is open the shuttle's cargo-bay doors and then flip three switches that open the canister's lid and turn on GAMCIT's electrical systems. The switches are in the crew compartment, so no one even lays a glove on the canister.

The students hope to fly GAMCIT on Astro 2, a two-week mission whose main payload is an astronomical observatory containing three ultraviolet telescopes. Astro 2 is currently set for launch in January, 1995, on the space shuttle *Endeavour*. And although the shuttle crew isn't supposed to have anything to do with the GAScans, one member is going to be pulling hard for this one—mission specialist John Grunsfeld, who will be making his maiden flight. Grunsfeld was a gamma-ray astronomer at Caltech's Space Radiation Lab before matriculating at astronaut school in 1992.

While at Caltech, Grunsfeld was the faculty advisor for the campus chapter of SEDS (Students for the Exploration and Development of Space—an international organization). He resuscitated Caltech's GAScan program, which had lain in a coma for a decade, by originating the GAMCIT project. Caltech's Student Space Organization (SSO), now defunct, had reserved four GAScan slots in the early 1980s, according to Grunsfeld, but SSO's first and only payload flew in 1984. It blew its main fuse upon reaching orbit, and that was all she wrote.

That's one bit of history that should not repeat itself. Daniel Burke, the electronics design engineer for Caltech's high-energy physics group, is GAMCIT's technical advisor. "He's given us a lot of advice and help in the design of the electronics," says Albert Ratner, a junior in mechanical engineering. Burke, who has known Grunsfeld since the days when both worked in the Space Radiation Lab, tries to stay in the background, saying, "My primary role is one of support, rather than actually designing or building. This is a bit awkward at times, since the urge is to simply take over and do the work. Restraint is required in order to allow the students to make some mistakes, develop their own initiative, and generally understand how things happen outside the artificial classroom environment. But it *will* be a functional instrument upon completion; we (the advisors) have a significant amount of experience in doing such projects."

Electrical failures notwithstanding, the thermometer is a GAScan's worst enemy. Allen Burrows, of JPL's environmental testing group, has helped the undergrads put GAMCIT's components through an exhaustive thermal program. "That's the single most common source of failure in GAScans," Ratner says. "They freeze or they burn, either at

From left: Ratner, McCall, Burke, and junior Bridget Mattingly have the GAMCIT component situation in hand. McCall holds the mold for the light cone. Mattingly holds one of 24 triangular assemblies that contain 10 D-sized batteries each. And Ratner holds the shipping container in which GAMCIT will journey to Goddard. Note that the shipping container also has a lid that NASA doesn't call a lid—it's a Reuseable End Plate.

the Cape or in space." Ratner and Eric Wemhoff, a senior in mechanical engineering, have done a comprehensive thermal analysis of the whole experiment for a course they're taking in heat transfer and thermal design. A cold snap at Canaveral is certainly a possibility in December, Ratner admits, but GAMCIT should be unfazed—after all, you keep film and batteries fresh by storing them in the fridge, and GAMCIT's electronics have been tested to −10° F. On the sunny side, everything works fine up to about 150° F, at which point the computers begin to get unhappy.

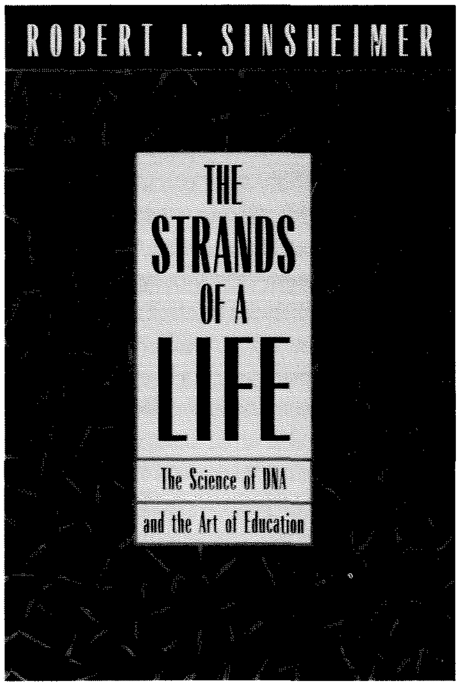
You don't just stroll into Kennedy Space Center with a hunk of machinery and say, "Here. Fly this." Grunsfeld and a small group of students began filing GAMCIT's preliminary paperwork sometime around 1990. Getting into the government mind-set has been an experience in itself. For example, GAMCIT's lid—a steel plate with a quartz window that keeps the can pressurized while allowing the camera to see out into space—is now a UDMD, or User-Designed Mounting Disk. "We called it a lid, and they flipped," Ratner recalls. "They said, 'People will confuse it with our lid.' It was so much more worth it to call it a UDMD than to have people be confused." (GAMCIT will ride in a NASA-supplied canister that also has a lid, you see. But, in fact, NASA doesn't call *its* lid a lid, either—it's a Motorized Door Assembly, or MDA.) As for navigating the flight-certification labyrinth, "If I actually call them and ask a specific question, they're fine. They'll explain everything. But in getting stuff to us in advance, in explaining requirements, in volunteering information, they've been really bad. I mean, I'm not psychic." Even so, GAMCIT had its Final Safety Package approved on January 14, 1994, which means that NASA is satisfied that GAMCIT won't explode, catch fire, or otherwise injure the shuttle. (Ratner spent Christmas Day doing stress analyses for the certification.)

Now that the design has been okayed, it's time to start construction. The GAMCIT team has tested most of the components. The large-scale machine drawings from which the flight hardware will be fabricated are pretty much finished, too. Assuming Astro 2's date with the stars doesn't slip and the god of payload assignments smiles, GAMCIT will be assembled during July. Two or three people will drive it to Goddard Spaceflight Center in Maryland in the ASCIT van in August (road trip, anyone?). The Goddard folks will put it in its actual flight container, test it for electromagnetic interference to be sure it doesn't jam some vital shuttle system, pressurize it with nitrogen, seal it, and ship it off to the Cape.

But that's all down the road. At present, the GAMCIT team is still shopping for parts. Buying GAMCIT's skin and bones—stainless-steel stock, Kevlar sheets, and heavy-duty epoxy cement—will be fairly easy. Acquiring its guts and brains on SEDS's shoestring budget has been more challenging. For example, the 35-millimeter film magazine that holds the roll of 250 exposures costs $5,000—more than the camera itself, which is "only" $3,000. SEDS president Benjamin McCall, a junior in chemical physics, is still working on that one, but companies have been coming through with much of what's needed. Intel, for one, donated GAMCIT's microprocessor brain, plus a 386 computer and a host of supporting equipment for the design effort. Says Ratner, "Intel has been more than generous, and they've been just beautifully fast. They're angels." Some of the other sponsors include Rocketdyne, which provided $2,500 in start-up funds; Trimble Navigation, which contributed the GPS unit; Hewlett-Packard, which lent the project some $20,000 dollars' worth of oscilloscopes and other testing equipment; and Intégral Peripherals, which supplied the hard disks on which GAMCIT will record all its data. And just recently, Duracell agreed to donate the batteries.

Of course, there's no guarantee that GAMCIT's data will solve the riddle of the bursts' origins, but that's life. "These are the most mysterious objects in astronomy right now," says Schmidt, who became faculty advisor to the GAMCIT team after Grunsfeld's departure for the Johnson Space Center. "I can think of no other object where the uncertainty in distance is a factor of a million." □ —*DS*

Books



University of California Press, 1994
$30.00
318 pages

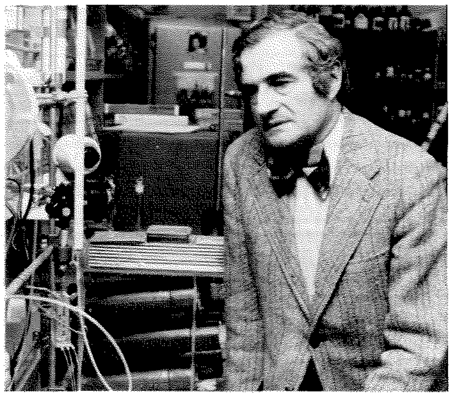
by Clyde A. Hutchison III

This autobiography of Robert L. Sinsheimer is the latest in the Science Book Series commissioned by the Alfred P. Sloan Foundation to foster public understanding of science. It will, of course, be of particular interest to readers of *Engineering & Science* because Sinsheimer spent 20 years at Caltech as professor of biophysics (from 1957 to 1977), and as chairman of the Division of Biology (1968–1977). It will also be of value to those concerned with the roots of molecular genetics. Sinsheimer's research on the bacterial virus ϕX174 (known affectionately as ϕX) pioneered many of the approaches we take for granted today. The book chronicles his contributions to science and to education, eloquently presents his philosophical viewpoint on these endeavors, and gives some glimpses into personal factors that have shaped a uniquely creative and productive career.

Sinsheimer describes his research on ϕX as "the centerpiece of my scientific career." The best known result of those studies was the discovery that the DNA of this virus is single-stranded, announced in 1959 in a startling paper in the first issue of *The Journal of Molecular Biology*. At the time, the discovery was as astonishing as "finding a unicorn in the ruminant section of the zoo." It is not, however, the unusual characteristics of ϕX, but rather the universally applicable features of the research that constitute his enduring legacy.

"Initially, we had only the rudimentary knowledge that ϕX was probably small and could grow in certain strains of *E. coli*. At the end of our research, we knew the complete sequence of its DNA and the details of its genetic structure . . ."

ϕX was selected as a research subject because of preliminary evidence that it was very small, even for a virus. In Sinsheimer's hands ϕX was the first bacterial virus to be purified to chemical homogeneity. Consequently, its DNA was the first viral DNA available in pure form for detailed physical and chemical analysis. This gave ϕX a unique position, which persisted for some 20 years, at the forefront in the development of molecular genetics. Many of the fundamental tools to link genetics and DNA chemistry were established using ϕX as the model of choice, first in Sinsheimer's lab at Caltech and then elsewhere, as ϕX and people trained to use it migrated around the globe. At Caltech, Fiers and Sinsheimer provided the first demonstration of a covalently closed circular DNA molecule—ϕX, of course. In Fred Sanger's laboratory in Cambridge, the ϕX genome was the first DNA to be completely sequenced. In 1978, the year after Sinsheimer decided to close his laboratory to accept the position as chancellor of the University of California at Santa Cruz, the first oligonucleotide-mediated, site-directed mutagenesis experiment was published, again using ϕX. The ϕX genetic system was ideal for this crucial step in the emergence of the new field of protein engineering. It is hard to overestimate Sinsheimer's central role in the birth of molecular genetics—through his establishment of the ϕX system, the many important



Robert Sinsheimer in 1974, when he was chairman of the Division of Biology.

discoveries made in his laboratory, and his influence on the many scientists that he trained.

I was particularly intrigued by the descriptions of the non-ϕX facets of Sinsheimer's scientific career and educational background. For example, his work during World War II on radar at the Radiation Laboratory at MIT occupies more space than the ϕX story. This early experience with "Big Science," before his graduate studies, stimulated his role in initiating the organized effort to sequence the human genome some 40 years later. I was also happy to have my memory refreshed concerning his fundamental contributions to nucleotide chemistry and the effects of radiation on nucleic acids.

When it was announced in 1968 that Robert Sinsheimer would become chairman of the Division of Biology at Caltech, those of us working in his laboratory were taken completely by surprise. He had previously appeared to be single-mindedly devoted to his research and teaching, with no apparent interest in administrative matters. In the mid-1970s when he publicly advocated a very cautious stance with respect to possible hazards of recombinant DNA research, I was even more puzzled. It was no surprise that he had a deep interest in the impact of modern biology on society. But his earlier writings, beginning in 1967, had seemed to view the prospect of mankind taking charge of evolution in a more positive light. By 1977 when he accepted the chancellorship at Santa Cruz, I was beyond being surprised. One reason I was eager to read *The Strands of a Life* stemmed from the hope of gaining some insight into these unexpected career transitions. It remains a bit mysterious, but I certainly learned a lot of interesting things about the circumstances of these events—about half the book is devoted to the Santa Cruz years.

One thing that comes clearly through the whole book is his love and respect for the scientific endeavor, only intensified by his encounters with the political arena of university administration:

> "Unlike anthropologists or economists, authors or poets, theologians or politicians, we natural scientists have the luxury of a single truth. There is only one proton mass, one periodic table, one genetic code. In consequence, science, during my career, has been essentially egalitarian. Nature is the only source of ultimate authority. Before nature, a world outside of man, a reality independent of human design or desire, we are all equal."

He concludes: ". . . I would love to return in a century or two to see where science stands and to learn what questions they are asking in the Sinsheimer Laboratory [at UC Santa Cruz]." When the technology to realize this dream becomes available, he certainly deserves to be high on the waiting list.

Clyde A. Hutchison III is the Kenan Professor of Microbiology at the University of North Carolina at Chapel Hill. He was a graduate student in the Division of Biology at Caltech where he did thesis research on the genetics of phage ϕX174 in the laboratory of Robert Sinsheimer, receiving his PhD in 1969.

Letters

Editor:

We thank H. Joel Jeffrey and Jay Labinger for their kind remarks about *The Golem* (*E&S*, Winter '94 and Fall '93). We are delighted that both writers think it is a useful account of science for scientists. What we say below is not meant to detract from our gratitude to Labinger for bringing the book to the attention of so many scientists in such a positive way. Nevertheless, we thought readers might be interested in the authors' view of Jeffrey's and Labinger's disagreement over the extent to which the book exhibits an unpalatably "social" view of science. In a word, we side with Jeffrey.

Labinger is correct in tracing the authors' origins to the movement known as "the sociology of scientific knowledge," and Jeffrey might well be uncomfortable with some of our previous work. It is the book that is being judged, however, not the authors. Jeffrey demonstrates through his correct reading that *The Golem* demands no allegiance to a radical viewpoint. Where Labinger suggests that we have "little interest in moderating [our] own positions in order to enlist scientists in true dialogue," he seems to have forgotten *The Golem* itself. Labinger's view of the book is, perhaps, influenced by his personal knowledge of some of the arguments we have had with our fellow observers of science.

The Golem is radical in its discussion of the relationship of science to other institutions, but it is very easy to demonstrate that it is not a radical book in its interpretation of what goes on in the laboratory. First, some of the accounts of passages of science in *The Golem* are taken from work by historians who have no sympathy with the sociology of scientific knowledge. The most noteworthy examples are the two stories relating to the theory of relativity (but Gerald Holton's history of Millikan's oil-drop experiment would have fitted equally well). That some conservative historians saw the same things as we did when they looked closely at passages of scientific controversy was one of our strongest motivations for putting the studies together.

Second, *The Golem* makes a strong case in favor of expertise. It says that we should recognize that there can be competing expertises but not that any opinion is as good as an expert opinion. As Jeffrey remarks, there is nothing in *The Golem* that suggests that the arguments described in the chapters were biased by anything other than honest beliefs.

Third, initial indications suggest that *The Golem* is being widely read and appreciated by practicing scientists and that the remarks of Pinch quoted by Labinger are already out of date.

There is, then, no need for us to "moderate our position" to make true dialogue possible; true dialogue is already possible. We would change only one thing about *The Golem*. Some readers have taken us to be claiming that the studies are statistically representative of the range of day-to-day activity in science. The studies were meant to be representative of controversial science; we think there is hardly any controversial science that does not follow the route described in *The Golem*. In a new preface to foreign editions and to the forthcoming paperback (Canto, fall 1994), we explain this more carefully.

Harry Collins, Director, Bath Science Studies Center, University of Bath, England

Trevor Pinch, Professor of Science and Technology Studies, Cornell University

Editor:

One of my prized recollections of Dr. DuBridge centers on a dialogue between "two freshmen." I was the freshman editor/publisher of the first post-World-War-II *little t* student handbook for the entering freshman class, summer of 1946–47. The book was incomplete without a welcoming message from the incoming president of Caltech, Lee A. DuBridge. After several rebuffs, I pleaded with the office in Throop to help me get in touch with him.

Dr. DuBridge was vacationing in Colorado en route to his new job and was not to be contacted. But finally I was given his telephone number. After several calls to the dude ranch where the DuBridges were staying, I made contact, and Dr. DuBridge graciously agreed to write a welcome for the handbook. His message read in part:

> "I am not sure whether it is proper, or possible, for one "freshman" to welcome another. However, I do take pleasure in extending on behalf of the California Institute of Technology, cordial greetings to all students entering the Institute for the first time this fall.
>
> "After all, we have much in common. You and I, together we must now take up new surroundings which are unfamiliar to us. We now become a part of one of the greatest institutions of its kind in the world. It is our privilege to help make it greater. We must discover its fine points and preserve them; uncover any weak points and make them strong. We take up these new tasks at a critical, but propitious time. The exigencies of war have thrown this institution, like all others, into a fluid state. We must see that the new pattern into which it crystallizes is an even better one—adequately adapted to new conditions."

These remarks display a forthright and friendly style, which became his hallmark in dealing with faculty, trustees, and students. He quickly established good relationships with students and maintained a permanent policy of accessibility to Caltech graduates as they moved on and progressed in science and industry.

To those who felt no one would be able to follow Robert "Uncle Bob" A. Millikan, Lee DuBridge was blessed with the perfect balance of humanity, understanding, and intelligence to encourage the growth of Millikan's child. The friendship that began that summer between two freshmen endured during my student days and in my many postgraduate contacts with Caltech and Dr. DuBridge.

Hugh C. Carter, BS '49

Random Walk

Roger W. Sperry 1914–1994

Roger W. Sperry, 1981 Nobel laureate in physiology or medicine and the Institute's Board of Trustees Professor of Psychobiology, Emeritus, died on April 17, 1994, of complications associated with muscular dystrophy. He was 80.

A native of Hartford, Connecticut, Sperry earned his bachelor's degree in English literature from Oberlin College in 1935, then focused his attention on psychology, earning his master's in that field in 1937, also from Oberlin. For his doctorate, he studied zoology, earning his degree from the University of Chicago in 1941. In 1954 he became the Hixon Professor of Psychobiology at Caltech, where he remained until his retirement from teaching in 1984. He is survived by his wife, brother, two children, and two grandchildren.

Sperry is best known for his "left-brain/right-brain" research, which won him the Nobel Prize along with David H. Hubel and Torsten N. Wiesel. He also received the National Medal of Science in 1989 from President George Bush, the Wolf Prize in Medicine and the Albert Lasker Medical Research Award in 1979, and the California Scientist of the Year Award in 1972, among many other honors.

A memorial service was held on June 3 in the Beckman Institute Auditorium, excerpts from which will be published in the next issue of *E&S*.

Honors and Awards

Clarence Allen, professor of geology and geophysics, emeritus, has received the California Earthquake Safety Foundation's Alfred E. Alquist Award for "achievement in earthquake safety and sustained leadership in the earthquake field."

Michael Alvarez, assistant professor of political science, has been awarded a 1994 Haynes Foundation Faculty Fellowship and a John M. Olin Faculty Fellow for the 1994–95 academic year.

Fred Anson, professor of chemistry and chairman of chemistry and chemical engineering, has won the Award in Electrochemistry, given to a member of the American Chemical Society's Division of Analytical Chemistry who has "uniquely advanced the field."

Pamela Bjorkman, assistant professor of biology and assistant investigator for the Howard Hughes Medical Institute, will receive a 1994 Gairdner Foundation International Award for her discovery, with Harvard professor Don Wiley, of the structure of a peptide-antigen complex that triggers an immunological response in the body. Bjorkman and Wiley are two of five to be so honored.

Lance Davis, Harkness Professor of Social Science, and Robert Gallman of UNC Chapel Hill have received the third Sanwa Award from the Center for Japan-U.S. Business and Economic Studies at New York University for their study of "International Capital Flows, Domestic Capital Markets, and Economic Growth in Four Frontier Countries."

Two Caltech assistant professors of biology have been chosen as Howard Hughes Medical Institute investigators. William Dunphy and Stephen Mayo join the 49 new and 225 current investigators nationwide who HHMI feels are "likely to make significant advances in biomedical research and to develop new approaches to overcoming disease."

The American Academy of Arts and Sciences has elected Robert Grubbs, Atkins Professor of Chemistry, and Shrinivas Kulkarni, professor of astronomy, to join its membership. Grubbs and Kulkarni are among the 210 honorees this year and among the 70 Caltech members of the Academy.

Hiroo Kanamori, Smits Professor of Geophysics and director of the Seismological Laboratory, has been awarded the 1993 Asahi Prize by the Asahi newspaper company in recognition of his "studies of the basic physics of the occurrence of earthquakes, and their applications to hazard reduction."

Wolfgang Knauss, professor of aeronautics and applied mechanics, will receive the Murray Medal, named for the society's first president from the Society for Experimental Mechanics.

Manfred Morari, McCollum-Corcoran Professor of Chemical Engineering, has received the Grössen Ehrenzeichen des Landes Steiermark, awarded by the Austrian state of Steiermark.

Ned Munger, professor of geography, emeritus, has been given a distinguished alumni award for public service by the University of Chicago, where Munger did his graduate work and later led programs in support of African and African-American universities and South African political change.

John Roberts, Institute Professor of Chemistry, Emeritus, is one of four 1994 Chemical Pioneers of the American Institute of Chemists, who cited his "major impact on advances in chemical science or industry."

Robert Rosenstone, professor of history, has received a Fulbright grant to teach a graduate seminar on "the historical film" at the University of Barcelona next year. He has also been awarded a 1994 National Endowment for the Humanities summer stipend to do a research project on films about the Spanish Civil War.

Philip Saffman, professor of applied mathematics, is the recipient of the 1994 Otto Laporte Award, presented by the American Physical Society. The award recognizes outstanding accomplishments in fluid dynamics research.

Erin Schuman, assistant professor of biology, is one of 100 "outstanding young scientists and economists" to be awarded a Sloan Research Fellowship.

David Stevenson, professor of planetary science and division chair for Geological and Planetary Sciences, has been presented with the 1994 Fred Whipple Award by the American Geophysical Union, given for outstanding contributions in planetology.

Kai Zinn, assistant professor of biology, is one of 14 scientists to receive a McKnight Neuroscience Investigator Award, which supports studies relating to the basic mechanisms of memory and the disorders affecting it.

In a White House ceremony on September 30, Hans Liepmann, von Kármán Professor of Aeronautics, Emeritus, received the National Medal of Technology from President Clinton, whose hair has scored the lowest coefficient of drag ever recorded for a sitting president. Liepmann, who won the National Medal of Science in 1986, is the eleventh person to get both medals.

Dervan Appointed Division Chair

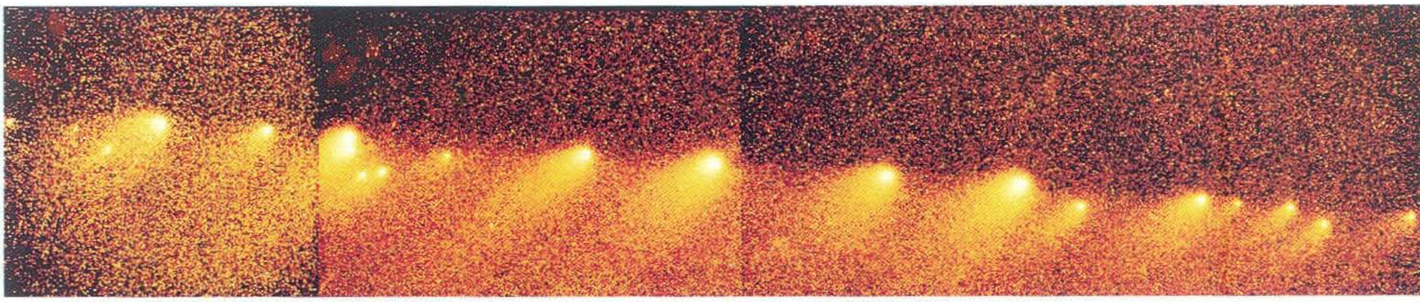
On July 1, Peter Dervan, the Bren Professor of Chemistry, will start his duties as the new chair of the Division of Chemistry and Chemical Engineering. He is replacing Professor of Chemistry Fred Anson, who is stepping down after 10 years as chair.

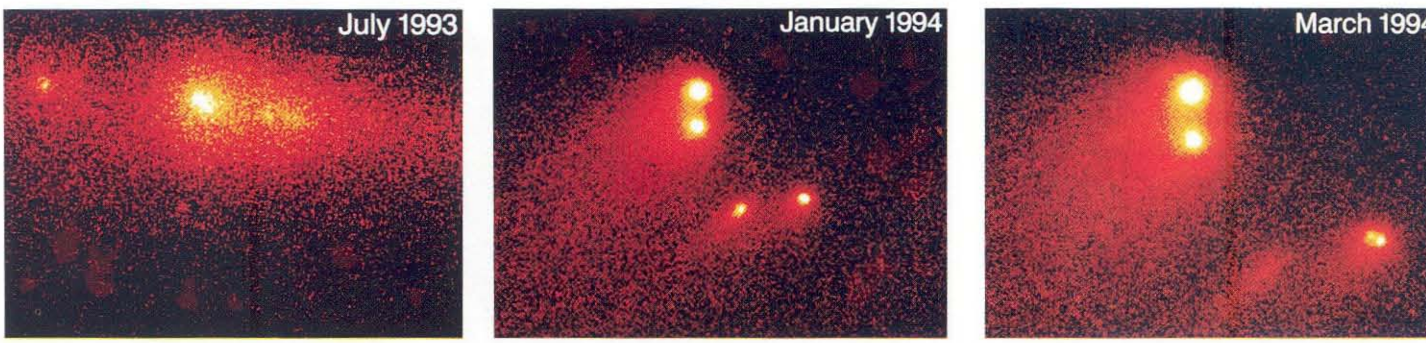
A graduate of Yale University, where he received his PhD in 1972, Dervan joined the Caltech faculty 21 years ago as an assistant professor. He was appointed associate professor in 1979, full professor in 1982, and was named to the Bren professorship in 1988.

Dervan's research pioneers the interface of chemistry and biology. His group is defining the principles for targeting single sites in the genetic material, DNA, by chemical methods. In 1993 the American Chemical Society recognized Dervan for his research and awarded him the prestigious Arthur C. Cope Award.

Dervan's numerous awards include the Willard Gibbs Medal and the William H. Nichols Medal. Well-known for his commitment to undergraduate teaching, Dervan received the highest honor given by Caltech undergraduates—the ASCIT teaching award in 1980 and 1981. Dervan is a member of the National Academy of Sciences, and is a fellow in the American Academy of Arts and Sciences.

Below: Shoemaker-Levy 9, as seen in false color by the newly repaired Hubble Space Telescope on January 24, 1994.





Above: Three shots of fragments Q (at center of each image) and P (at right). The pre-service-call July 1993 image revealed that Q was actually two hunks a mere .3 arc seconds apart—too close for ground-based telescopes to distinguish. The fainter P was just a blur, due to Hubble's spherical aberration. In January, 1994, Q's halves had drifted a full arc second apart, and Hubble's sharpened eye saw that P, too, had been twins all along. By March, P1 had faded into a puff of dusty vapor, and P2 had split in two. As the comet crumbles, the explosive power of the pieces dwindles. Fortunately, the other chunks have been hardier, at least so far.

Comet Crash Update

Periodic Comet Shoemaker-Levy 9 continues on its one-way trip to Jupiter (*E&S*, Fall 1993.) The train of comet fragments will reach the end of the line over a five-and-a-half-day span centered on July 19, 1994. The latest orbital calculations by Paul Chodas and Donald Yeomans of Caltech's Jet Propulsion Laboratory put the impact points a hairsbreadth beyond the horizon—three to eight degrees, as seen from Earth. This means that the upwelling fireballs should be visible over the horizon within minutes of impact, assuming that they rise a couple of hundred kilometers above the cloud tops, says Chodas, and the entry wounds themselves will roll into view some ten minutes after impact. Depending on the fragment size and the impact model used, this is close to the plume's predicted lifetime.

The question is how opaque the plume will be after rising 200 kilometers, or, if you're an infrared astronomer, how hot it will be. Recent calculations by graduate student Toshiko Takata, Visiting Associate in Planetary Science John O'Keefe, JPL's Glenn Orton (PhD '75), and Professor of Geophysics Thomas Ahrens (MS '58) indicate that a two-kilometer-diameter fragment could create a plume with a visual magnitude as high as −2, roughly the magnitude of Jupiter itself; a 400-meter chunk could be +0.3, comparable to such bright stars as Betelgeuse or Arcturus. In the former case, assuming the plume peeps over the horizon, Jupiter would suddenly flare to twice its normal brightness as seen from Earth. But in a final fit of perversity, all of the largest fragments are slated to hit either during the day or after Jupiter has set as seen from Pasadena, Chodas says.

The July issue of *Sky & Telescope* has 18 pages devoted to the event, including tips for amateur observers and a complete table of Chodas and Yeoman's impact times, plus eight pages of artist's conceptions for those of us who are willing to take someone else's word for it.

Right: The bad news is that Shoemaker-Levy's fragments will vanish from our sight just seconds before impact.

Below right: The good news is that Galileo is guaranteed to see the whole show, although reading its reviews at 10 bits per second (due to the antenna problem) will tax the most patient JPL engineer—and you thought your fax machine was slow!

Below: Shoemaker-Levy's orbit and fragment train, drawn to scale with Jupiter, as seen from Earth. (The train's width has been exaggerated for clarity.) Jupiter is the slightly thicker dot at the upper bend in the dotted line. By collision time, Shoemaker-Levy's fragments will stretch across five million kilometers.