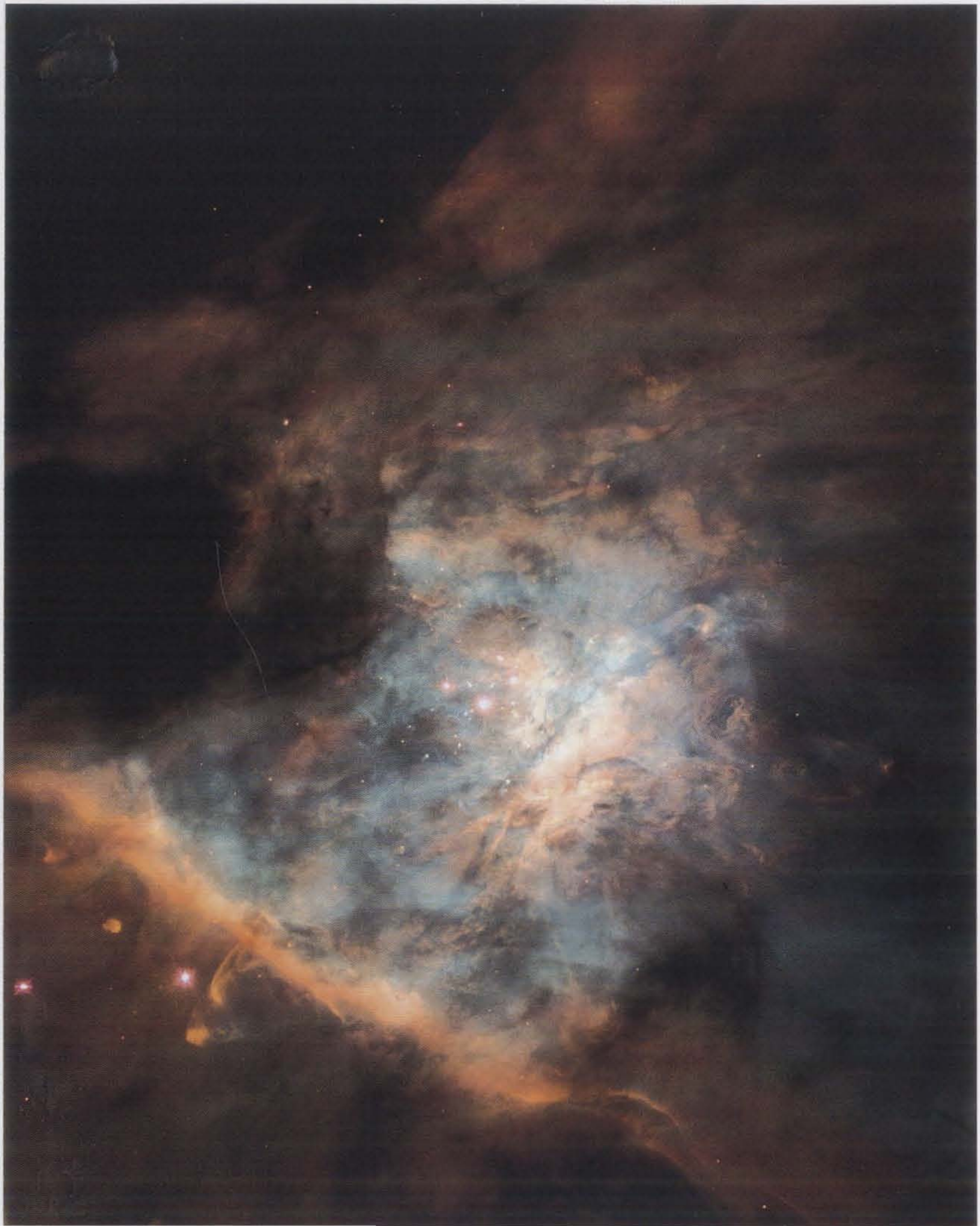
California Institute
of Technology

Engineering & Science

**Volume LIX, Number 1
1996**

In this issue



This panoramic view of the central region of the Orion nebula was assembled from 45 separate Hubble Space Telescope images by C. R. O'Dell of Rice University. The image covers a region of sky about five percent of the area of the full moon. The nebula, a churning maelstrom of gas and dust, is lit up by ultraviolet light from the four hot young stars—collectively called the Trapezium—at the center of this image. The nebula contains some 700 stars in various stages of formation. At least 150 of these stars are immersed in disks that we believe are nascent planetary systems. Those closest to the Trapezium are shedding material under the intense pressure of the starlight, forming outward-pointing tails. When the Keck interferometer is completed, it will be looking at systems like this in hopes of actually seeing planets. For more on what the Keck Telescope is up to, see the story on page 8.

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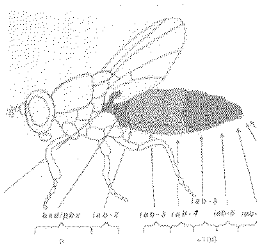
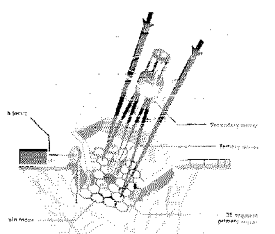
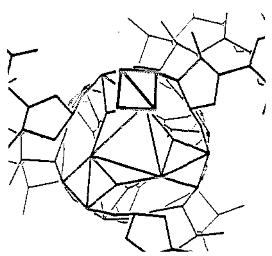


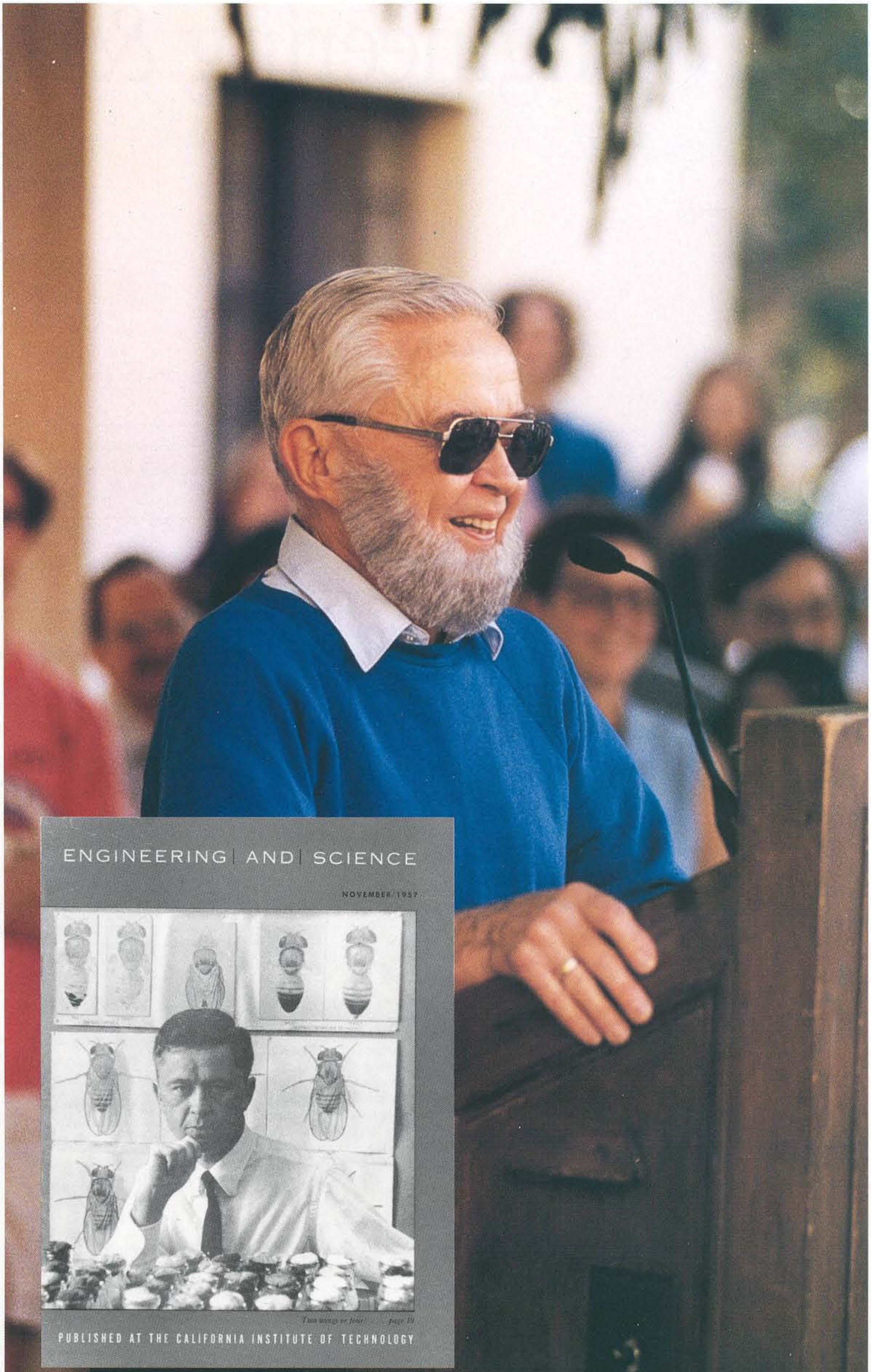




On the cover: Ed Lewis has been cross-breeding mutated *Drosophila* since he was a teenager in the 1930s. Here, after receiving the Nobel Prize for the insights gleaned from this work, he poses in his lab with the latest of countless fruit-fly generations.

Engineering & Science (ISSN 0013-7812) is published quarterly at the California Institute of Technology, 1201 East California Boulevard, Pasadena, California 91125. Annual subscription $10.00 domestic; $20.00 foreign air mail; single copies $5.00. 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, 5, 7, 36, 37, inside back cover – Bob Paz; inside front cover, 41 – STScI; 4, 22, 25, 28 – Caltech Archives; 5, 6 – Sue Celniker; 8, 13, 14, 16–19 – Keck Observatory; 10 – Huntington Library; 10, 18 – George Djorgovski; 11 – Steve Landy; 12, 20 – CARA; 12 – Russ Underwood; 15 – Josh Barnes, Lars Hernquist; 15 – CTIAO/AURA; 15 – NRAO/ASP; 15 – HST/ASP; 18 – ESO; 20 – Donna Womble; 20, 41 – Doug Smith; 21, 43 – NASA/JPL; 30 – A. C. Barrington Brown; 42, 43 – Air Force/JPL; 44, inside back cover – White House


ENGINEERING AND SCIENCE
NOVEMBER/1957
PUBLISHED AT THE CALIFORNIA INSTITUTE OF TECHNOLOGY

Edward B. Lewis
Nobel Laureate 1995

In a remarkably consistent career, Lewis has worked with Drosophila *for 60 years, continuing a tradition that had been brought to Caltech by Thomas Hunt Morgan.*

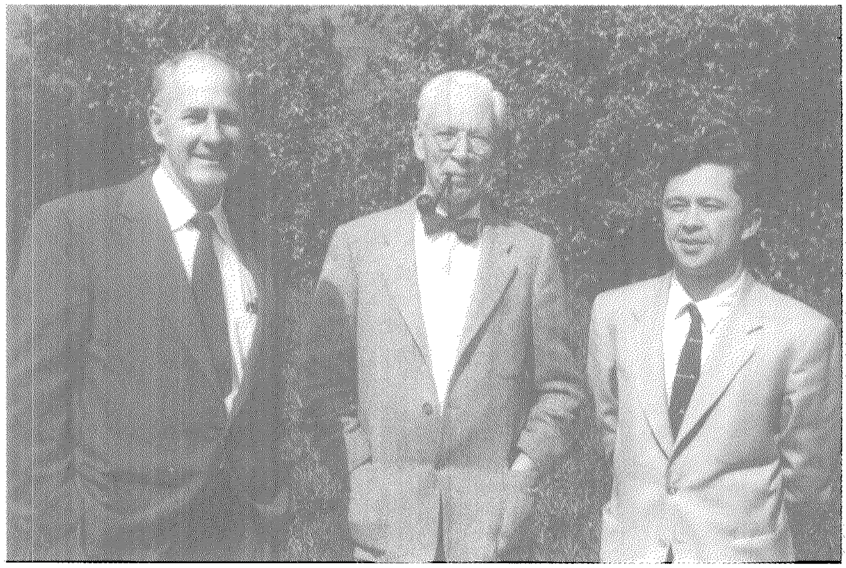
It's not Santa Claus, but Ed Lewis, just returned (incognito) from Switzerland where a happy Caltech crowd was waiting to help him celebrate his Nobel Prize. The inset shows Lewis on the cover of the November 1957 *E&S*.

Edward B. Lewis appeared on the cover of *E&S* once before—in November 1957, looking much younger, of course, but still surrounded by the jars of the fruit flies known to biologists as *Drosophila melanogaster*. In the cover article, a mere three pages long, entitled "Two Wings or Four?", Lewis described the discovery of a group of *Drosophila* genes called the bithorax complex and the construction of a strain of mutant flies with four wings instead of the usual two. The 1957 article concluded: "We now have a working model for picturing the genetic control of development. Whether it is the correct model or not remains to be seen. In pursuing that model, however, we should make progress in our understanding of the living organism."

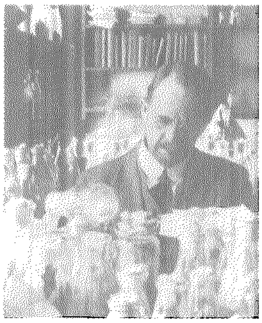
Lewis's model was indeed correct. And the progress he made over the next decades "in our understanding of the living organism" won him the 1995 Nobel Prize in physiology or medicine. Lewis shared the prize with Christiane Nüsslein-Volhard of the Max Planck Institute in Tübingen and Eric Wieschaus of Princeton University who, according to the official release from the Nobel Committee, "were able to identify and classify a small number of genes that are of key importance in determining the body plan and the formation of body segments." Lewis was cited, in part, for discovering "how genes were arranged in the same order on the chromosomes as the body segments they controlled." "Together," said the committee, "these three scientists have achieved a breakthrough that will help explain congenital malformations in man."

Although other discoveries in the past dozen years have shown the relevance of Lewis's research to medicine, he has worked only with flies. In a remarkably consistent career, Lewis has worked with *Drosophila* for 60 years, continuing a tradition that had been brought to Caltech by Thomas Hunt Morgan. Morgan was the first to use *Drosophila* for genetic studies; he began working with this organism at Columbia University in 1908. Soon this tiny fly, with its 10-day life cycle, became the most famous experimental animal in the world, and by breeding generation after generation of flies, Morgan and his students established that genes, located on the chromosomes, are the units of heredity. In 1928, at the persuasive invitation of Caltech's Robert Millikan, Morgan moved to Pasadena with his stocks of flies and his whole research group—a group that included Calvin Bridges and Alfred Sturtevant, who in 1911 had made the discovery that genes are arranged on a chromosome in linear order like beads on a string. Morgan, who won the Nobel Prize in 1933, recognized the contributions of Bridges and Sturtevant by generously dividing the prize to support the education of their children.

Lewis remembers Morgan from the early forties as someone by then no longer very active in the laboratory, but whose legacy had already imprinted itself on Caltech's biology division. "Morgan didn't like any speculation that smacked of mysticism," Lewis remembers. "Instead, he and his students carried out simple, clean experiments designed to test specific hypotheses." Lewis has clearly followed in the same "reductionist" tradition.



Thomas Hunt Morgan (below) founded *Drosophila* research, as well as Caltech's biology division, of which he was its chairman until 1946. (Fruit-fly bottles looked pretty much the same 80 years ago.)
Right: George Beadle, (on the left) who succeeded Morgan as chairman, with Alfred Sturtevant, and Ed Lewis circa 1960.



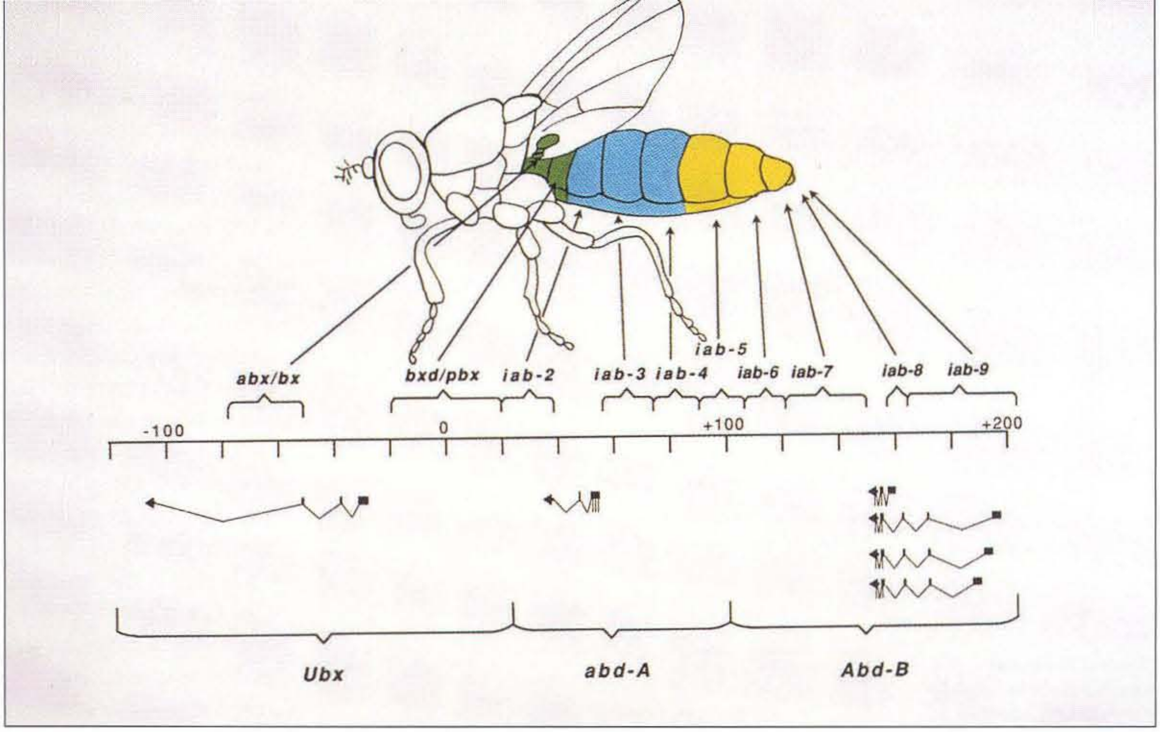
The laws of genetics had never depended upon knowing what the genes were chemically and would hold true even if they were made of green cheese.

When Morgan was collecting *his* Nobel Prize, Lewis, the son of a watchmaker in Wilkes-Barre, Pennsylvania, was a freshman in high school. There happened to be a good public library in Wilkes-Barre, where Lewis discovered in its one scientific journal, *Science*, an ad offering stocks of *Drosophila* from Purdue University. In 1934 Lewis and his friend Edward Novitski (now professor of genetics, emeritus, at the University of Oregon), ordered some and cultured them in their high-school biology laboratory. Novitski carried on a correspondence with Professor Rifenberg at Purdue, where he eventually went to college, and also with Calvin Bridges at Caltech, who sent the young men free batches of flies.

After a year at Bucknell University on a music scholarship (he still plays the flute), Lewis transferred to the University of Minnesota, where he encountered Professor Clarence P. Oliver, who, in the Morgan tradition, "gave me a desk in his lab and complete freedom to carry out *Drosophila* work. I was working in the lab whenever I could, although it wasn't for course credit. You wouldn't expect this to happen at a big university, but it did." It was here that Lewis began work on a rough-eyed mutation, first called 'star-recessive,' but later renamed 'asteroid.' Novitski had found the mutation at Purdue and sent it to him. These mutations did not quite behave like a series of mutations of the same gene, as would be expected. Lewis graduated from Minnesota in two years (1939) with a degree in biostatistics (because zoology would have taken another year). Then he was awarded a teaching fellowship at Caltech. Lewis chose as his adviser Alfred Sturtevant, who, like Morgan, encouraged his students to go their own way. Lewis continued with his work on the rough-eyed flies: "It looked as though the gene might be either subdivisible, or, the way we really interpreted it, the gene was really a cluster of genes that acted like a single one."

After earning his PhD in 1942, Lewis studied meteorology at Caltech, as an Army Air Force cadet, and oceanography in a crash course at UCLA. After a stint of forecasting weather at Hickam Field on Oahu, he was assigned to the Tenth Army and shipped out to Okinawa in April of 1945, where he lived aboard one of the command ships that had the necessary weather data, "such as it was." In 1946 he returned to Caltech, where Millikan had promised him there would be a job waiting. Little had changed at Caltech. "It was much the same as when I was a graduate student," said Lewis, "because the same faculty members were still here. I came back as an instructor. There was always freedom to do research and a lot of interaction with faculty, not only in biology."

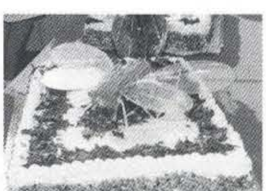
Lewis considered it a lucky time, a golden age. "I feel sorry in a way for young people who come in now. There's so much to learn and so much competition. The era of 'big science' was just getting off the ground then. Everything seemed exciting, all the problems. They also seemed beyond solution. That sounds contradictory—to be excited about something that's beyond solving. We didn't know what the genes were, so we tried to deduce how they worked from purely genetic experiments. Genetics is an



The genes of the bithorax complex regulate development of the fruit-fly's posterior half; these genes (*abx/bx* through *iab-9*) are lined up on the molecular map in the same order that they are turned on in the fly. They fall into three functional domains, color-coded here on the fly's body, each containing a highly conserved segment of DNA called a homeobox. They're named *Ubx* or *Ultrabithorax* (green); *abd-A* or *abdominal-A* (blue), and *Abd-B* or *Abdominal-B* (yellow).

abstract subject, which allows one to deduce many properties of the genes without any knowledge of what the genes are made of. Actually, the dogma of the time was that they were proteins, but this didn't help, and in fact was completely wrong."

Drosophila genetics had many advantages then, and now, according to Lewis. "There was an immense background of information available as well as hundreds of mutants. All of the obvious things had been done by then, so you could go into greater depth of analysis than you could in any other organism. You could begin to try to see how a gene is constructed, even though DNA hadn't yet been determined to be the hereditary material. The laws of genetics had never depended upon knowing what the genes were chemically and would hold true even if they were made of green cheese."



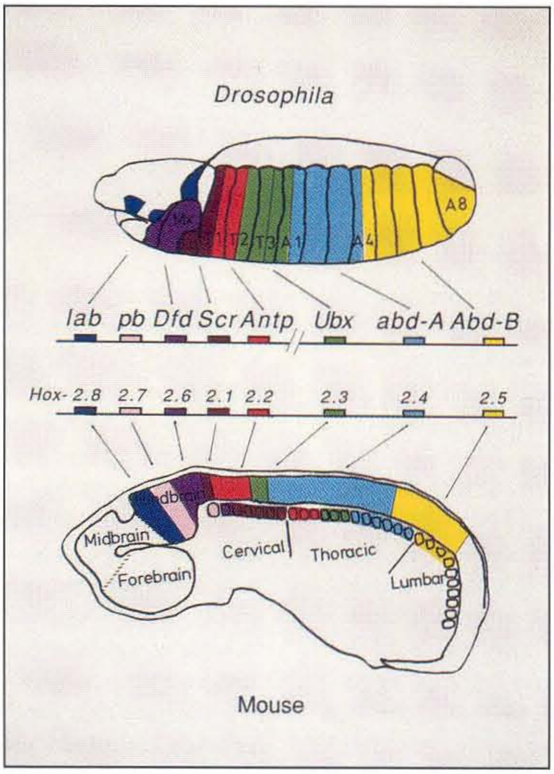
The four-winged fruit fly, born in the 1950s, made the *E&S* cover in 1981 and the Nobel Prize cake in 1995.

Returning to his original pre-war hypothesis for the origin of new genes, Lewis now found a gene cluster that at first appeared to be a single gene but turned out to be a group of very closely linked genes. These genes determined the development of the posterior half of the *Drosophila* fly—part of the thorax and the entire abdomen—and were indeed the very cluster he had been looking for. He named it the bithorax complex.

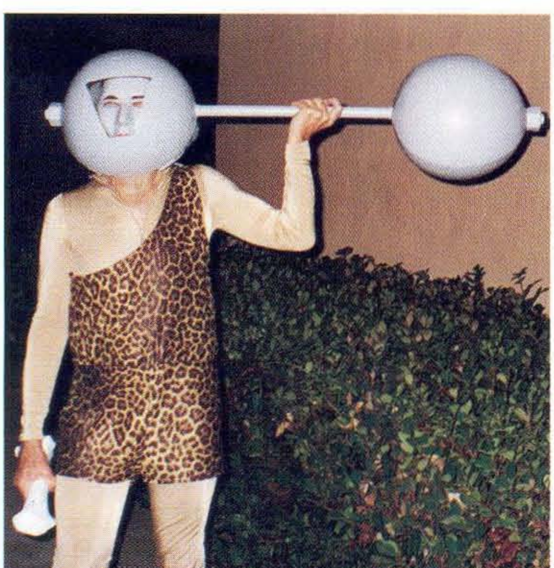
This is where the four-winged fly came in. Using x-rays to induce mutations in the bithorax complex, Lewis constructed a strain of fruit flies with four wings, the second set of which actually resulted from a duplication of the thoracic segment. Although the four-winged fly became the most visible symbol of the bithorax complex (it graced another *E&S* cover in 1981 and, created in frosting, the cake at Lewis's campus Nobel celebration), it was really just a "stunt," according to Lewis. "It wasn't connected to the theory," says Lewis. "It was just a byproduct of the theory that we were testing."

The genes of the bithorax complex are called homeotic genes, a word coined more than a hundred years ago to mean a type of variation in which "something has been changed into the likeness of something else." Calvin Bridges found the first homeotic gene in 1915, and hypothesized that gene duplications occur naturally and in tandem. Lewis carried the idea further to theorize that the original gene maintains its old function, while the copied gene takes on a new role. Lewis speculated that all these genes were descended from an ancient homeotic gene as the result of a series of duplications and diversifications by mutations. This theory provides an elegant and simple mechanism to explain how simple forms of life evolve into more complex ones. It also yielded a dividend: with the bithorax complex, Lewis had found a critical group of functionally related developmental genes—genes that control how an organism develops from egg to adult. He has stuck with this system ever since, trying to learn everything that these genes can reveal about this process.

Over the next few decades Lewis experimented on the bithorax complex, painstakingly knocking out genes with x-rays, cross-breeding mutated flies for hundreds upon hundreds of generations to discover which body parts were controlled by which genes. Lewis's exhaustive analysis of mutations in the bithorax complex spelled out how normal embryonic development can go awry. And he found some extraordinary things. "We discovered that during early development, the genes control how the body segments develop in a hierarchical manner. The closer a body segment is to the posterior of the organism, the more genes of the complex are turned on, always in an order that apparently coincides with the order of the genes in the chromosome. This law seems to hold as well for the homeotic complexes in vertebrates, including human beings." In *Drosophila*, the fact that more and more genes are turned on toward the fly's posterior end means that the abdomen is the most highly developed part of the fly. Lewis calls this the "abdomenization" of the organism, a phenomenon, he likes to tell in public lectures, that is probably familiar to older members of the audience. (Lewis, 77, stays trim with daily swims in the Caltech pool.)



Homeotic gene expression is highly correlated among species, as can be seen here in the fruit-fly embryo (top) and mouse embryo (bottom). The fly and mouse genes are arranged in clusters: the mouse has four, only one of which is shown, while two clusters control the development of the fly—the Antennapedia complex (red through purple) determines the fly's anterior end and the bithorax complex (green through yellow), the posterior end. The homeobox sequences of the two species are very similar.



Winning the Nobel Prize can't keep Lewis from the *really* important things, like the graduate students' Halloween party at Prufrock House. This is indeed Lewis in the leopard skin peering out of the barbell; he's dressed as a René Magritte painting called *Perpetual Motion.*

In the 1980s other researchers isolated a similar cluster, which they named Antennapedia, that performs the same function for the anterior part of the fly. (Its name comes from a mutation that causes legs instead of antennae to sprout from the fly's head.) Together, these two gene complexes, which represent less than one percent of the fly's total complement of genes, play a regulatory role far out of proportion to their numbers. They are the architects of the body plan, which tell all the parts how and where to form. Then, Walter Gehring and his colleagues at the University of Basel (Switzerland) and Matt Scott and A. J. Weiner, while in Thom Kaufman's lab at Indiana University, independently isolated a short sequence of DNA—only 180 base pairs long—from both the bithorax and Antennapedia complexes. Named the homeobox by Gehring, this DNA fragment was used to identify similar complexes in other organisms, including human beings.

The ubiquitous homeobox is so highly conserved among so many animals, commencing with the most primitive worms, that it is now a powerful marker for tracing evolutionary lineages throughout the animal kingdom. Its discovery also substantiates the theory that the homeotic gene clusters arose by a process of tandem duplication, lending intriguing credence to Lewis's original theory that all the homeotic genes were descended from one ancient gene. Such an ancestral gene would have left its trace as a single conserved fragment of DNA—like the homeobox.

Although he never abandoned *Drosophila* during all the intervening decades, Lewis did

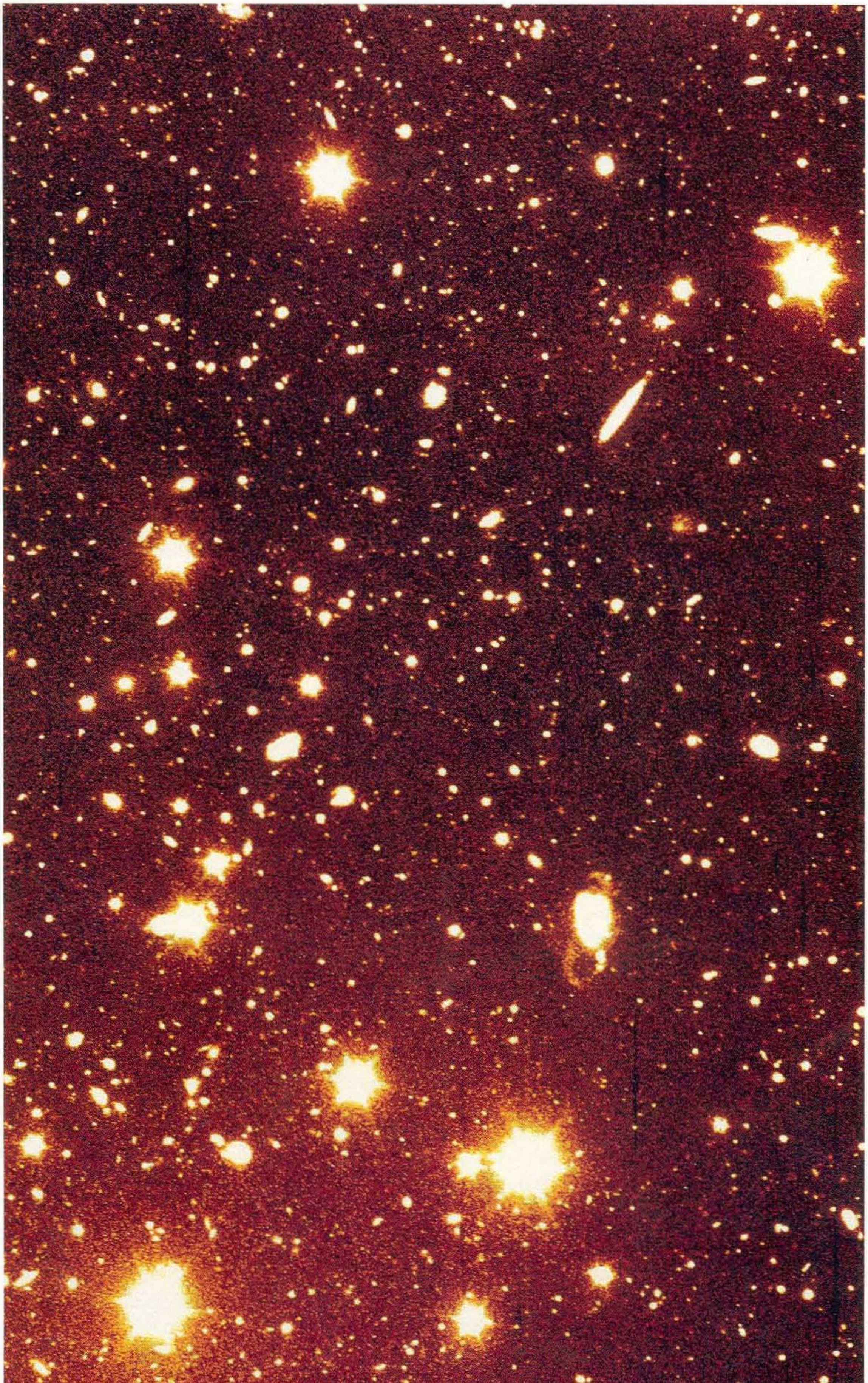
Sans beard, Lewis enjoys his homecoming party with colleagues (from left) Norman Horowitz and Herschel Mitchell, both emeritus professors of biology, and with Annamarie Mitchell. At far right, Lewis arrives at the festivities with his wife, Pamela.

make a deviation to put his knowledge to service in some of the controversial public health issues of the fifties. From using x-rays to create specific genetic mutations, it was only a small jump to the suggestion that x-rays could also cause mutations in body cells that could lead to cancer. This hypothesis had first been advanced in 1928, again using *Drosophila*, by H. J. Muller, another student of Morgan's. (He later won the Nobel Prize for demonstrating that x-rays can induce mutations.)

In 1957, in a paper in *Science*, Lewis showed that there is a linear relation between the amount of exposure to radiation and the incidence of human leukemia down to doses as low as 50 rads. He was called to testify before the Joint Congressional Committee on Atomic Energy to present his findings. Although the idea that cancer is caused by mutations is accepted now, it was very controversial at the time, says Lewis. Physicians weren't trained in genetics, and geneticists were reluctant to consider the effects of mutations on body cells. As a result of his landmark paper, he was appointed to the National Committee on Radiation, an advisory committee to the U.S. Public Health Service; later he served on committees of the National Academy of Sciences on biological effects of ionizing radiation, and on the National Council of Radiation Protection. In the late fifties Lewis also helped stop an experiment to inject radioactive tritiated water into the Los Angeles basin's groundwater to track its route.

Meanwhile, Lewis had been promoted to full professor in 1956. In 1966 he was named the Thomas Hunt Morgan Professor of Biology, when Sturtevant, who first held the chair, retired. "It had something to do with maintaining the *Drosophila* tradition," says Lewis, "and I was the only full-time faculty member doing *Drosophila*." In an article, "Remembering Sturtevant" published this month in *Genetics*, Lewis describes Sturtevant's fascination with pedigrees, perhaps not unusual for a geneticist, which had inspired Sturtevant to compile an intellectual pedigree of his own. "Sturtevant, of course, was a direct descendant of T. H. Morgan and of E. B. Wilson, another eminent biologist who was a contemporary and friend of Morgan's at Columbia. Morgan and Wilson were, in turn, direct descendants of Martin and Brooks, two men who were at Johns Hopkins University where Morgan had obtained his doctorate; Martin was descended from T. H. Huxley and Brooks from Louis Agassiz; and so it went."

Lewis himself is shown in these pedigrees as a direct descendant of Sturtevant and Muller. At Lewis's homecoming celebration (he was en route to a conference on homeotic genes in Switzerland when the Nobel Prize was announced), almost every speaker evoked the continuing *Drosophila* tradition at Caltech and the contributions of Morgan, Bridges, and Sturtevant in founding modern genetics. Lewis himself paid tribute to their "enormous insight and intuition." But it was Lewis's own insight and intuition to which this Nobel Prize pays tribute, as well as to his single-minded and almost single-handed dedication to a line of basic research in classical genetics that continues to yield remarkable insights in an age of 'big science.' □ *—JD*



Science with the Keck Telescope

By counting galaxies according to their color and apparent brightness in these deep, deep reaches of the universe, we can actually test models of cosmology.

by S. George Djorgovski

This may be the deepest visible-light image ever obtained—a portion of a field from a visible-light deep-galaxy survey being undertaken at the Keck Telescope by Caltech astronomers. Virtually every dot you see here is a galaxy. The complete image, which covers an area of sky about one-fifteenth that of the full moon, contains about 6,000 galaxies.

Wonderful things are happening in astronomy today, and the Keck Telescope is right in the thick of them. With its 10-meter-diameter primary mirror, it is the largest optical telescope in the world. The Keck is a joint venture between the University of California, Caltech, and the University of Hawaii, and is located in an astronomical preserve on the summit of Mauna Kea, Hawaii. A second Keck Telescope is now under construction there, thanks to the continued generosity of the W. M. Keck Foundation.

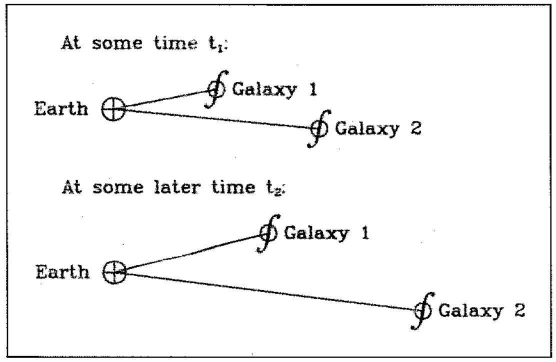
Before I describe what we're doing with the Keck, let's begin with a quick summary of the universe as we know it. Our star the sun, the earth, eight other major planets, and lots of moons, comets, and assorted other things form our solar system. The sun and about a hundred billion other stars, along with some gas and so on, form our galaxy—the Milky Way. Galaxies like to come in groups and clusters—for example, Andromeda, also known as M31; the Milky Way and its satellite galaxies, which include the Magellanic Clouds; and other galaxies form what's called the Local Group. Groups and clusters tend to agglomerate into superclusters, which are the largest structures we know. The center of the Local Supercluster, of which we are a member, is about 50 million light-years away in the Virgo Cluster. (A light-year is the distance that light travels in a year, about 5.88 trillion miles.) The light now reaching us from the outskirts of the Local Supercluster started on its way more than 70 million years ago, when dinosaurs were still walking the earth.

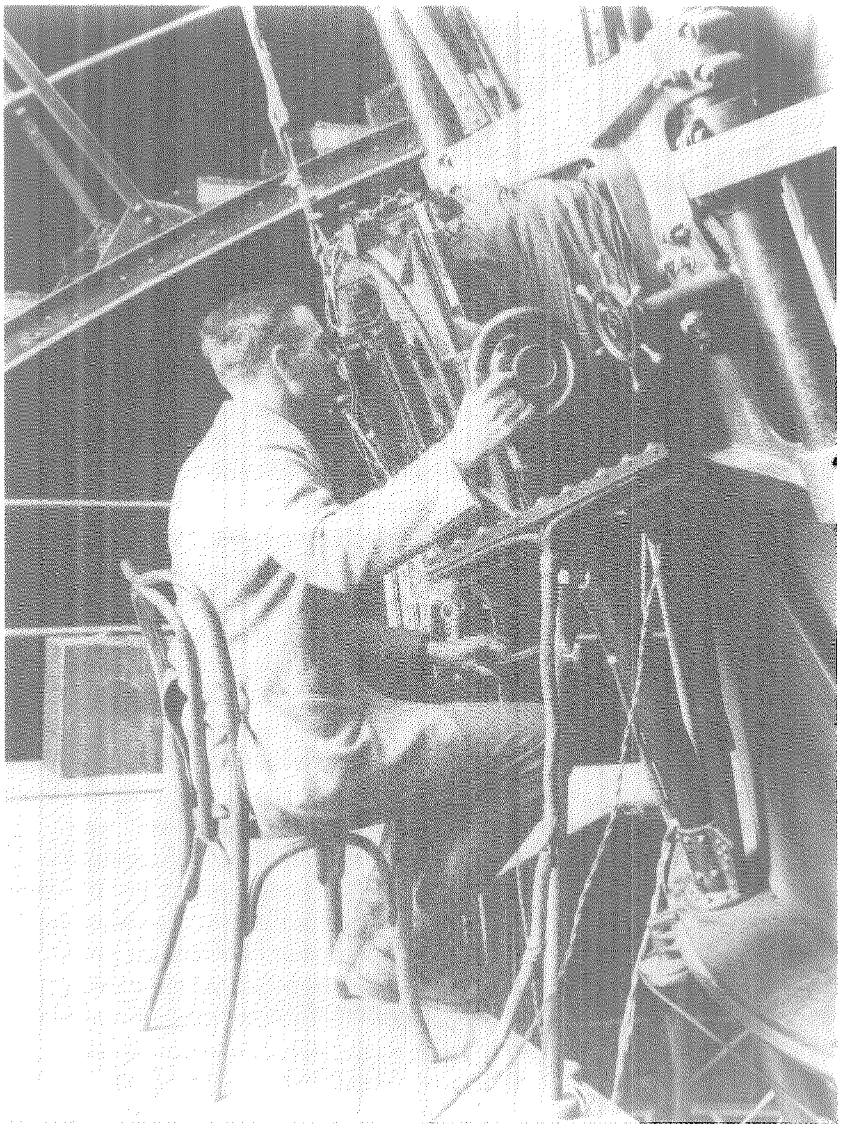
Let me try to illustrate these distances. Imagine that you shrank the earth down to a small grain of sand, about one one-hundredth of an inch in diameter. Then the sun would be an inch across and five feet away, and the solar system would be about a fifth of a mile across. The nearest star would be about 260 miles away, almost all the way to San Francisco, and our galaxy would be six million miles across. The next nearest galaxy would be 40 million miles away. At this point, you begin to lose scale, even with this model—the nearest cluster would be four billion miles away, and the size of the observable universe would be a trillion miles. If you were to ride a taxi across it at five dollars per mile, you could pay off the national debt.

Observational cosmology—the study of the universe at large—began in the 1920s, when Edwin Hubble, the Pasadena astronomer for whom the Space Telescope is named, discovered that he could see individual stars pulsating in the Andromeda Nebula, as it was then called. Such stars, called Cepheid variables, had been studied in the Milky Way, and their pulsation rate was known to depend upon their brightness. Hubble figured out from the apparent brightness of those within Andromeda that they were very, very distant; they were so faint that, if Andromeda contained them, it must be a galaxy just like our own. Until then, it hadn't been clear what these nebulae were. Were they clouds of gas in the Milky Way? Was the Milky Way the entire universe? Hubble's discovery made the universe much, much larger than people had supposed.

This was a big discovery all right, but it pales beside his next one. Several astronomers, espe-

Below: Edwin Hubble at the Mount Wilson Observatory's 100-inch telescope. Right: As the universe expands, the galaxies within it move away from us. The universe is expanding everywhere at once, so more distant galaxies appear to be moving away faster than nearby ones because there's more of the expanding universe between us and the farther galaxies. Thus measuring the apparent velocity of a galaxy gives its relative distance. (These distances must be calibrated against things whose distances we know, but that's another story.)

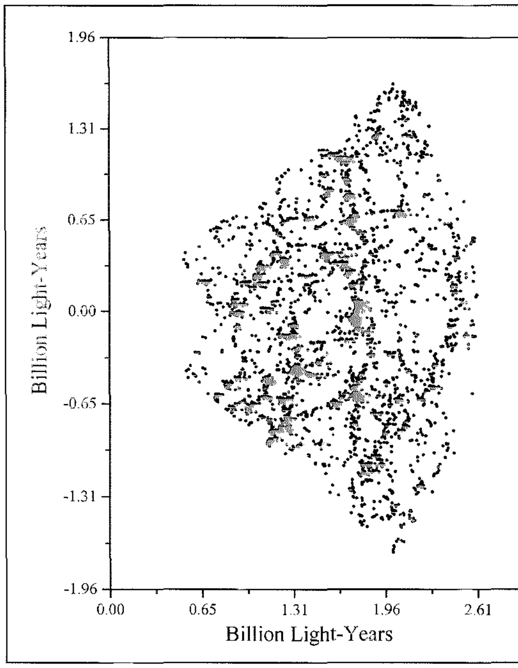




cially Vesto Melvin Slipher, had found that those nebulae were moving away from us at different speeds. Hubble and his colleague Milton Humason, using the 100-inch telescope at the Mount Wilson Observatory, discovered that the farther away a galaxy seemed to be, the faster it was receding. Hubble's interpretation of that observational fact was that the universe as a whole was expanding and carrying the galaxies with it, as shown in the diagram above. If you look at two galaxies, and then look again later, each galaxy's apparent velocity will be its change in distance divided by the time between looks. Galaxy number two—the more distant one—will have moved farther away, relatively speaking, than galaxy one, and so will appear to be moving away faster. You can simulate the expanding universe with the surface of a balloon. As the balloon inflates, every point on its surface gets farther away from every other point, and the farther away they are, the faster they move apart. You can continue this until infinity, or, in the case of the simulation, until the so-called "little bang" comes into play.

One can hardly make a bigger discovery. Some years earlier, Albert Einstein had developed his general theory of relativity, and he had had to introduce a fudge factor in it—the so-called cosmological constant—in order to prevent the universe from expanding. (At that time, everyone assumed it was static.) When Einstein visited Pasadena in the 1930s, Hubble took him to Mount Wilson and showed him the fleeing galaxies. Einstein was very impressed. He later said that the cosmological constant was the big-

Right: This fan-shaped slice of the universe—80 degrees wide by 1.5 degrees thick—contains 3,754 galaxies, and is part of a mapping project by Stephen Shectman and Stephen Landy at the Carnegie Observatories. (We're where the fan's hinge would be.) The entire map to date contains six slices and some 26,000 galaxies. This plot assumes that the Hubble constant (a measure of how fast the universe is expanding) is 50, making the farthest galaxies in the survey 2.6 billion light-years away.



gest blunder of his life. Einstein was a pretty good theoretical physicist, but he failed to make the greatest prediction of his career—that the universe is expanding.

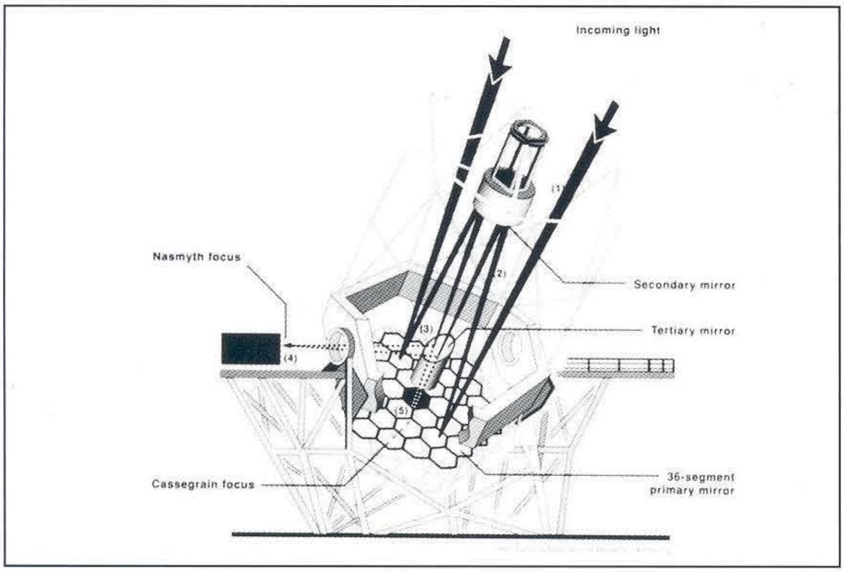
The discovery that the distance to a galaxy is proportional to its apparent velocity is now known as Hubble's law, and it is fundamental to cosmology. We need to measure the distances to galaxies in order to figure out many of their important quantities, such as luminosity, age, diameter, and mass. But distances are very hard to measure. On the other hand, velocities are relatively easy to measure from the so-called Doppler shift, or redshift. As a source of light moves away from us, the light actually stretches, and its wavelength gets longer. Thus its color shifts toward the red—which has longer wavelengths—and, the faster the source is moving, the larger the shift. Starlight can be broken down into patterns of spectroscopic lines—specific wavelengths that are absorbed or emitted by various chemical elements. People have measured these emission and absorption lines in labs here on earth, so we know exactly at what wavelengths the lines occur. Thus, when the light from a distant galaxy displays a known spectral pattern but at a set of wavelengths different from what we have observed in the lab, we can measure how much the light has been shifted. This gives us a proxy for how far away the source is, so that, by measuring the velocities of galaxies one by one, we can start to map the universe. Amazing structures are revealed—filaments composed of many thousands of galaxies enclosing voids a couple of hundred million light-years across.

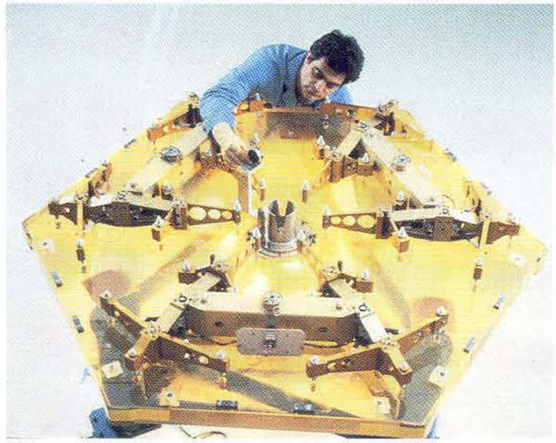
Astronomers measure the relative intensity of light with a perverse system called magnitude, and the reason we do this is twofold: one is to keep physicists out of the field, and the other is to torture astronomy students with homework problems.

About 100 billion galaxies are estimated to exist in the universe, and that's just a lower limit extrapolated from what we can see.

There is another thing I have to introduce, and that's the so-called magnitude scale. Astronomers measure the relative intensity of light with a perverse system called magnitude, and the reason we do this is twofold: one is to keep physicists out of the field, and the other is to torture astronomy students with homework problems. The magnitude scale was introduced by ancient Greeks, who said that the brightest stars are first magnitude, and the faintest ones you can see with the naked eye are sixth magnitude. The system was quantified in the 19th century, and now each magnitude is the fifth root of 100—about two and a half times—dimmer than the preceding one. The faintest objects we can detect with Palomar's 200-inch telescope are about 25th magnitude. (That's equivalent to a 25-watt light bulb—the kind you find in your oven—seen from a million kilometers away.) The Keck Telescope can see objects 20 to 50 times fainter. You need to collect lots of light in order to detect things so very faint, and that's why we need big telescopes. George Ellery Hale, the founding father of Mount Wilson, Palomar, and Caltech always wanted more and more light. Hale would be proud of the Keck Telescope.

The Keck is a revolutionary design. Instead of making one huge mirror, which is no end of pain, Jerry Nelson (BS '65), now a professor at UC Santa Cruz, decided to make 36 smaller pieces of mirror—a much easier job—then cut them into hexagons and combine them to make the surface





Above: The Keck Telescope. Incoming starlight (1) bounces off the primary mirror toward the secondary mirror (2), and thence to the tertiary mirror (3). The light can then go to large instruments on the Nasmyth deck (4), or to smaller instruments behind the primary mirror at the Cassegrain focus (5).

Right: An individual mirror segment before becoming a mirror. Note the complex support structure, which helps it maintain its shape in the telescope.

of a single mirror. Each segment is six feet in diagonal diameter, so a taller person could just sprawl across one. (We do not allow that to happen.) Each segment is precisely polished and bent into exactly the right shape, and sensors at the edges of the segments maintain their relative positions regardless of how the telescope tilts. The mirror is so smooth that if it were the diameter of the earth, its nonuniformities would be less than three feet in height, and the segments would be aligned to within two or three inches. (For a full description of the making and the workings of the Keck Telescope, see *E&S*, Winter '92.)

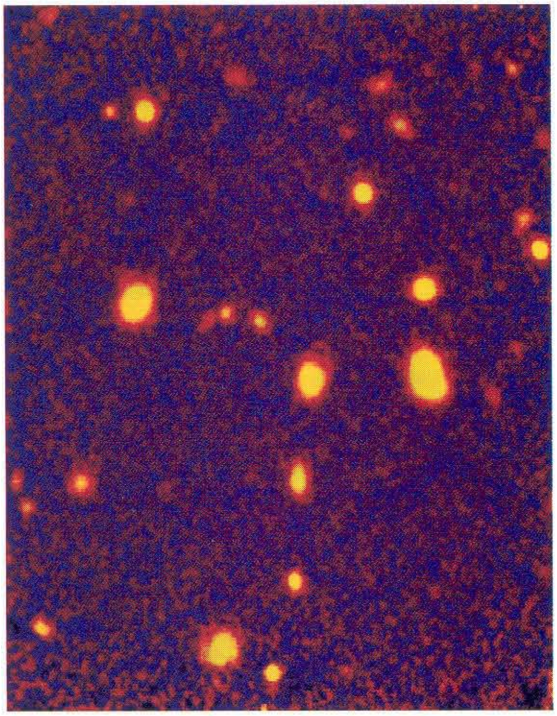
With a big telescope, everything else gets big, too. The light beams are wide due to the mirror's diameter, so the instruments are large. One, called the HIRES (for high-resolution) spectrograph, is the size of a two-car garage. It's so big that we can't hang it from the back of the telescope, like you would a camera. It has to sit on a special platform, called the Nasmyth deck, that we shunt the light to. And big things cost big money. Even observing time is expensive. The cost of building and operating the telescope prorated over its lifetime comes out to roughly a dollar per second every night, whether it's cloudy or not. I once suggested that we install a little meter in the control room, rigged so that if something went wrong the meter would start running, and we could see how much money we're wasting. That's probably not such a good idea, but in any case it's very expensive science.

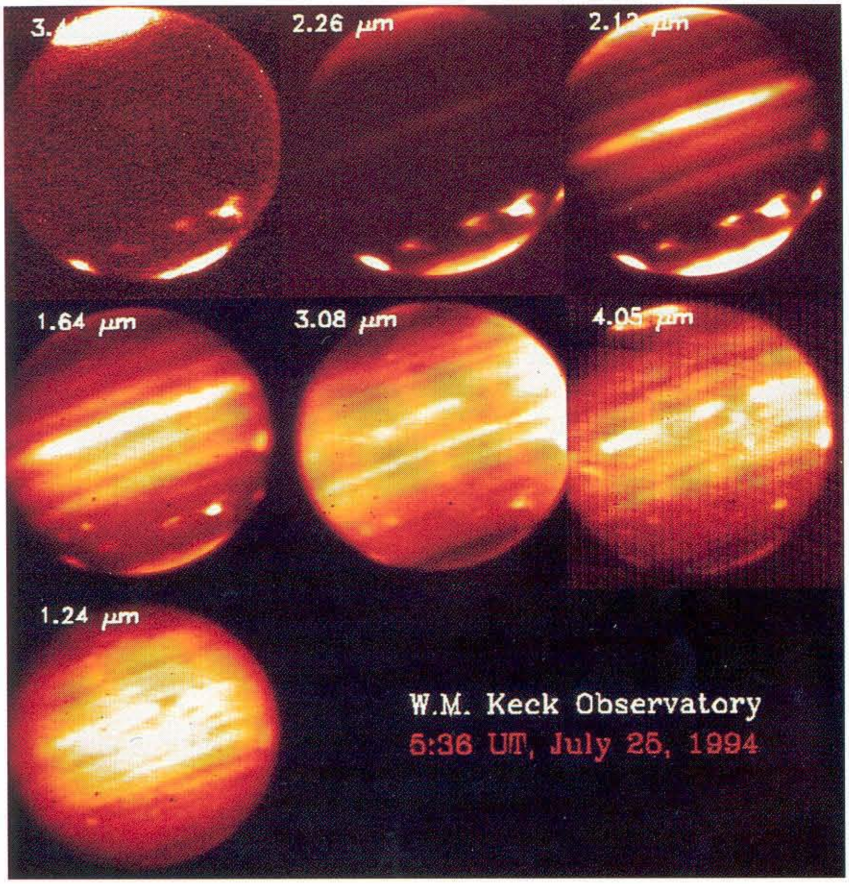
So what are we doing with the biggest telescope on earth? As you know, the fragmented comet Shoemaker-Levy 9 struck Jupiter as a spectacular firework in July 1994. (See *E&S*, Fall '93.) A collaboration led by Imke de Pater and James Graham, both of UC Berkeley, was standing by with Caltech's Near-Infrared Camera. And wouldn't you know it—it was cloudy and, as the predicted moment of the first impact approached, all present were tearing their hair out. But at the very last instant, the clouds parted, and wonderful data came in. On the opposite page is a picture of the glowing impact sites. The kinetic energy from just one of the larger fragments was equivalent to thousands of global thermonuclear wars in a single go. If one were to hit planet earth, it could ruin your whole day. From this event, astronomers have learned a great deal about the chemistry of Jupiter's atmosphere and the physics of large impacts. Since it's perfectly possible that something big will hit us at some point, this is obviously an important thing to study.

Now let's leap all the way out to the edge of the universe and try to see the faintest objects we can possibly see. That may sound trite, but it's actually very good science. We turn the telescope on some relatively blank patches of sky and look for a long time to collect as much light as we possibly can, and then we classify and count the objects we see. Such surveys have been done with other telescopes at visible wavelengths, but the infrared isn't so well explored. Besides, the bulk of emitted energy from the most distant galaxies has been redshifted to the infrared by the time it gets to us, and the Keck is the world's best telescope for infrared astronomy, so it was an obvious thing to do. We've looked in several

Below: Seven pictures of a battered Jupiter, taken at different wavelengths of infrared light. The five glowing spots in a line in the southern hemisphere are the heat plumes generated by the impacts of (from left) comet fragments H, Q1, R, D and G (which hit one Jupiter day apart in the same spot), and L.

Right: Long, long ago in an infrared galaxy far, far away...



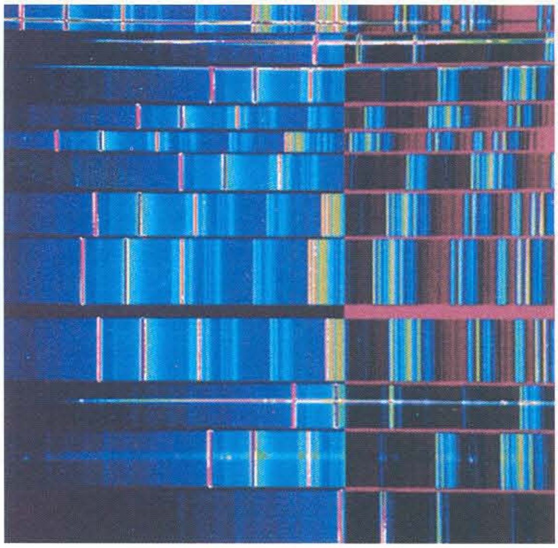


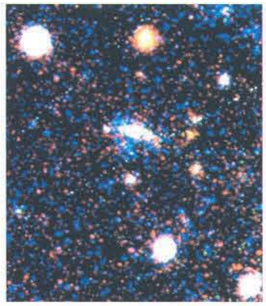
widely separated directions, to try to choose random, representative samples of the universe. One such sample is the photo at left, which is probably the deepest infrared image of the sky ever obtained. It's a very tiny portion of sky—less than an arc minute in angular diameter. There may be a couple of stars in it, but most of the objects you see are very distant galaxies. We don't know how far away they are yet, but we're certainly going to try to find out.

This survey is the work of a number of Caltech people: Roger Blandford, the Tolman Professor of Theoretical Astrophysics; Professor of Astronomy Judith Cohen (MS '69, PhD '71); Gerry Neugebauer (PhD '60), the Hughes Professor and professor of physics; Professor of Physics Tom Soifer (BS '68); me; Member of the Professional Staff Keith Matthews (BS '62); and grad students David Hogg, James Larkin, and Mike Pahre. (These folks also provided most of the pictures I'll show you.) It has probed the very faintest limits ever reached. Its dimmest infrared observations, translated into visible light, would be about 28th or 29th magnitude—much fainter than that 25-watt light bulb a million kilometers away.

By counting galaxies according to their color and apparent brightness in these deep, deep reaches of the universe, we can actually test models of cosmology. Such counts can help answer two important questions: First, will the universe expand forever, or will it ultimately recollapse? A universe dense enough to recollapse will expand more slowly, due to its greater gravity; thus, it will occupy a smaller volume, and contain fewer galaxies to be counted. Second, how do galaxies evolve? Looking billions of light-years away is equivalent to looking billions of years back into the past—we see the galaxies as they were when they emitted that light billions of years ago. Galaxies were generally brighter in the past, because they were then in an intense period of star formation and so were full of hot, young stars; the more star formation was going on, the brighter they were. Brighter objects can be seen at larger distances, so we can see more of them. Thus, the earlier galaxies evolved in the history of the universe, the more of them we should see at great distances. On the other hand, galaxies can collide and merge, so their numbers can decrease over intermediate timescales.

Detailed modeling of the galaxy counts we observe can be used to disentangle these effects. As we look in a given wavelength at ever more distant galaxies, we see light that was originally emitted from them at ever shorter wavelengths, due to the cosmological redshift. The effects of star formation are more prominent at the shorter using the LRIS in its imaging mode. (With the LRIS, one gets either pictures or spectra, so, again, specific galaxies are selected from the images for later spectral analysis.) These are probably the deepest visible-light images ever obtained, and, again, virtually every one of the little blobs of light is a galaxy that's probably billions of light-years away.





Above: This swatch of Madras plaid is actually a false-color LRIS multi-slit spectrum. Each horizontal band is a section of the spectrum from a different piece of the sky. The vertical stripes are the background spectrum from the night sky, and the thin horizontal streaks in the top three bands and in the second and third bands from the bottom are the spectra of faint, distant galaxies. The little beads on each streak are the galaxy's individual emission lines, which can be used to measure the galaxy's redshift.

Right: 4C41.17 is the distorted horizontal blob in the center of the photo.

(bluer) wavelengths, since luminous, massive young stars are blue. Thus, counting galaxies seen in visible light is a process very much influenced by their star-formation histories, perhaps confusingly so—it may be hard to distinguish star-forming dwarf galaxies that are relatively nearby from quiescent giants far away. An added complication is that the bluer wavelengths are more sensitive to absorption by interstellar dust.

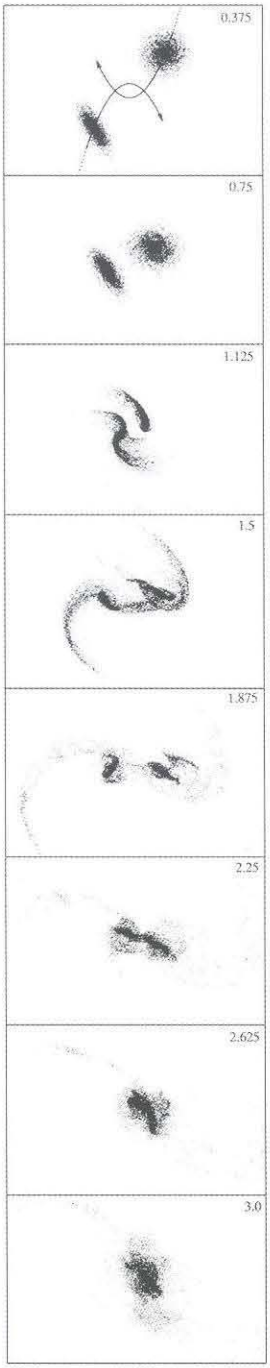
Counting galaxies in the near-infrared largely bypasses these problems, but the only way to really be sure of what you're seeing is to take spectra. The spectral fingerprints of various classes of galaxies are quite distinctive, and, of course, their redshifts tell you their distances. However, you have to collect a lot more light to get a spectrum than you need to take a picture. And since the whole point of the survey is to look at as much of the sky as quickly as possible, we're using the Keck to take pictures, from which we select specific galaxies for follow-up spectral studies. We do the spectrographic work using the Low-Resolution Imaging Spectrograph (LRIS), an instrument that Cohen and Professor of Astronomy, Emeritus, J. Beverley Oke built at Caltech for the Keck.

It is, however, still useful to do deep galaxy surveys in visible light, as it complements the information that's obtained in the infrared. Such a survey is being done by Professor of Astronomy Shrinivas Kulkarni and Cohen, along with grad students Hogg and Lin Yan, and then-postdoc Ian Smail, now in England. The photo at the beginning of this article is a segment of one of their survey pictures, which they obtained by using the LRIS in its imaging mode.

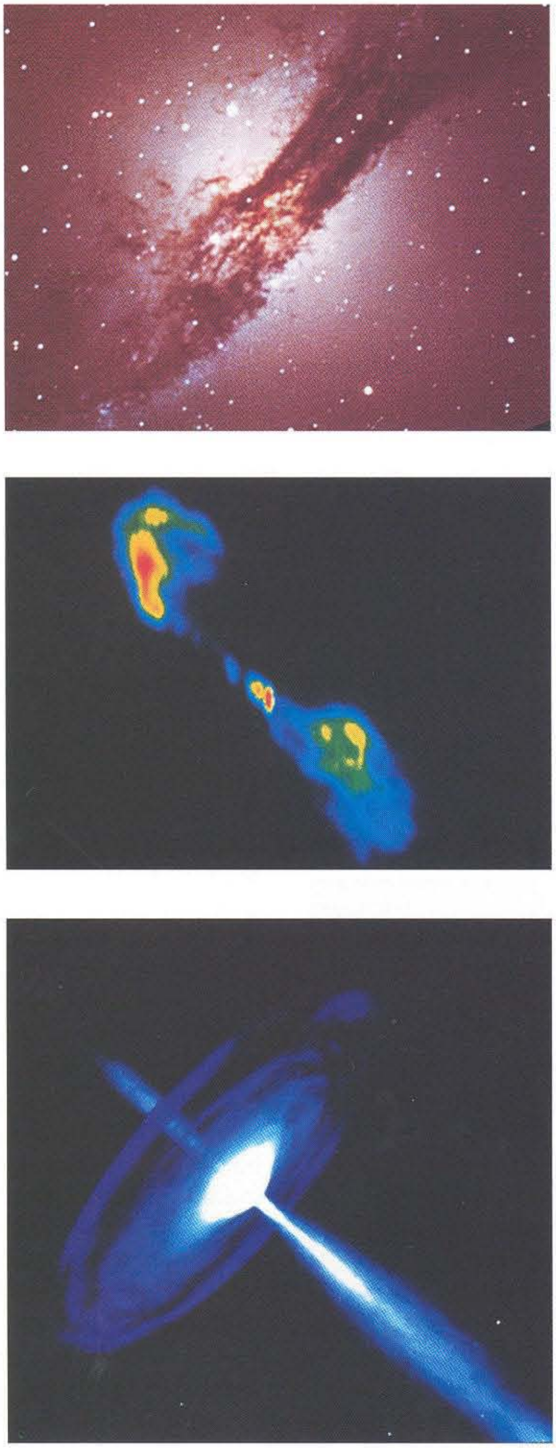
Just counting galaxies is interesting, but we really want to know how far away they are. We've now started a project to measure their redshifts, using the LRIS in a multislit mode where, instead of measuring distances to galaxies one by one, we put a mask in the instrument that selects 20 or 30 faint galaxies at once. A team of astronomers at UC Santa Cruz plans to build an instrument dedicated to this purpose that will take roughly 100 spectra at once. As far as we can tell, the universe is pretty much homogeneous—that is, it's the same in all directions—up to very large scales. There is clustering of galaxies, even very faint galaxies, but the deeper into space you look, the weaker it gets. One reason for this is that large-scale structures need time to grow—it takes a while for galaxies to fall together—so the clustering signal gets fainter the further back you look. The origins of large-scale structure and clustering of galaxies are fascinating problems, and that's one of the things we're trying to address in this deep-sky survey.

We can also study individual galaxies that we know are far away. At left is an infrared image from the very first science run with the Keck, of a radio galaxy—a galaxy that's also a copious emitter of radio waves—called 4C41.17. At the time, it was the most distant galaxy known. It has a redshift of 3.80. Because an object's redshift is a proxy for its distance, and because light takes a very long time indeed to reach us from such far-off galaxies, this redshift is equivalent to a look-back time of 88 percent of the age of the universe. In other words, we're seeing 4C41.17 as it was when the universe was a mere 12 percent of its present age. We don't know how old the universe is—that's one of the biggest questions in cosmology today—but if we assume that the universe is about 15 billion years old, which is our best guess at the moment, then the light from this galaxy started toward us 12 billion years ago. This light was already about two-thirds of the way here when the solar system formed. It's therefore possible that we are seeing light emitted from what was then a very young galaxy still in the process of formation.

At this point, you might well ask how we know that the universe is still expanding, if our data are 12 billion years old? What's happening



Left: Two spiral galaxies approaching each other on parabolic trajectories (arrows) distort, form a bridge, and eventually merge. The number in each frame shows the time elapsed since the beginning of the simulation, in units of 250 million years.



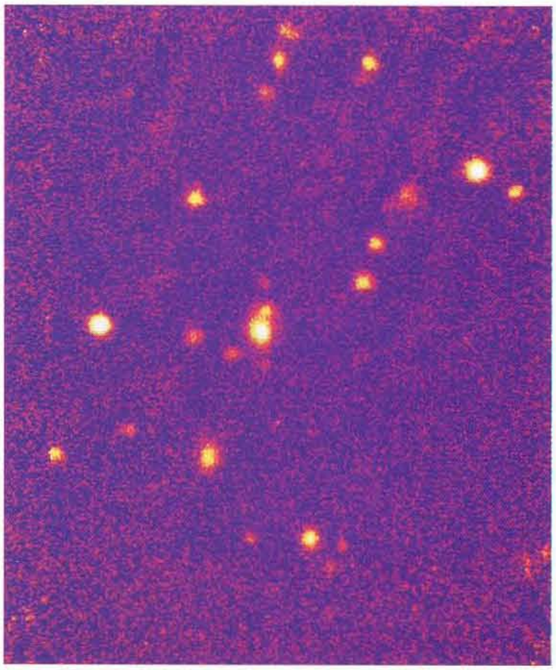
Left: Radio galaxy Centaurus A as seen in visible light (top) and at radio wavelengths (middle). The dark band across the center of the top photo is a thick cloud of dust that we assume is the byproduct of a galactic collision. The two views are at the same orientation, but not the same scale. If we were close enough for a better view, the galaxy and its plasma jets might look like the bottom picture.

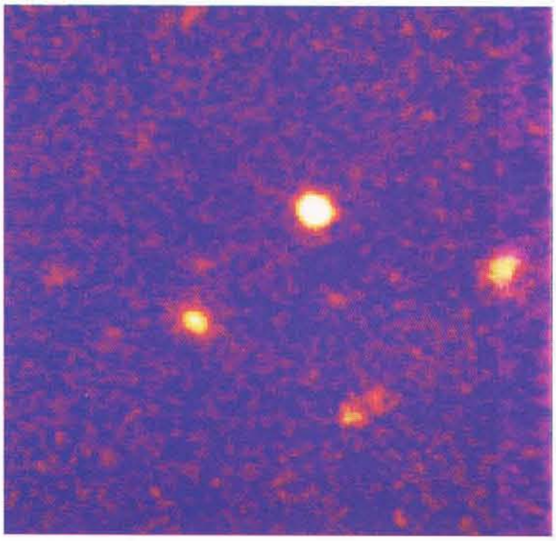
back there now? And the answer is, we have to wait another 12 billion years and look again to find out. It's a good Caltech thesis—or maybe a good tenure project, I don't know.

Since the universe is expanding in every direction, it may come as a surprise that galaxies like to collide. At far left is a numerical simulation of two galaxies that should pass by each other, but no—they distort each other tidally, bash together, collapse, and make a single object, while vast masses of gas sink right smack into the galactic core. Such collisions, and even near misses, may be what creates radio galaxies and quasars, which are also strong sources of radio waves.

Radio galaxies often appear misshapen in visible light, but they look even stranger at radio wavelengths. They tend to have a small, bright core right in the middle, flanked by two huge blobs of plasma—charged particles, like electrons—spurting out in opposite directions. Most of the galaxy's radiated energy comes from the plasma blobs. What we believe is going on, and those of you who bought Kip Thorne's book, *Black Holes & Time Warps*, may recognize the illustration, is that the radio galaxy has a massive black hole in its belly. Gas stirred up by the collision spirals in toward the black hole, forming a disk because it has too much angular momentum to fall straight in. The spiraling gas gets ionized and accelerated into fast jets of plasma that, perhaps guided by a magnetic field, shoot out perpendicular to the disk to become the radio lobes.

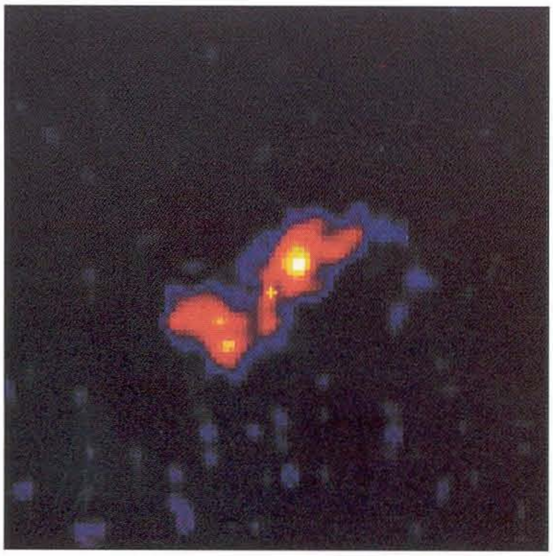
Quasars emit the radiant energy of a galaxy from an object roughly the size of the solar system. Their redshifts reveal them to be the most





B3 1253+432 (left) is unprepossessing to the eye, being the faint red object above the left-hand point of the diamond, but a plot of its spectrum (below) shows something more exotic. One axis of this plot is velocity, and the other is spatial distribution. If everything associated with the galaxy were receding from us at the same speed, all the data would plot to a straight line. Instead, we see two lobes of gas being ejected from the galactic core (white cross) in opposite directions. One lobe is coming toward us, the other is heading away from us. The colors indicate the signals' intensity, which provides information about the mass and excitation states of the gas.

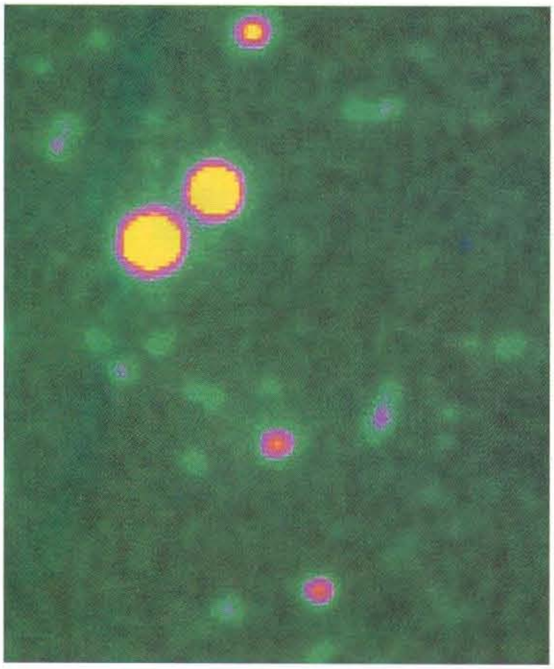
Right: The bright yellow spot at the center of this image is 3C324. The cluster of elliptical galaxies surrounding it include the two red spots just below it and the red spot just above it.

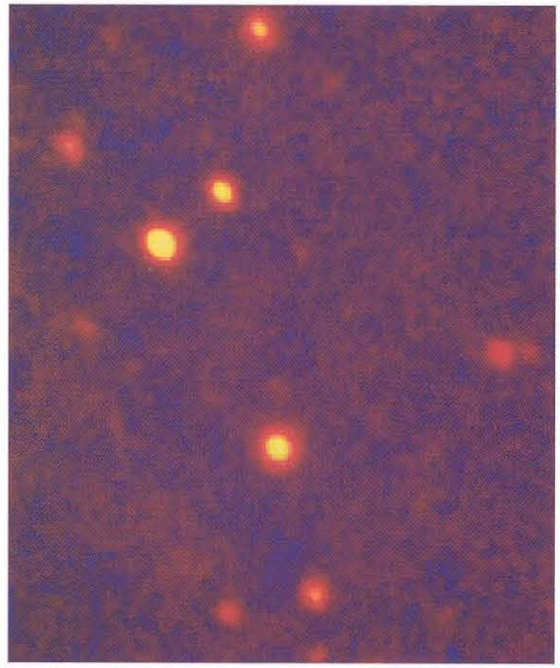


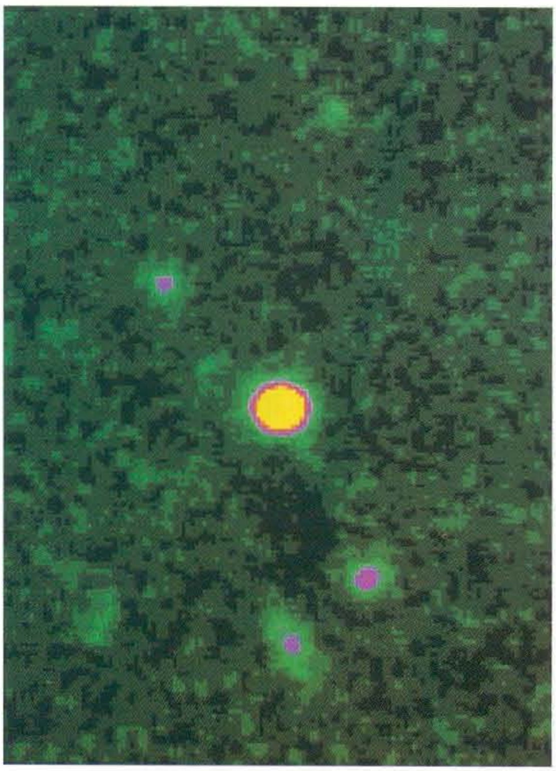
distant known objects in the universe, and they thus give us hints about galaxy formation in the very earliest days of the universe that we can see. Most people believe that there's a quasar hidden in every radio galaxy, and whether we see the quasar or not depends on our viewing angle. This is fine, because when the quasar is obscured, we can study the radio galaxy, which the quasar would otherwise outshine.

High-redshift radio galaxies may look peculiar, but at least we can see them—they're the most distant galaxies known. They give us some glimpse of what galaxies in the early universe might look like. This thing at left, known by the romantic name of B3 1253+432, has a redshift of 2.33, which means that the look-back time is 80 percent of the way to the Big Bang. B3 1253+432 doesn't look too impressive, but its spectrum showed something truly spectacular, as shown in the lower image. This galaxy seems to be ejecting vast masses of ionized hydrogen at speeds of a couple thousand kilometers per second, which is about as fast as we've ever seen. We don't understand such phenomena very well, but it shows you the power of the Keck, both in infrared imaging and in optical spectroscopy, to study such systems.

Above is another radio galaxy, also known from previous studies, called 3C324. It has a redshift of 1.21, which is a look-back time of about 65 percent of the way to the Big Bang. Mark Dickinson from the Space Telescope Science Institute, Hy Spinrad from Berkeley, and colleagues used the Keck to find that 3C324 is surrounded by what appears to be a cluster of







Above: The brightest two objects in the middle of this photo are a pair of quasars as seen in visible light.
Right: Some of the objects around the quasar pair weren't very bright in visible light, but stand out more prominently in this infrared image. These infrared-bright galaxies could be elliptical galaxies in a cluster associated with the quasar pair.

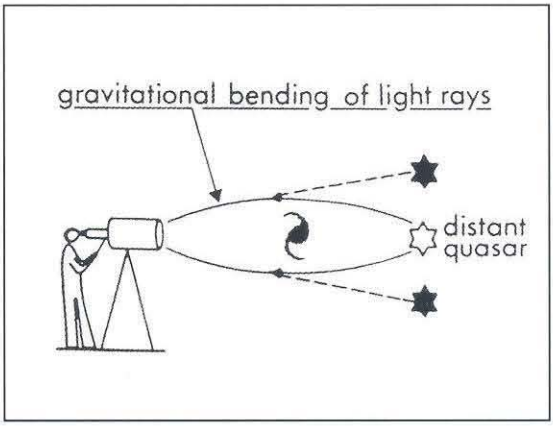
Left: Quasar Parkes 1614+051 is the large yellow blob in the center of this picture. The red dot above and to the left of the quasar is a previously discovered active galaxy. The green dot midway between the two, however, is a newfound companion galaxy. Analysis of its spectral data is still under way, but preliminary results hint that the companion galaxy is at the same distance as the quasar and the active galaxy, indicating that they may all belong to a compact group of galaxies.

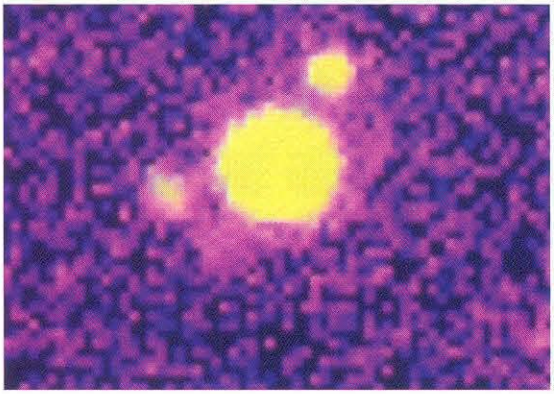
relatively normal elliptical galaxies. This is a major find, because it will probably prove to be one of the most distant clusters known. The researchers recently obtained spectra of it with the Keck, and have measured some of the distances, but the work hasn't been published yet.

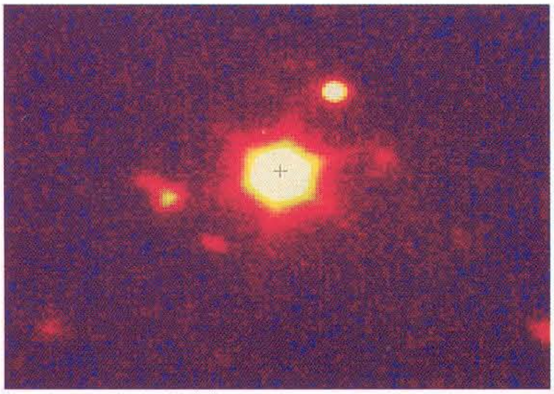
We're also studying several known pairs of quasars. We're doing this for two reasons. One is to better understand what makes quasars "quase"—in order to produce a quasar's stupendous amount of energy, a great deal of material would have to be dumped into that black hole. The other is that, since galaxies like to cluster, wherever we see a quasar pair, there may be other, relatively normal galaxies lurking nearby. The pictures above show two separate quasars at a redshift of 1.35—68 percent of the look-back time—and they also seem to be surrounded by a cluster of normal, reddish-colored galaxies. We would dearly like to measure the distances to the red galaxies, but we simply haven't had the chance to do so yet. This could be the most distant cluster known, if it pans out. Clusters of galaxies are good because they give you a whole selection of galaxies at the same distance, which you can compare to clusters nearby. This can tell you a lot about the way in which galaxies evolve.

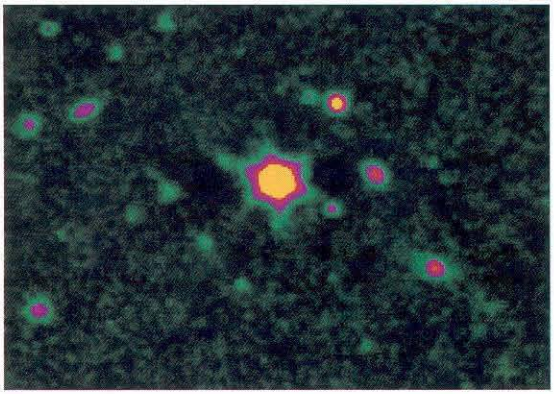
Another known quasar, called Parkes 1614+051, sits next to an active galaxy (an otherwise normal galaxy that has a brighter-than-normal core) at the same distance. They have a redshift of 3.21, or a look-back time of 86 percent of the way to the Big Bang. This might be a whole compact group of galaxies, perhaps still forming.

Below: Gravitational lensing can cause an observer to see two images of a single distant quasar. (You can see this was drawn especially for the Keck Telescope.)









Top: The thing that looks like a water molecule is a quasar pair named UM 425. One quasar is the oxygen atom; the other quasar is the right-hand hydrogen atom. This is the view from the European Southern Observatory's 3.6-meter telescope at optical wavelengths.

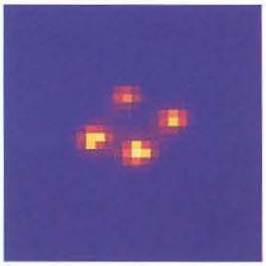
Middle: The 10-meter Keck's-eye view of the same quasar pair, also at optical wavelengths, unveils two fainter companions, one of which was sort of visible before (but only if you knew to look for it).

Bottom: In the Keck's infrared view, the pair on the left side becomes a threesome, the big quasar acquires two close attendants, and the little quasar gets a buddy of its own. Note that the brighter light sources morph into six-sided objects in the Keck images—an inheritance of the telescope's hexagonal mirror segments. Thus every star is a Star of David at the Keck.

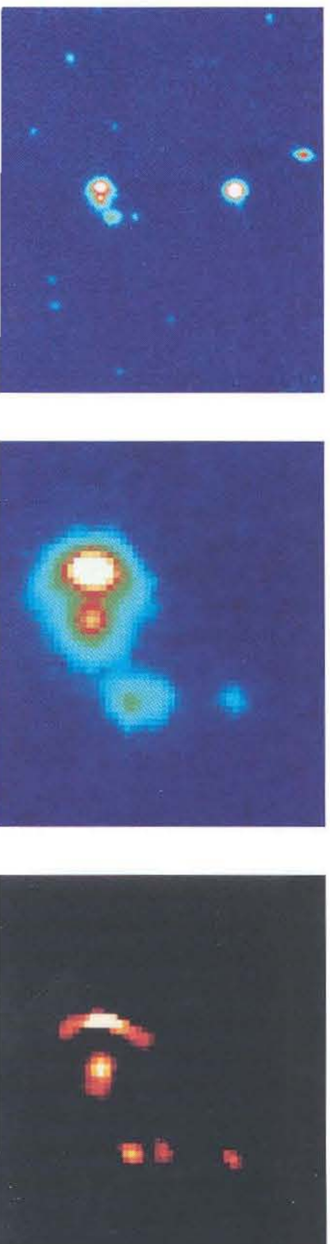
Here's an example of just how much better you can study such systems with the Keck. The top left picture is of UM 425, a known quasar pair at a redshift of 1.46, taken with the European Southern Observatory's 3.6-meter telescope. Below it is the same pair as seen by the Keck. The Keck has revealed a lot of features that couldn't be seen before. We've taken these quasars' spectra, and they're noticeably different, so the quasars are probably interacting, perhaps in a compact group of galaxies. The real surprise came when we took an infrared image (bottom), revealing a whole set of new infrared galaxies that are probably at redshifts similar to that of the two quasars—70 percent of the look-back time to the Big Bang. The sharpness of these images is unprecedented.

Another strange astrophysical phenomenon we can look at is gravitational lensing. One of the consequences of Einstein's theory of general relativity is that a gravitational field will bend the paths of light rays; indeed, that's how the theory was originally tested. Now, suppose there's a distant quasar in the background, and there's a galaxy, or a cluster of galaxies, in the foreground. As shown in the diagram above, the light rays from the quasar will be bent by the foreground gravitational field, and instead of missing the earth, will fall into the telescope. The observer can extrapolate the diverging light rays backward, and should see two images instead of one. (Actually, it wouldn't have to be two images—depending on the geometry of the situation, it could be four images, or five, or segments of a ring, or even a complete ring.) The

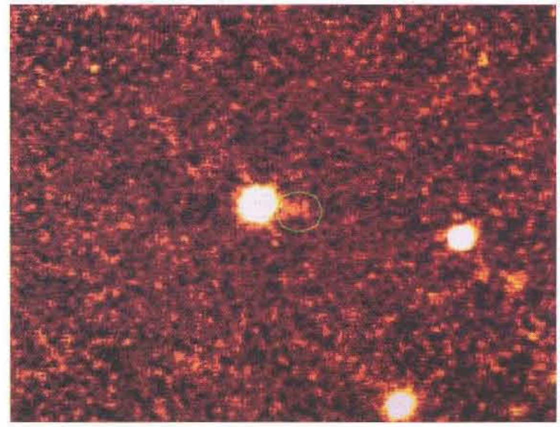
Below: The Cloverleaf lens, as see by the Keck.



Left: An early picture from the Keck's NIRC (top) resolved IRAS 10214+4724 into a multiple source, seen better in this zoom-in (center). A newer NIRC image (bottom) dissects the object into a point source and an arc—a telltale mark of a gravitational lens. The seeing in the two images has sharpened from 0.8 to 0.4 arc seconds due to improvements in mirror alignment.



Below: BRI 1202-0725, as seen by the NIRC. The green circle marks the faint foreground galaxy that's imprinting absorption lines on the quasar's spectrum.



first extragalactic example of this phenomenon was discovered in 1979, but it was actually predicted in the 1930s by an eccentric genius at Caltech, Fritz Zwicky, who made many other fundamental discoveries and predictions.

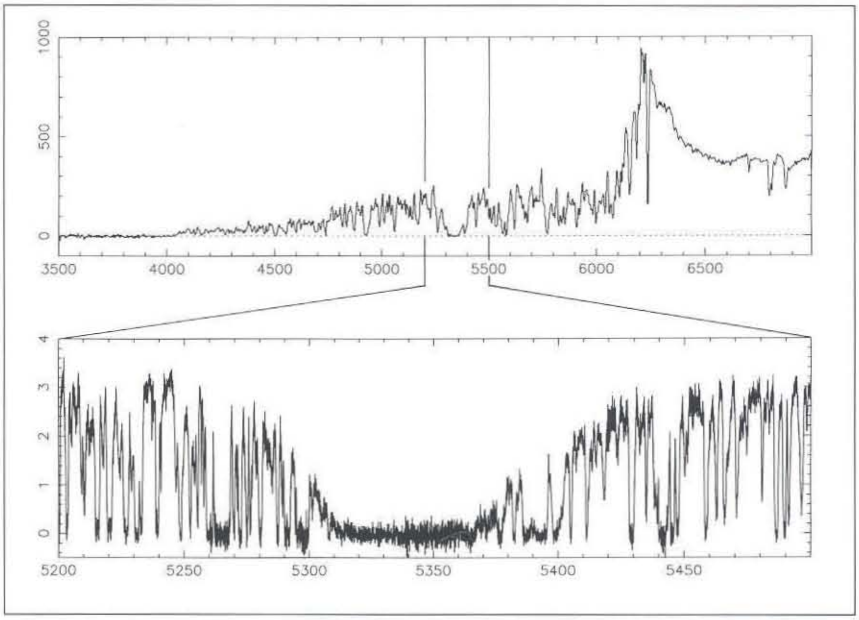
The Keck turns out to be a wonderful machine to check on gravitational lenses. Having a large telescope that gathers lots of light is good, but there's another important factor, and that's image quality. Images are blurred by the earth's atmosphere, most of which lies at altitudes below the Keck; therefore, the telescope's image quality is superb. This image shows a previously known lens called the Cloverleaf, for obvious reasons. The Cloverleaf is about an arc second wide. Typical seeing at Palomar, which is about as good as you're going to get anywhere in the continental United States, is one or two arc seconds, so you'd just see a little square blob. The Keck can distinguish between objects that are 0.3 arc seconds apart, which is fantastic. To give you an idea, 0.3 arc seconds is roughly what a dime would look like from five miles away.

But the important thing about gravitational lenses is that they really *are* lenses. Because you can use a gravitational lens to help you see farther than you could otherwise, you can think of it as an attachment you put in front of the telescope. (And if you thought that a hundred million dollars was expensive for a telescope, think how much it would cost to buy a cluster of galaxies!) So using gravitational telescopes in combination with the power of the Keck can teach us about ever more distant galaxies.

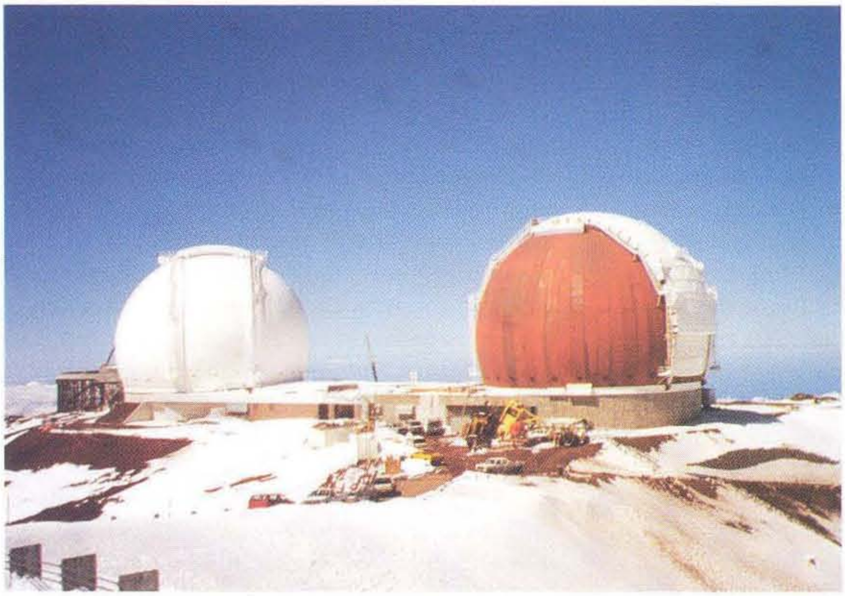
This leads us to what was once believed to be the most luminous object in the universe. It's called IRAS 10214+4724, but nobody knew what it was, or why it appeared to be as bright as 50,000 Milky Way galaxies—500 trillion times more luminous than our sun. One of the first images from the Keck Near-Infrared Camera resolved it into a nice set of components, including something that kind of looked like an arc. When people saw this, they began to mumble, "gravitational lens, gravitational lens." Recently, James Graham and his collaborators obtained superb images, including the one at bottom left, with the Near-Infrared Camera, which indeed shows a point source and an arc. So this object, which launched dozens of papers proposing theories to explain how it could be so fantastically luminous, turns out to be yet another gravitational lens. Subsequent observations by the Hubble Space Telescope have confirmed the Keck result.

The infrared sky is a whole new sky—a lot of wonderful new stuff is showing up. A year or so ago, we looked at the second most distant quasar known, BRI 1202-0725, with the Near-Infrared Camera, and a faint infrared galaxy showed up nearby. Last winter Wallace Sargent, the Bowen Professor of Astronomy, and postdocs Limin Lu and Donna Womble obtained the quasar's spectrum and discovered that it shows absorption lines due to a foreground galaxy at a redshift of 4.4. The infrared galaxy could well be that absorber—it's faint enough to be that distant. A group at the European Southern Observatory recently obtained more data and essentially proposed the same thing. If confirmed, this infrared galaxy would then be the most distant galaxy

Right: The upper spectrum, of quasar Q 0000-263, was gathered at the five-meter Hale Telescope at Palomar in 50 minutes. (Data from Sargent, Steidel, and Boksenberg, 1989.) The lower one, of the same quasar, took the Keck HIRES only four times longer to collect but has a 45-fold increase in resolution.



Below: Kecks I (white dome) and II (primer-red dome). I used to tell visitors that the infrared telescope lived in a special red dome, but it's since been painted white.

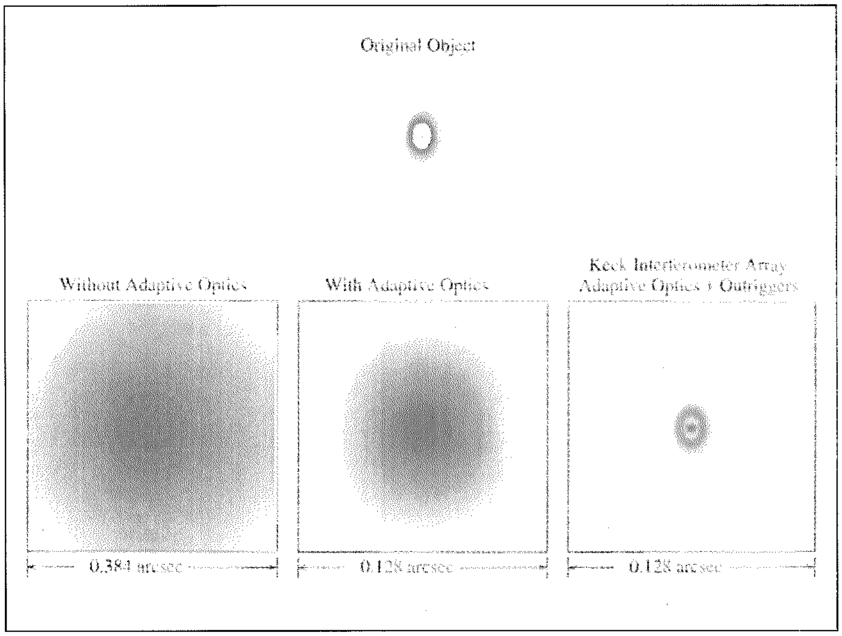


known, other than the quasars themselves.

There's a great deal of work being done on quasar absorption lines at the Keck. If you look toward a quasar, your line of sight might pass through a cloud of gas, which might be associated with a galaxy. Then, superimposed on the quasar's spectrum, you would see absorptions due to that gas. Multiply this by the many thousands of gas clouds that lie between you and a far distant quasar, and you have one humongously complex spectrum. The Keck Telescope coupled with the HIRES instrument is now the preeminent tool for studying intergalactic gas, because the Keck's great light-gathering power and the HIRES's superior spectrographic capability provide much more detailed spectra per unit of observing time.

What of the future? Eighty-five meters away from the Keck Telescope stands Keck II, which is almost finished. It should see first light this winter, and we hope to start doing science with it next autumn, or maybe even sooner. It was built under budget and ahead of schedule. The second Keck will be optimized for infrared astronomy, which means doing some extra little things, like coating the mirrors with silver rather than aluminum to make them more reflective to infrared light.

Of course, it's nice to have two telescopes instead of one, but the plan is to combine light from the two Kecks in an interferometer. The idea is to observe an object with both telescopes at once and combine the light into one signal with a precision of a few parts in a hundred billion or so—a small fraction of a wavelength.



What adaptive optics can do for you. In this simulation, the object being observed (top, center) is a protoplanetary disk—an embryonic solar system. The disk, which we're seeing face-on, surrounds a young, sun-like star 140 parsecs away from us. The star's light has swept away all the gas and dust out to the radius of Earth's orbit, allowing the star to shine forth from the cleared area while planets continue to form in the remaining part of the disk. The bottom left panel shows how this star might look to the Keck in its current state. The center panel shows how adaptive optics should sharpen the image, and the right panel shows what one might see with two Kecks and four small telescopes. (Such outrigger telescopes would provide additional information over baselines other than the one connecting the two Kecks.)

This doesn't mean you see any deeper, because you only get as much light as hits the two mirrors, but you achieve an ability to discriminate angular separations between objects that's equivalent to having an 85-meter-diameter telescope—the distance between the two mirrors. And seeing objects that are very close together can be used to confirm that planets exist around other stars and that our solar system is not unique, which is one of the major goals in astrophysics today. This is why the NASA planetary community has become a partner in the second Keck.

But there's something else we have to do before we can start doing interferometry. Light gets smeared and blurred as it passes through the earth's turbulent atmosphere, so we can't see quite as far or as sharply as we would if the atmosphere weren't in the way. Many people are working on a new technology, called adaptive optics, that allows us to probe the atmosphere by using a bright star next to the object we're studying. (If there isn't a star handy, some other observatories shine a laser beam skyward. Roughly 100 kilometers up, in the upper troposphere, the beam reflects from a sodium layer—detritus from meteors that burned up in the atmosphere—and generates a fake "star." Although still visible to the telescope after a 200-kilometer round trip, the outgoing beam can't be seen by anyone standing even a few hundred feet from the dome.) We know that the light from the bright star (or the laser) starts out with its rays perfectly parallel, so we can measure how the light was distorted by the atmosphere. This information is sent to a small adjustable mirror that moves to compensate for the distortion and take out the blur. In other words, adaptive-optics technology untwinkles the stars and makes them as sharp and steady as seen by the Hubble Space Telescope. We're just beginning to design an adaptive-optics system for Keck II, and we hope eventually to have one for Keck I as well. Then we will be able to combine the light in interferometer fashion to achieve the best possible resolution the telescopes are capable of. The farther apart you put your telescopes, the more difficult it gets to do interferometry. We don't really know how hard it will be to make it work over an 85-meter baseline. It's going to take a great deal of hard work, but there are no insurmountable technical obstacles.

The simulation at left shows what one might expect to see with one Keck and adaptive optics (left box), and with two Kecks and adaptive optics (center box). (If we were to then add four smaller telescopes, and observe for a few nights to gather more light, we might see something like what's shown in the right box.) Once the adaptive optics are working, the Keck will be competitive with the Space Telescope in sharpness of vision, at least at infrared wavelengths (which the Space Telescope does not observe), and probably at a very minor fraction of the cost of the Space Telescope. This is where the future lies, I think—in the adaptive-optics revolution, in removing the detrimental effects of the earth's atmosphere. It's much cheaper than putting telescopes on the moon, and who knows what wonderful discoveries still await us? □

Associate Professor of Astronomy S. George Djorgovski got his BA in astrophysics from the University of Belgrade in 1979, and his PhD in astronomy in 1985 from UC Berkeley. He then spent two years as a junior fellow at Harvard, and joined the Caltech faculty in 1987. His interests span many fields of astronomy, but in particular include questions about the structure, formation, and evolution of galaxies, quasars, and globular star clusters. He was an independent co-discoverer of what has come to be called the "fundamental plane," which describes the global properties of elliptical galaxies—that three measurable variables (radius, luminosity density, and the average kinetic energy of the galaxy's stars) satisfy a simple mathematical relationship. This insight has provided a benchmark against which theories of galaxy formation can be tested. (The question of where galaxies come from is one of the most exciting problems in astronomy today.) He was awarded a Presidential Young Investigator grant by the NSF in 1991, and was an Alfred P. Sloan Fellow from 1988 to 1991.

The Triple Helix

"I did not feel that I was in a race with Watson and Crick. . . . They felt that they were in a race with me."

by Thomas Hager

Most of the former contestants in the Caltech-Cambridge DNA duel gathered at a Caltech protein conference in September 1953 (this is about a third of the group). Pauling and Corey stand at right in the front row; John Kendrew at left. Wilkins is in the second row at the left behind Kendrew (no, they are not twins); Rich is second from left and Crick at far right. In the back row Max Perutz stands seoond from left, next to Schomaker, who is next to Watson, looming over Pauling's head. In 1962 Crick, Watson, and Wilkins won the Nobel Prize in physiology or medicine, while Perutz and Kendrew of the Cavendish Lab won it in chemistry. Pauling won the 1962 Nobel Peace Prize.

This article is excerpted from Force of Nature: The Life of Linus Pauling, *by Thomas Hager;*

In James Watson's 1968 book, The Double Helix, *he writes an irreverent account of the race to discover the structure of DNA—as seen from England, where the race was won in 1953. From the beginning, Watson and Francis Crick at Sir Lawrence Bragg's Cavendish Laboratory at Cambridge University, knew they were in a contest with Linus Pauling, "Cal Tech's fabulous chemist" for the prize, "the most golden of all molecules." Also involved in a somewhat uneasy collaboration on the English side were the x-ray crystallographers Maurice Wilkins and Rosalind Franklin at King's College in London.*

Meanwhile, what was going on at Caltech? In his recent biography of Pauling, Tom Hager gives the view from Pasadena. While Watson and Crick were wringing their hands about what progress he might be making, Pauling wasn't giving it much thought at all. He certainly considered DNA within his own province, but initially had little interest in it; he was preoccupied by proteins, which he thought far more complex and interesting than deoxyribonucleic acid. When he was refused a passport to attend a meeting of the Royal Society in London in May 1952, he missed the chance to see Wilkins and Franklin's x-ray photos and have his mind changed. (But Pauling's close collaborator, Robert Corey, did see the photos, which takes the blame for Pauling's failure off the State Department.) Pauling's passport came through in July, in time to attend the International Phage Colloquium at Royaumont, outside Paris, and hear the proof that DNA was indeed the master molecule of genetics. He spoke with Watson at Royaumont, met Crick at Cambridge, but did not bother to take the opportunity to visit King's College, missing his chance a second time. Pauling's interest was, however, finally piqued.

The real prize, the true secret of life, Pauling now knew, was DNA, and it was here that he next turned his attention.

On November 25, 1952, three months after returning from England, Pauling attended a Caltech biology seminar given by Robley Williams, a Berkeley professor who had done some amazing work with an electron microscope. Through a complicated technique he was able to get images of incredibly small biological structures. Pauling was spellbound. One of Williams's photos showed long, tangled strands of sodium ribonucleate, the salt of a form of nucleic acid, shaded so that three-dimensional details could be seen. What caught Pauling's attention was the obvious cylindricality of the strands: They were not flat ribbons; they were long, skinny tubes. He guessed then, looking at these black-and-white slides in the darkened seminar room, that DNA was likely to be a helix. No other conformation would fit both Astbury's x-ray patterns of the molecule and the photos he was seeing. Even better, Williams was able to estimate the sizes of structures on his photos, and his work showed that each strand was about 15 angstroms across. Pauling was interested enough to ask him to repeat the figure, which Williams qualified by noting the difficulty he had in making precise measurements. The molecule Williams was showing was not DNA, but it was a molecular cousin—and it started Pauling thinking.

The next day, Pauling sat at his desk with a pencil, a sheaf of paper, and a slide rule. New data that summer from Alexander Todd's

Proteins were Pauling's primary interest in the early 1950s. This photo of Pauling and Robert Corey with a protein model appeared in the October 1951 issue of *Engineering & Science*, illustrating an article on "The Structure of Proteins."

laboratory had confirmed the linkage points between the sugars and phosphates in DNA; other work showed where they connected to the bases. Pauling was already convinced from his earlier work that the various-sized bases had to be on the outside of the molecule; the phosphates, on the inside. Now he knew that the molecule was probably helical. These were his starting points for a preliminary look at DNA. He did not know how far he would get with this first attempt at a structure, especially because he still had no firm structural data on the precise sizes and bonding angles of the base-sugar-phosphate building blocks of DNA, but it was worth a look.

And this was what the central problem had reduced itself to in his mind: a question of phosphate structural chemistry.

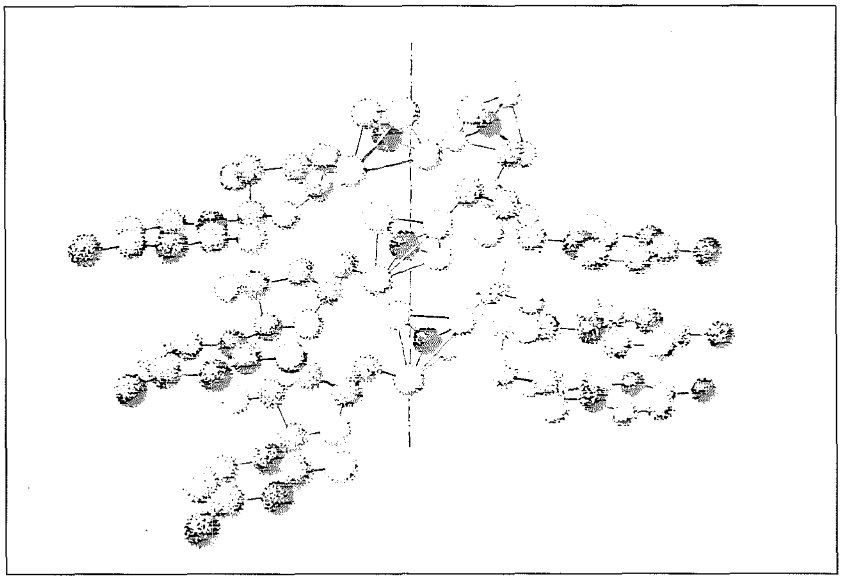
Pauling quickly made some calculations to determine DNA's molecular volume and the expected length of each repeating unit along its axis. Astbury's photos showed a strong reflection at 3.4 angstroms—according to Pauling's calculations, about three times his estimated length of a single nucleotide unit along the fiber. Repeating groups of three different nucleotides seemed unlikely; a threefold chain structure would explain the repeat more easily. His density calculations indicated that three chains would need to pack together tightly to fit the observed volume, but that was all right. In crystallography, the tighter the packing, the better. After five lines of simple calculations on the first page of his attack on DNA, Pauling wrote, "Perhaps we have a triple-chain structure!"

He was immediately captivated by the idea: three chains wound around one another with the phosphates in the middle. Sketching and calculating, he quickly saw that there was no way for hydrogen bonds to form along the long fiber axis, holding the windings of the chain in place, as in the alpha helix. Without them, what held the molecule in shape? One place that hydrogen bonds could form, he saw, was across the middle of the molecule, from phosphate to phosphate. That was a surprise, but everything else seemed to be working out. After six pages of calculations, he wrote, "Note that each chain has . . . roughly three residues per turn. There are three chains closely intertwined, and held together by hydrogen bonds between PO_4's." The only problem was that there did not seem to be quite enough space in the center of the molecule, where the phosphates came into closest contact. He put down his pencil for the night.

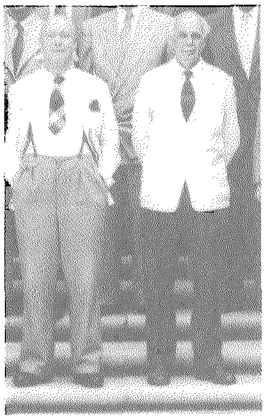
Three days later, he came back to the problem. According to Astbury's figures, DNA was a relatively dense molecule, which implied tight packing at the core. But trying to jam three chains' worth of phosphates into Astbury's space restrictions was like trying to fit the stepsisters' feet into Cinderella's glass slipper. No matter how he twisted and turned the phosphates, they wouldn't fit. *"Why are the PO_4 in a column so close together?"* he wrote in frustration. If Astbury's estimates on distances could be relaxed a bit, everything would fit, but Pauling could not do that without deviating too far from Astbury's x-ray data. Pauling next tried deforming the phosphate tetrahedra to make them fit, shortening some sides and lengthening others. It looked better, but still not right. He stopped again.

Next, he had an assistant go back through the literature in the chemistry library and pick up everything he could find on the x-ray crystallography of nucleic acids. There was not much to go on besides Astbury's work and that of Sven Furberg, a Norwegian crystallographer who had studied under Bernal and had found that the bases in DNA were oriented at right angles to the sugars. There was not one detailed structure of any purine or pyrimidine, much less a nucleotide.

On December 2 he made another assault, filling nine pages with drawings and calculations. And, he thought, he came up with something that looked plausible. "I have put the phosphates as close together as possible, and have distorted them as much as possible," he noted. Even though some phosphate oxygens were jammed uncomfortably close in the molecule's center, not only did it all just fit, but Pauling saw that the innermost oxygens packed together in the form of an almost perfect octahedron, one of the most basic shapes in crystallography. It was very tight, but things were lining up nicely. It had to be right. It had been less than a week



This perspective model of DNA appeared in Pauling and Corey's paper, "A Proposed Structure for the Nucleic Acids," published in the *Proceedings of the National Academy of Sciences* in February 1953. The phosphate tetrahedra are in the center, connected by the sugar rings into chains with the purines and pyrimidines (here represented by purine only) attached on the outside.



Sir Lawrence Bragg (right), Nobel laureate, cofounder of x-ray crystallography, and director of the Cavendish Laboratory, was Pauling's great rival. He chaired one of the sessions of Pauling's protein conference here in September 1953. At left stands William Astbury of Leeds University, on whose x-ray data Pauling based his DNA model.

since he first sat down with the problem.

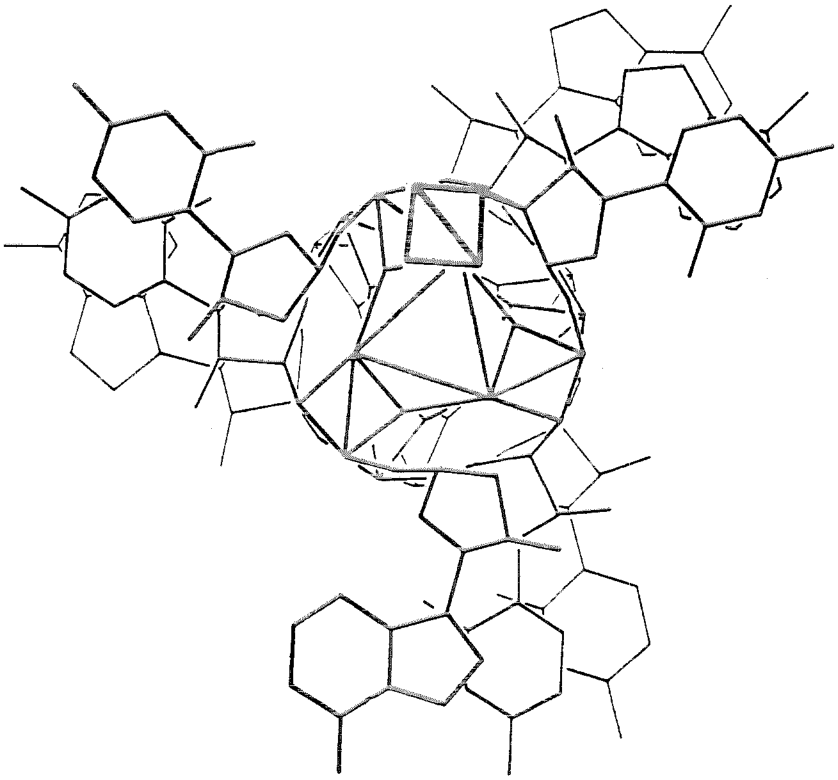
The next day, Pauling excitedly wrote a colleague, "I think now we have found the complete molecular structure of the nucleic acids." During the next several weeks he ran downstairs every morning from his second-story office in Crellin to Verner Schomaker's office, "*very* enthusiastic," Schomaker remembered, bouncing ideas off the younger man, thinking aloud as he checked and refined his model. He began working with Corey to pinpoint the fine structure.

Then came trouble. Corey's detailed calculation of atomic positions showed that the core oxygens were, in fact, too close to fit. In early December, Pauling went back to twisting and squeezing the phosphate tetrahedra. Someone brought up the question of how his model allowed for the creation of a sodium salt of DNA, in which the positive sodium ions supposedly adhered to the negative phosphates. There was no room for sodium ions in his tightly packed core, was there? Pauling had to admit he could find no good way to fit the ions. But that would sort itself out later. The other results were positive. Running the proposed structure through Crick's mathematical formula indicated that his model helix would fit most of the x-ray data, although not all of it. Schomaker played with some models on his own and found a way to twist the phosphate tetrahedra so that they were not quite so jammed, but for the moment Pauling saw no reason to change his ideas. The core phosphates were too neatly close-packed not to be true.

And this was what the central problem had reduced itself to in his mind: a question of phosphate structural chemistry. The biological significance of DNA would be worked out later, he thought; if the structure was right, the biological importance would fall out of it naturally in some way. At this point it was his business to get the structure, not the function. So he ignored the larger context surrounding the molecule and focused singlemindedly on one thing: finding a way to fit those phosphates into the core so that the resulting helixes fit the available data.

His faith in that approach had been justified by his success with the alpha helix. He had built his protein spiral from strict chemical principles, published it in the face of contradictory data, and later found the facts he needed to answer his critics. He was confident now about his ability to jump ahead of the pack, to use his intuitive grasp of chemistry to tease out a structure that felt right. If you waited for every doubt to be answered first, you would never get credit for any discovery. And his DNA triple helix felt right.

A week before Christmas, he wrote Alex Todd at Cambridge, "We have, we believe, discovered the structure of nucleic acids. I have practically no doubt. . . . The structure really is a beautiful one." Pauling knew that Todd had been working with purified nucleotides and asked him to send samples of x-ray analysis. "Dr. Corey and I are much disturbed that there has been no precise structure determination reported as yet for any nucleotide. We have decided that it is necessary that some of the structure determinations be made in our laboratory. I know that the Cavendish people are working in this field, but it is



Another view of Pauling's model from his February 1953 paper shows the tightly packed phosphates in the middle with the nucleotide residues spiraling around the outside.

they stressed, "the first precisely described structure for the nucleic acids that has been suggested by any investigator"—thus positioning the work as the nucleic acid equivalent to the alpha helix. He went through his reasoning for the core structure. Most of the paper concentrated on precisely stacking phosphate tetrahedra, but there was a little biology, too. In Pauling's model, the bases, the message-carrying portion of nucleic acids, were directed outward, like leaves along a stalk, with room enough to be put into any order, providing maximum variability in the molecule and thus maximum specificity in the message. Astbury had already noted that the 3.4-angstrom repeat in nucleic acid was about the same as the distance per amino acid along an extended polypeptide chain, raising the idea that new proteins might be struck directly off a nucleic acid mold. Pauling noted that his model allowed the same thing to happen, with the sides of four adjacent bases along his chains forming a space just right for fitting an amino acid.

such a big field that it cannot be expected that they will do the whole job." He then wrote his son Peter and Jerry Donohue that he was hoping soon to complete a short paper on nucleic acids.

But the structure still was not quite right. Everything would seem to fall into place when Corey came up with another set of calculations showing that the phosphates were packed just a little too tightly, their atoms jostling each other a little too closely to be reasonable. Pauling would readjust and tinker, bend and squash, so close to the answer yet unable to make it all fit perfectly.

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{Two days before Christmas, professional FBI informer Louis Budenz named as concealed Communists 23 people, including Linus Pauling. An irate Pauling called him a liar, but Budenz was protected from prosecution for perjury by congressional privilege.}

Depressed about this unexpected political attack, Pauling took the unusual step of inviting some colleagues into his laboratory on Christmas Day to have a look at this work on DNA. He was tired of the niggling problems with his model and ready for some good news. He got it from his small audience, who expressed enthusiasm for his ideas. Much cheered, Pauling spent the last week of the year working with Corey on the finalization of a manuscript.

On the last day of December 1952, Pauling and Corey sent in their paper, "A Proposed Structure for the Nucleic Acids," to the *Proceedings of the National Academy of Sciences*. This was,

There was, however, an uncharacteristic tentativeness in the piece. This was "a promising structure," Pauling wrote, but "an extraordinarily tight one"; it accounted only "moderately well" for the x-ray data and gave only "reasonably satisfactory agreement" with the theoretical values obtained by the Crick formula; the atomic positions, he wrote, were "probably capable of further refinement."

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It was, in fact, a rush job. Pauling knew that DNA was important; he knew that Wilkins and Franklin were after it and that Bragg's group had already made at least one stab at it. He knew that it was a relatively simple structure compared to proteins. And he knew that whoever got out a roughly correct structure first—even if it was not quite right in all its details—would establish priority. That is what he was aiming for, not the last word on DNA but the first, the initial publication that would be cited by all following. It did not have to be precise. He wanted credit for the discovery.

The hurried haphazardness of the nucleic-acid paper can best be understood by comparison to Pauling's protein work. Pauling's alpha helix was the result of more than a decade of off-and-on analysis and thousands of man-hours of meticulous crystallographic work. Before he published his model, his lab pinned down the structure of the amino-acid subunits to a fraction of a degree and a hundredth of an angstrom. There was an abundance of clean x-ray work available on the subject proteins, allowing Pauling to scrutinize and eliminate dozens of alternative structures. Two years passed between the time he came up with the rough idea for his helix and the time he published it. Much of that interval was spent with Corey, overseeing and refining the precise construction of a series of elaborate three-dimensional models.

None of that went into DNA.

"The only doubt I have . . ."

Crick and Watson were downcast by the news from Peter in late December that Pauling had solved DNA. Alternating between bouts of despair and denial—trying to figure out how he could have beaten them and then deciding that he certainly could not have without seeing Wilkins and Franklin's x-ray work and then thinking, well, of course, he is Pauling, so anything is possible—they continued working on the problem themselves. If they could come up with something independently before Pauling's paper appeared, at least they might share credit.

There it was in black and white in a respected text: The phosphates had to be ionized. The book they were looking at was Pauling's General Chemistry.

The previous spring, a few months after they had been warned off DNA and a few months before Pauling's visit to the Cavendish, Crick and Watson had been introduced to Erwin Chargaff, the acerbic and opinionated Austrian-born biochemist who had been using chromatography to analyze the chemical composition of nucleic acids. Chargaff was not impressed. "I never met two men who knew so little and aspired to so much," he said. "They told me they wanted to construct a helix, a polynucleotide to rival Pauling's alpha helix. They talked so much about 'pitch' that I remember I wrote it down afterwards, 'Two pitchmen in search of a helix.'" But this conversation was critical to Crick and Watson. Chargaff told them that there was a simple relationship between the occurrence of different bases in DNA, that adenine and thymine were present in roughly the same amounts and so were guanine and cytosine. One of each pair was a larger purine; the other, a smaller pyrimidine. It was the same relationship that he had told Pauling about during their Atlantic crossing in 1947 and that Pauling had ignored.

But it made all the difference to Crick and Watson. Franklin's criticisms had already pointed them toward putting the phosphates on the outside of the molecule; now they had the clue of a one-to-one relationship between the bases on the inside. They began thinking about helixes in which the purines and pyrimidines lined up somehow down the core of the molecule.

When Pauling's much anticipated DNA manuscript arrived via Peter in early February 1953, both researchers were surprised to see something that looked like their own abortive three-chain effort, only more tightly put together. A few minutes' reading showed that there was no room at the core for the positive ions needed to hold together the negatively charged phosphates. Crick and Watson were dumbfounded. Pauling's structure depended on hydrogen bonds between the phosphate groups, but how could there be a hydrogen there when the phosphates in DNA lost their hydrogens at normal pH? "Without the hydrogen atoms, the chains would immediately fly apart," Watson said. They had already been through this with their own model, but they checked it again, and there it was in black and white in a respected text: The phosphates had to be ionized. The book they were looking at was Pauling's *General Chemistry*.

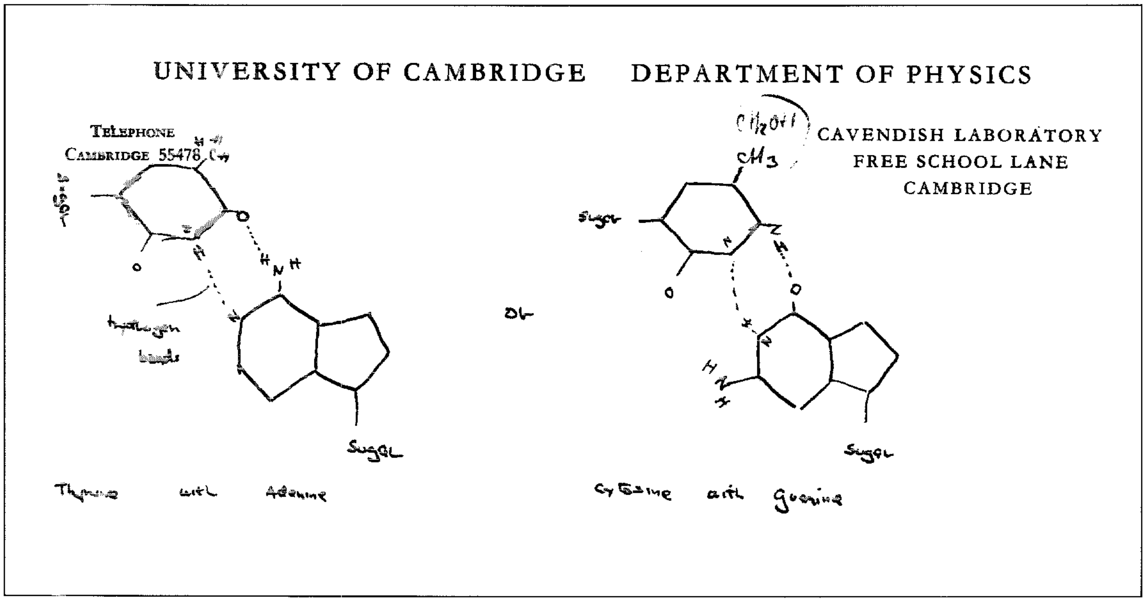
There was an immense feeling of relief. "If a student had made a similar mistake, he would be thought unfit to benefit from Caltech's chemistry faculty," Watson later said. He and Crick immediately went off to confirm their criticism with Cambridge's chemists. Before the day was out, Pauling's mistake was the talk of the college: Linus's chemistry was wrong.

Just as importantly for Watson, when he told Wilkins of Pauling's mistake and his idea that DNA was helical, he was given a reward: his first look at the more recent x-ray patterns Franklin had gotten from the molecule. She had found that DNA existed in two forms, a condensed dry form and an extended wet form the structure assumed when it drank up all that water. Astbury's photos, the ones Pauling had used, had been of a mixture of the two forms. Franklin's recent shots, much clearer and of only the extended form, immediately confirmed to Watson that the molecule was a helix and gave him several vital parameters for its solution.

With obvious satisfaction, Crick, still smarting a bit from the coiled-coil affair *[a dispute over credit for a solution to the alpha-helix structure]*, wrote Pauling, to thank him for providing an advance copy of his nucleic acid paper. "We were very struck by the ingenuity of the structure," he wrote. "The only doubt I have is that I do not see what holds it together."

Pauling's apparent misstep pleased Bragg so much that he agreed to let Crick and Watson go back full-time to DNA. There was a window of opportunity here, and he wanted the Cavendish to take advantage before Pauling had time to regroup.

Pauling, however, had already moved on to a new project, a theory of ferromagnetism that he



Watson's letter of March 12, 1953, to Max Delbrück contained this drawing at the top of the second page illustrating the last piece of the puzzle—the hydrogen bonds that form between thymine and adenine and between cytosine and guanine, making a ladder of paired bases across the double helix. At the end of his letter, Watson asked Delbrück, shown below in his Caltech lab in 1949, not to mention this latest solution to Pauling. But it was too good to keep quiet; Delbrück showed him the letter immediately.

worked on through the spring. He also began making plans for a major international protein conference in Caltech the next fall and was drawn back to DNA only when Peter wrote him in mid-February about the English hooting at his structure. Corey had by now finally finished checking Pauling's atomic coordinates, some of which appeared again to be unacceptably tight. "I am checking over the nucleic acid structure again, trying to refine the parameters a bit," Pauling wrote Peter back. "I heard a rumor that Jim Watson and Crick had formulated this structure already sometime back, but had not done anything about it. Probably the rumor is exaggerated." In late February he finally tried Schomaker's suggestion of twisting the phosphate groups 45 degrees and found that it eased some of the strain.

Something was still wrong. When Pauling gave a seminar on his DNA structure at Caltech, the reception was cool; afterward, Delbrück told Schomaker that he thought Pauling's model was not convincing. He mentioned a letter he had gotten from Watson saying that Pauling's structure contained "some very bad mistakes" and in which Watson had added, "I have a very pretty model, which is so pretty that I am surprised that no-one ever thought of it before." Pauling wanted to know more. He quickly wrote Watson inviting him to his fall protein conference, mentioning that he had heard from Delbrück about his DNA work, and encouraging him to keep working on the problem. "Professor Corey and I do not feel that our structure has been proven to be right," he wrote, "although we incline to think that it is." In early March he drove with Ava Helen to the University of California at Riverside to examine a collection of organic phosphates there, finding candidates for structural analysis that would be similar to the phosphate groups in DNA, looking for models to tell him how much he could deform his tetrahedra. Crick's barb about what held the molecule together led him to gather chemical precedents for the existence of adjoining negative charges in the same molecule, and he began to reason to himself that perhaps the DNA core environment was a special one that allowed the phosphates to exist as he had proposed. It was still, to Pauling, a matter of phosphate chemistry. Meanwhile, Todd had sent him the requested samples of nucleotides, and Pauling started their x-ray analysis.

He was finally laying the groundwork for a reasonable structure. But it was too late.

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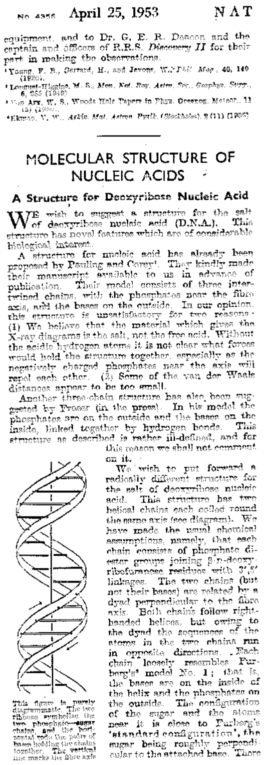
Given the go-ahead to return to DNA, thanks to Pauling's paper, Crick and Watson each began feverishly devising models, focusing more on two-stranded models now that Chargaff had gotten them thinking of bases somehow pairing with each other. The "very pretty model" of which Watson had written Delbrück was one attempt, but it was wrong, as Jerry Donohue pointed out.

Donohue's input turned out to be critical. A magna cum laude graduate of Dartmouth who had worked and studied with Pauling at Caltech since the early 1940s, Donohue knew structural chemistry inside and out. Hydrogen bonding



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equipment, and to Dr. G. E. R. Deacon and the captain and officers of R.R.S. *Discovery II* for their part in making the observations.

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[2] Longuet-Higgins, M. S., *Mon. Not. Roy. Astro. Soc., Geophys. Supp.*, 5, 285 (1949).
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MOLECULAR STRUCTURE OF NUCLEIC ACIDS

A Structure for Deoxyribose Nucleic Acid

WE wish to suggest a structure for the salt of deoxyribose nucleic acid (D.N.A.). This structure has novel features which are of considerable biological interest.

A structure for nucleic acid has already been proposed by Pauling and Corey[1]. They kindly made their manuscript available to us in advance of publication. Their model consists of three intertwined chains, with the phosphates near the fibre axis, and the bases on the outside. In our opinion, this structure is unsatisfactory for two reasons: (1) We believe that the material which gives the X-ray diagrams is the salt, not the free acid. Without the acidic hydrogen atoms it is not clear what forces would hold the structure together, especially as the negatively charged phosphates near the axis will repel each other. (2) Some of the van der Waals distances appear to be too small.

Another three-chain structure has also been suggested by Fraser (in the press). In his model the phosphates are on the outside and the bases on the inside, linked together by hydrogen bonds. This structure as described is rather ill-defined, and for this reason we shall not comment on it.

We wish to put forward a radically different structure for the salt of deoxyribose nucleic acid. This structure has two helical chains each coiled round the same axis (see diagram). We have made the usual chemical assumptions, namely, that each chain consists of phosphate diester groups joining β-D-deoxyribofuranose residues with 3',5' linkages. The two chains (but not their bases) are related by a dyad perpendicular to the fibre axis. Both chains follow right-handed helices, but owing to the dyad the sequences of the atoms in the two chains run in opposite directions. Each chain loosely resembles Furberg's[2] model No. 1; that is, the bases are on the inside of the helix and the phosphates on the outside. The configuration of the sugar and the atoms near it is close to Furberg's 'standard configuration', the sugar being roughly perpendicular to the attached base. There

This figure is purely diagrammatic. The two ribbons symbolize the two phosphate—sugar chains, and the horizontal rods the pairs of bases holding the chains together. The vertical line marks the fibre axis

The elegant structure of Watson and Crick's double helix, with its paired nucleotides forming a ladder through the center, left no doubt in anyone's mind by the time it was published in *Nature* in April 1953.

More than beautiful, the structure had meaning.

had been a specialty of his, and he saw that Crick and Watson, chemical novices that they were, had been playing with the wrong structures for guanine and thymine. He set them right, switching the hydrogen atoms essential for cross-bonding into their correct positions, destroying their earlier model and pushing them toward the correct solution.

With Donohue's corrections, Crick and Watson could now see hydrogen bonds forming naturally between specific pairs of purines and pyrimidines: adenine to thymine and guanine to cytosine. That was the last piece of the puzzle, and the result was dazzling. Matching a large with a small base not only smoothed the structure's outline but provided a simple explanation for Chargaff's findings. The resulting structure, a sort of ladder with base pairs as the steps and the sugar-phosphate backbone as the runners, formed easily into a helix that matched the x-ray data.

More than beautiful, the structure had meaning. Each strand was a complementary mirror image of the other; if separated, each could act as a mold for forming a new double helix identical with the original. This immediately provided ideas about replication that Pauling's model, with its bases facing out and unrelated to each other, could not.

On March 12, Watson sent Delbrück a letter, illustrated with rough sketches, discussing their new model. He warned his mentor not to tell Pauling about it until they were more certain of their results, but Delbrück, never one to keep secrets, immediately showed the letter around. Pauling's mind raced as he read it. He saw immediately that the Cavendish structure was not only chemically reasonable but biologically intriguing. "The simplicity of the structural complementariness of the two pyrimidines and their corresponding purines was a surprise to me—a pleasant one, of course, because of the great illumination it threw on the problem of the mechanism of heredity," he said. In it he could see echoes of many of the things he had been thinking and writing about complementarity since his 1940 paper with Delbrück.

The same day that Alex Rich *{who worked in Pauling's lab}* first heard about the Watson-Crick structure, he awoke in the middle of the night, got out of bed, went into his office, and began building a rough version of the Watson-Crick double helix out of the pieces of molecular models he had there. All he knew was that they had paired the DNA bases across the center of the molecule, but knowing that was enough. He quickly paired the correct bases, saw that it worked beautifully, and went back to bed shaking his head.

Pauling, while not yet ready to concede the race, was impressed. A few days after seeing Watson's letter, he wrote a colleague, "You must, of course, recognize that our proposed structure is nothing more than a proposed structure. There is a chance that it is right, but it will probably be two or three years before we can be reasonably sure. . . ." A few days later, he received an advance copy of the Watson and Crick manuscript, which started by attacking his DNA model and ended by thanking Jerry Donohue for his help. Pauling looked it over and wrote his son, "I think that it is fine that there are now two proposed structures for nucleic acid, and I am looking forward to finding out what the decision will be as to which is incorrect. Without doubt the King's-College data will eliminate one or the other."

He still had not seen any of Franklin's or Wilkins's recent x-ray photos and withheld final judgment until he did. His chance would come soon: He was planning to go to Brussels in April for a Solvay Conference on proteins and intended to stop off in England on the way to see the Watson-Crick model and the photos from Wilkins's and Franklin's laboratories. When he applied for a passport, his old nemesis Ruth Shipley *{head of the State Department's passport division}* again recommended denial, this time based on her belief that Pauling's Industrial Employment Review Board (IERB) testimony proved that he was refusing to be considered for top-secret clearance. After Pauling explained that he had been cleared for top-secret material

He was amazed that this unlikely team, an adolescent postdoc and an elderly graduate student, had come up with so elegant a solution to so important a structure.

in the past and would be willing to be again, but only if it was required for his work—and after he once more swore in her presence that he was not a Communist—his passport was approved.

In early April, a few days after Crick and Watson submitted their paper for publication, Pauling arrived in Cambridge. After spending the night with Peter, he walked into Crick's office and for the first time saw the three-dimensional model they had wired together out of die-cut metal plates. Crick chattered nervously about the features of the double helix while Pauling scrutinized it. He then examined Franklin's photo of the extended form of the molecule. Watson and Crick waited. Then, "gracefully," Watson remembered, "he gave the opinion that we had the answer."

It was a joyful moment for the two young men and a deflating one for Pauling. He was amazed that this unlikely team, an adolescent postdoc and an elderly graduate student, had come up with so elegant a solution to so important a structure. If they were right, his own model was a monstrous mistake, built inside out with the wrong number of chains. But he recognized now that the Cavendish team was almost certainly right.

There was only one thing left for him to do: Show the world how to handle defeat with style.

Pauling left Crick's office and met Bragg for lunch, during which Sir Lawrence vainly tried to restrain his ebullience. After so many years of coming in second, his team had finally beaten Pauling! Later, Pauling joined the Cricks at a pleasant dinner at their house at Portugal Place. Through it all he remained charming and funny and remarkably accepting of the new DNA structure, a true gentleman, both wise enough to recognize defeat and great enough to accept it with good humor. A day or two later both Bragg and Pauling went to the Solvay meeting—an occasional select gathering of the world's top researchers funded by a Belgian industrialist—where Bragg provided the first public announcement of the double helix. Pauling was generous in his support. "Although it is only two months since Professor Corey and I published our proposed structure for nucleic acid, I think that we must admit that it is probably wrong," he told the group. "Although some refinement might be made, I feel that it is very likely that the Watson-Crick structure is essentially correct."

Watson (left) and Crick show off their DNA model, which they had wired together out of die-cut metal plates.

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(There was no shortage of opinions as to what had gone wrong—from ignoring the molecule's biological

function to ignoring others' results. Pauling himself blamed the x-ray photos he had used, his misreading of DNA's density, and his lack of knowledge about purines and pyrimidines.}

Each excuse contained a measure of truth. But each was a symptom of a problem, not the problem itself.

There were two reasons Pauling failed with DNA: hurry and hubris. He rushed because DNA was the biggest prize around and if he did not crack it, someone else—probably someone in England—soon would. Although he later denied he was competing with the British researchers for the DNA structure—"I did not feel that I was in a race with Watson and Crick," he said. "They felt that they were in a race with me"—the fact was that he *was* in a race, perhaps not with the unknown Watson and Crick but certainly with Wilkins and Franklin and, above all, with his oldest rival, Sir William Lawrence Bragg. Pauling wanted to publish his DNA structure quickly in order to beat Bragg's group, and Wilkins, too, and he took a chance doing it without having done his homework.

In certain cases he had to trust himself, not the experimental results. He had to trust his intuition, his nose for a good structure.

Pauling had no precise structures for the nucleotide subunits. The x-ray photos he used, those that Astbury had done years before, were muddy and vague, and Pauling never attempted to make x-ray photos of his own prior to publication. He started with one idea, the phosphate-core model, and never deviated from it. No three-dimensional models were ever built. Pauling did not even have Corey check his figures a final time before sending in the paper. He wanted the credit for solving DNA, and to get it he had to publish first.

More importantly, he rushed because he thought he could get away with it. His success with the alpha helix had given him faith that he could jump ahead successfully. All of the basic assumptions that he had made in the late 1930s had been right; 15 years of further research had only proved it. He was right about hydrogen bonding and the planar peptide bond and the nonintegral repeat. As long as he stuck with what he knew about chemistry, he was always right.

The alpha helix had graced him with success and cursed him with overweening pride. After its solution, he believed he no longer needed to do the homework required by others. It was clear that he was the best person in the world at solving the structure of giant molecules—any molecules, for that matter. He knew that he had put together the correct basic structure of the alpha helix two years before he published it, two long years during which Bragg might have come up with the answer and beaten him to it. Pauling had hesitated then because of his doubts about the 5.1-angstrom x-ray reflection, an experimental observation that turned out to be irrelevant. The lesson was clear: In certain cases he had to trust himself, not the experimental results. He had to trust his intuition, his nose for a good structure. He knew that his triple-stranded DNA structure was very tight and that it begged the question of how the negatively charged phosphates could keep from repelling each other, but he believed that those matters would work themselves out, as the missing reflection in his alpha helix had worked itself out as a matter of coiled coils. The phosphate packing in the center of his model was too pretty, too clever not to be right.

He wanted the prize, he gambled, and he lost.

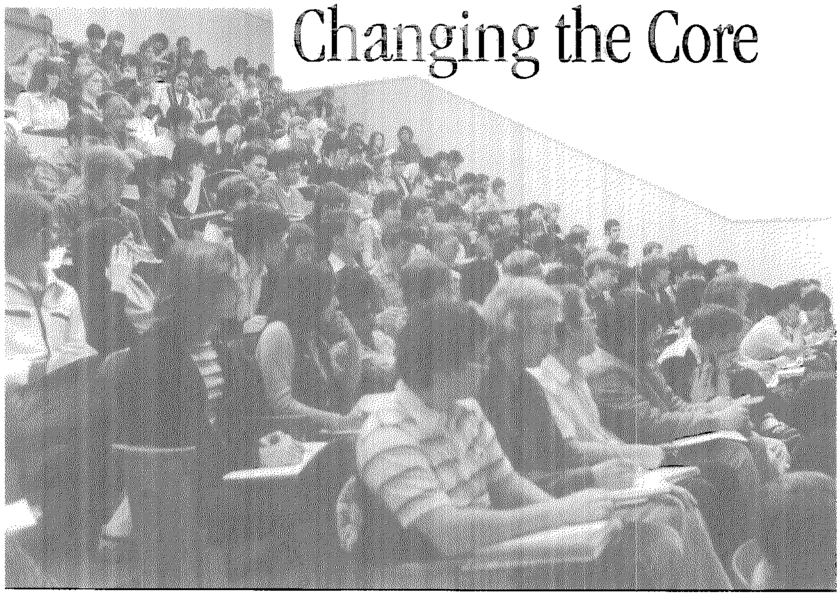
He regretted it, of course, the remainder of his life, although he was soon back to his usual cheerful self around the lab. Within a few months he could joke with Alex Rich about it, asking him how his new project on a special form of DNA was going, then adding, "You work hard on that problem, Alex, because I like *most* of the important discoveries to be made in Pasadena."

The encounter with DNA would become the stuff of legend in the literature that would spring up around its discovery. Watson and Crick would take center stage, with Pauling assuming the smaller part of an offstage voice, a legendary Goliath in a far land felled by two unlikely Davids. A year would rarely go by after 1953 without someone, a scientist or writer, asking him where he had gone wrong.

Ava Helen finally tired of it. After hearing the questions and explanations over and again, she cut through the excuses with a simple question. "If that was such an important problem," she asked her husband, "why didn't you work harder on it?" □

Thomas Hager, who is director of the Office of Communications at the University of Oregon, wrote this biography with extensive cooperation from Pauling himself before his death in August 1994 at the age of 93. Pauling granted Hager hours of interviews and access to private papers, correspondence, and diaries; in addition to scores of interviews with Pauling's family, colleagues, and others, Hager also consulted previously unreleased FBI and State Department documents. Force of Nature *can be ordered from the Caltech Bookstore (Mail Code 1-51, Pasadena, CA 91125) for $35.00; add $6.50 for shipping and handling.*

Changing the Core



by David J. Stevenson

Freshman physics in 201 East Bridge in 1980.

"If a man does not learn his physics, chemistry, and mathematics in college, he never learns it."

Among the distinctive features of Caltech is one that its alumni and students know all too well: the demanding set of math and science courses that all Caltech undergraduates since the 1920s have taken in their first two years. This set of courses, known as the core curriculum, or simply the core, is going to change beginning with the next freshman class—fittingly the class of 2000.

The modern Caltech, which began in the 1920s with the arrival of Robert A. Millikan, emphasized from the outset a rigorous training in basic science and math. Millikan himself told a Caltech audience in 1920: "If a man does not learn his physics, chemistry, and mathematics in college, he never learns it." Consequently, the 1920–21 course catalog articulated that "a thorough training in mathematics, physics, and chemistry must precede the application" of other sciences (which were thought of then as engineering and applied science), and, therefore that, "the first two years are given over to a common training." Also in 1921, the Board of Trustees stated our educational mission as being "to train the creative type of scientist or engineer urgently needed in our educational, governmental and industrial development." For the most part, this mission seems as appropriate now as then, although some might hesitate to embrace the sentiment behind the phrase "governmental and industrial development," given the current concern about limits to growth in those areas.

Certainly, the need for creative scientists and engineers is as great now as ever. The world, however, has changed immensely in the past 70 years, many of the changes occurring in just the recent decades. The scientific enterprise has grown enormously, driven in substantial part by the technological needs of the Second World War and later conflicts, including the Cold War—the need for nuclear weapons, radar, and jet aircraft, for example. During this period, physics was in the ascendancy, but more recently we have seen the explosive development and increasing importance of biological science, and the growing recognition of environmental issues, which dominate so many of the world's science policy decisions. Although Caltech has not grown very much, alumni and faculty have participated considerably in these changes. But has the core curriculum kept pace?

The current core consists of two years each of math and physics and one year of chemistry, pretty much what it was in Millikan's day. There is no required biology, earth science, or astronomy. Actually, Caltech did require courses in geology in the forties, and some options continued to require geology and biology for some time, but the trend in the last few decades has been toward fewer requirements and a more flexible curriculum. It is possible for a student to graduate from Caltech knowing little or nothing about biology, the area that now occupies more of the global scientific community than all other areas combined. The biology that students encounter in high school is highly variable but often conveys little of the intellectual groundings of modern biology, including molecular biology. Many students also encounter little or no earth science and environmental science in

ALL COURSES

FIRST YEAR

For Classes Entering September, 1922, and Thereafter

SUBJECTS	Subject Number	Hours per Week			Units
		Class	Lab.	Prep.	
I. FRESHMAN YEAR					
REQUIRED (Throughout the Year)					
Physics	401-403	2	4	3	9
Chemistry	301,302 311	3	6	3	12
Mathematics	453-456	3	0	6	9
English and History	601-603	3	0	6	9
Orientation	771-773	1	0	1	2
Drawing	701-703	0	6	0	6
Physical Education	....	0	3	0	3
Military Science	781-783	1	2	1	4
Shop Work[1]	741-744	0	4	0	4

Although "thereafter" sounds ominous, clearly the core has been tinkered with since Millikan first decreed it in this course catalog (*Bulletin*) of December 1920. Modern students are at least spared orientation, drawing, military science, and shop work.

high school and have little notion about the quantitative and intellectual basis of these areas of science.

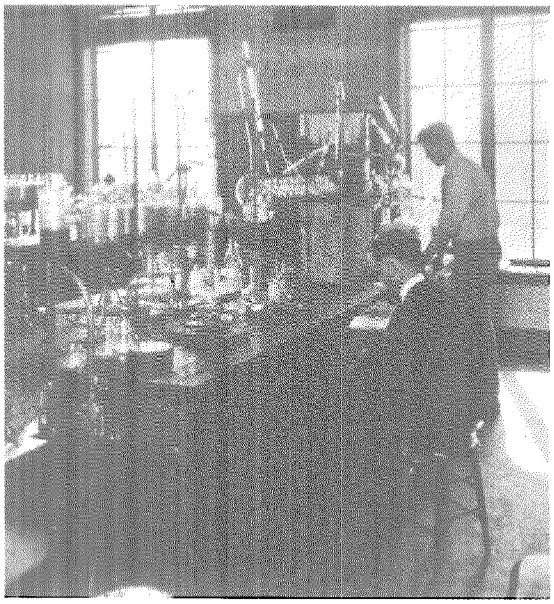
Important features are shared by many of the sciences outside the current core: they make a practical application of the basic sciences; they deal with complex systems; they require skills and ways of thinking that may not be evident or strongly encouraged in the current core; and they are frequently information-rich, or at least deal with large amounts of data from which one must extract information. Physics, for example, seeks to identify a small number of laws whose operation can often be exemplified in just small amounts of well-chosen data. Biology and astronomy, on the other hand, are examples of sciences that often seem to make sense of very large amounts of data, which the computer revolution has now made it possible to process. It is important in today's world for a well-educated scientist to be aware of the issues and approaches of these other areas of science, even though they may continue to choose to be an electrical engineer or a physicist. This is necessary simply to be scientifically and technologically literate.

Caltech faculty, like Caltech students, do not have a monolithic view of what the core should contain and accomplish. Certainly the current core is strongly oriented toward the acquisition of tools, and the large enrollments in de facto core courses in computer science and applied mathematics shows that there is widespread acceptance of the importance of basic tools, no matter which option is chosen. It is also widely accepted that the existing basic physics, math, and chemistry courses have served our students well. The challenge, then, is to find a balance between an existing core that still seems to work, and the desire to introduce elements of other sciences, without imposing too many requirements on the students. Core curriculum reform is also needed to address shortcomings in the coordination or coherence of current core courses, and to consider new ways of teaching science and of improving the ability of students to communicate their science to others.

The daunting process of changing a 70-year-old tradition has not been undertaken lightly. It began in 1992, with the Academic Policies Committee, chaired by Professor of Aeronautics Tony Leonard. The committee, which included students and which sought input from students and alumni, had by the end of 1993 distilled the diversity of opinion into three basic views. About the only thing those views had in common was to leave the humanities and social science requirement untouched (which would have pleased Millikan, who had insisted on it in the first place). One of the viewpoints held that the status quo worked just fine, and minor tinkering would bring it up to date. The second, dubbed the "minimalist" view, wanted the smallest possible core, arguing that the individual options had the best understanding of what their students needed, and that there was little need for a common core for all students, irrespective of option. The third, labeled the "fundamentals" approach, thought that the core curriculum should provide a broad education that would allow students to address the complex, interdisciplinary issues that today's scientists and engineers must learn to deal with.

Faculty Board meetings in early 1994 were devoted to what the minutes describe as "lively and fruitful discussions," during which proponents of the two more revolutionary proposals argued passionately in favor of their views. Ultimately, a proposal based on the fundamentals approach won out, but in deference to the strong sentiment against increasing the size of the core, it was agreed that the total units of the core would stay the same.

The Academic Policies Committee also submitted a number of related proposals to the Faculty Board; the one that received strongest support proposed placing the core curriculum firmly in the hands of the Institute faculty at large rather than in the hands of the individual options. This is consistent with the "fundamentals" approach. It means that the content of the core mathematics courses will not be solely determined by the traditional instructors of those



Chemistry lab in 1923.

It is important in today's world for a well-educated scientist to be aware of the issues and approaches of these other areas of science, even though they may continue to choose to be an electrical engineer or physicist.

courses (primarily pure mathematicians), or the physics courses by the physicists or the chemistry courses by the chemists. This decision required setting up a group—now called the Core Curriculum Council—to oversee the core, decide on its content, and select the best instructors for the courses.

In 1994–95, a task force cochaired by Harry Gray (the Beckman Professor of Chemistry and director of the Beckman Institute) and David Goodstein (professor of physics and applied physics, the Gilloon Distinguished Teaching and Service Professor, and vice provost) proposed changing from pass/fail to letter grades in the third term of the freshman year. The pass/fail system for freshmen did not begin in the ancient past with Millikan, but had been instituted in 1966–67 on an experimental basis, which lengthened into a 30-year tradition. The philosophy behind pass/fail was to instill in students the importance of their first-year work, while still providing them with the opportunity to settle into an environment that is vastly different from that of their high school. The change to letter grades for the third term of the freshman year has already been implemented for the current class, but it is too soon to know the full consequences. The Gray-Goodstein task force also suggested some specific ideas about how the core curriculum should change—reducing core physics by 9 units (from 54 to 45), math by 9 units (from 54 to 45), and chemistry by 3 units (from 24 to 21, 6 of which are required freshman lab), thus freeing up 21 units and providing the opportunity to insert some new courses. These new courses, in areas such as biology and earth science, are often referred to as menu courses, since it is likely that there will be some limited choice available to the students as to which ones they will take.

The basic structure of this proposal was accepted by the faculty, and the implementation was left to the Core Curriculum Council, which I currently chair. This council has about 30 members, including the current instructors of core courses and four students. Since a committee of 30 would be too unwieldy to get much done, we also have a steering committee, which will be doing the bulk of the work of defining the new curriculum and will use the larger council as a sounding board and source of advice. The current steering committee membership is Jacqueline Barton (professor of chemistry), Roger Blandford (Tolman Professor of Theoretical Astrophysics), Charles Brokaw (professor of biology), David Goodwin (associate professor of mechanical engineering and applied physics); Richard McKelvey (professor of political science), Barry Simon (IBM Professor of Mathematics and Theoretical Physics), two student members, Stephanie Haussmann and Alison Slemp (both of whom are seniors in biology), and Tony Leonard and myself.

Before defining the new menu courses, the steering committee had to figure out when to schedule them. Should they show up in the third term of the freshman year or first term of the sophomore year or even later? To the extent that they may serve to guide students in their choice of options, an early scheduling is preferable. On the other hand, the basic tools in math, physics, and chemistry also need to be properly covered. So, first the committee had to decide on how to structure and modify the existing math, physics, and chemistry requirements to fit into their new reduced number of units.

The proposed implementation is still under discussion, but the following less controversial aspects appear to have wide support: first-year math and physics will probably continue to occupy all three terms; the contraction in math and physics will likely occur in the third term of the sophomore year, and will be accomplished by the judicious removal of particular topics scattered throughout the current syllabi, rather than by wholesale amputation, or by speeding up the delivery of material; all students will be required to take two new courses in areas of science that are not currently part of the core, and these new courses will be offered in the third term of the sophomore year. Because there will no longer be a required chemistry class third term, it will be possible for students to take both new courses in

FRESHMAN YEAR

Current		Proposed	
First Term		**First Term**	
Ma1a	9	Ma1a	9
Ph1a	9	Ph1a	9
Ch1a	6	Ch1a	6
CS1**	6	CS1**	6
HSS	9	HSS	9
PE	3	PE	3
	42		42
Second Term		**Second Term**	
Ma1b	9	Ma1b	9
Ph1b	9	Ph1b	9
Ch1b	6	Ch1b	9
HSS	9	HSS	9
Chem3a	6	Chem3a	6
PE	3	PE	3
	42		45
Third Term		**Third Term**	
Ma1c	9	Ma1c	9
Ph1c	9	Ph1c	9
Ch1c	6	Bi1	9
HSS	9	HSS	9
Elective	6 or 9	Elective or menu	6 or 9
PE	3	PE	3
	42 or 45		45 or 48

SOPHOMORE YEAR

Current		Proposed	
First Term		**First Term**	
Ma2a	9	Ma2a	9
Ph2	9	Ph2a	9
Science*	18	Science*	18
HSS	9	HSS	9
	45		45
Second Term		**Second Term**	
Ma2b	9	Ma2b	9
Ph2b	9	Ph2b	9
Science*	18	Science*	18
HSS	9	HSS	9
	45		45
Third Term		**Third Term**	
Ma2c	9	Science*	27 to 36
Ph2c	9	Menu Course•	9 or 0
Science*	18	HSS	9
HSS	9		45
	45		

Although nothing is yet set in stone, here is an example of what a schedule under the new core might look like, compared to the current one. The net increase in the freshman year is three units in both the second and third term. Since most students do actually take 45 units or more, this is not out of line, although it does reduce flexibility. *Refers to all noncore science courses, whether electives or option requirements. **The introductory computer science course is not required in the current core, but almost everyone takes it anyway. •If the menu course (e.g. astronomy or Earth and environment) was not taken freshman year.

their freshman year or one in each of their first two years. The structure of the menu is, of course, a very important issue and is still being debated. Should biology be required? Some have argued that the best way to succeed with the menu is to provide choices but put much effort into assuring that the choices are so enticing that the goals are accomplished without coercion. Others say that biology is so important and ties in so well with the (now shrunken) chemistry core that it ought to be required. The current proposal under consideration includes biology as a required course in the third quarter of the freshman year.

There are still other aspects of core curriculum reform that have not yet been discussed by the committee at length. We hope to introduce a short course on science communication for all students. We also hope to be able to to introduce innovative approaches to teaching, both in the classroom and through utilizing material available on the World Wide Web. Perhaps the new structure will also encourage greater interplay between the sciences than is currently evident—for example, the use of biological and chemical examples in the teaching of basic physics. Last but not least, we hope to draw on a wider pool of instructors, so that, for example, core math classes might be taught by people who are not part of the pure math faculty.

Of course, it's desirable and appropriate that there be some experiments in this evolutionary process of curriculum revision; an "experiment" is something that can be abandoned later if it doesn't work. The reform under way obviously presents great challenges, not the least being the students' views. When I talk to students, I find many to be very conservative in their views on the core. The views many of them express about biology or other currently noncore areas seem to have been determined by high school experiences or by existing course offerings (although the new menu courses may not even remotely resemble existing courses.) For example, some students have only vague ideas about the revolutionary nature of molecular biology and the extent to which the behavior and development of biological systems can now be quantified.

Alumni, although they may have intensely disliked some courses at the time, are also generally supportive of the traditional Caltech core and not always enthusiastic about proposed changes. But final decisions have yet to be made; this is a community effort and we welcome alumni views. The Core Curriculum Committee will be presenting its final recommendations to the Faculty Board in February, so alumni still have an opportunity to influence the outcome. You may e-mail me at djs@arms.caltech.edu or call me at 818-395-6534. □

Dave Stevenson is professor of planetary science and was recently appointed the first holder of the newly established George Van Osdol Professorship. Stevenson received his bachelor's and master's degrees from Victoria University in his native New Zealand and his PhD from Cornell. A member of the Caltech faculty since 1980, his research concerns the origin, evolution, and structure of the planets, including Earth.

Obituaries

Arrola DuBridge 1900–1995

Arrola Bush Cole DuBridge, the wife of Caltech's late president emeritus, Lee DuBridge, and an active and committed member of the Caltech community for more than 20 years, died on September 30 in Hingham, Massachusetts. She was 95.

Mrs. DuBridge had been associated with the Institute since 1974, when she and DuBridge, both widowed at the time, were married. The two had met many years earlier while attending Iowa's Cornell College, where they were classmates and friends. Dr. DuBridge, who served as Caltech's president from 1946 to 1969, died in January 1994.

Born on March 12, 1900, and raised in Iowa, the future Mrs. DuBridge—then Arrola Bush—earned degrees in psychology and English from Cornell College, where she met and married her first husband, Russell Cole. Cole served as president of Cornell College from 1943 to 1960, and, upon his retirement, the couple moved to Massachusetts. After his death, Arrola Cole became a social worker at the state's Correctional Institute for Women in Framingham. Later she spent five years as social director of Chapman College's "World Campus Afloat," a shipboard education program that travels around the world each semester.

For a time, Mrs. DuBridge was a scriptwriter for NBC radio and television. A talented violinist, she also taught violin and played in the symphony orchestra of Cedar Rapids, Iowa.

Arrola DuBridge gave generously of her time and support to a wide range of Institute activities, including the Caltech Women's Club, the Caltech Service League, the Caltech Associates, and the Caltech Alumni Association, which named her an Honorary Alumna in 1992. That same year, the Price Charities in San Diego endowed the Arrola DuBridge Scholarship Fund in her honor at the Institute. The scholarship provides funding for undergraduate women. Arrola and Lee DuBridge were widely known for their interest in and dedication to young people in science, a commitment the ARCS (Achievement Awards for College Scientists) Foundation recognized in 1992 by establishing the Lee and Arrola DuBridge Endowed Scholarship Fund to support undergraduates at Caltech.

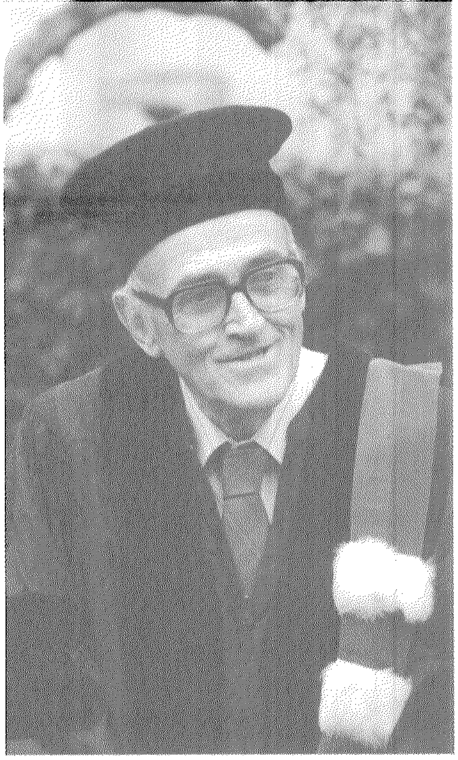
A memorial service was held on campus on November 15.

Clair C. Patterson 1922–1995

Clair C. "Pat" Patterson, professor of geochemistry, emeritus, died suddenly on December 5, at his home in The Sea Ranch, California, northwest of Santa Rosa. He was 73.

Patterson, who had a remarkable talent for finding the most important scientific problems and then solving them, is best known for his determination of the age of the earth and the solar system, and for his pioneering work on lead pollution in the modern world.

The passion that directed Patterson's research was his desire to better understand the geochemistry of metals in terrestrial rocks, waters, and atmospheres, in meteorites, and in the solar



system. Patterson was a pioneer in the study of lead in the earth's crust. He developed precise analytical techniques that enabled him to establish the true levels of preindustrial lead in the environment. His analysis of lead isotopes in meteorites and oceanic minerals led him in the early 1950s to conclude that the earth and solar system are 4.6 billion years old. This result is one of the most important measurements of time ever made. Current theories of stellar birth and evolution, and our very understanding of the history of the universe, are based in some measure on this important measurement.

While studying lead isotopes, Patterson found that human civilization had mined and dispersed an unprecedented amount of the metal around the world. Ice cores from the Greenland ice cap, dating back thousands of years, showed that the amount of lead in modern snow is much higher than in preindustrial times. This knowledge led Patterson to wonder whether this abundance of lead might affect humans. His studies of the bones and teeth of prehistoric people confirmed that modern humans contain up to 1,000 times more lead than did their ancient ancestors.

His message, that people were being contaminated by lead from water pipes, from leaded gasoline, and from the solder used to seal canned foods, was not popular. But Patterson was a courageous and determined man, and he knew that he was right. He fought, against great odds and the money of powerful corporations, to discontinue the use of lead in these materials, and eventually, through his tenacity and his extremely thorough methods, his results and recommendations were accepted.

Patterson was born in Des Moines, Iowa, and earned his bachelor's degree in chemistry at Grinnell College in Grinnell, Iowa, in 1943. He continued to study chemistry at the University of Iowa, where he earned his master's degree in 1944, and at the University of Chicago, where he completed his doctorate in 1951 with Harrison Brown as his thesis advisor. He stayed on at the University of Chicago as a postdoctoral fellow for one year, and when Brown came to Caltech to establish the geochemistry program in 1952, Patterson came with him as a research fellow. He was a senior research associate from 1973 until 1989, when he was named professor of geochemistry.

Among his many honors, Patterson received the J. Lawrence Smith Medal from the National Academy of Sciences in 1975 and the Professional Achievement Award of the University of Chicago in 1981. He was elected to the National Academy of Sciences in 1987, and has also had a peak in Antarctica and an asteroid named for him. Most recently, he won the 1995 Tyler Prize for Environmental Achievement, the premier international environmental honor in the world.

A memorial service is being planned.

Olga Taussky-Todd 1906–1995

Olga Taussky-Todd, professor of mathematics, emeritus, and one of the world's leading experts on algebraic number theory and matrix theory, died at her home in Pasadena on October 7.

Taussky-Todd was born in Olomouc (Olmütz) in the Moravian part of Czechoslovakia. She attended the Koernerschule in Linz, where her talent for mathematics was evident early. She later wrote in a personal memoir for the Caltech Archives: "Gradually it became clear to me that [mathematics] was to be my subject. However, I had no idea what that meant. First of all, I was fully conscious that the fact that I was doing well at school had nothing to do with it. The work at school was really not that difficult if one applied oneself to it, but it was so uninteresting that you could not wish to apply yourself. I felt there was another mathematics. I later found that the yearning for and the satisfaction gained from mathematical insight brings the subject near to art. While talent is undoubtedly needed by itself, it does not always make a person a mathematician."

Taussky-Todd clearly did have that yearning and that satisfaction from mathematical insight. She went on to study with the number theoretician Philip Furtwängler at the University of Vienna, where Kurt Gödel was a friend and fellow student ("Remembrances of Kurt Gödel," *E&S* Winter 1988). Taussky-Todd earned her PhD in 1930.

Olga Taussky-Todd

At the University of Göttingen in 1931–32, she served as an assistant to Richard Courant and edited the collected works of David Hilbert. She spent 1934–35 with Emmy Noether, one of the founders of modern algebra, at Bryn Mawr College in Pennsylvania. Noether also taught at Princeton, where Taussky-Todd frequently accompanied her, and it was here that she became deeply interested in topological algebra. She became one of the first to point out connections between abstract algebra and topology.

She was appointed to a Yarrow Research Fellowship at Girton College, Cambridge, in 1936, and continued her work in topological algebra, which was new to Cambridge at the time. She was awarded, *ad eundem*, the degree of MA by the University of Cambridge in 1937, only after Parliament had changed the statutes that theretofore had permitted the degree to be awarded to men only. In 1937 she took up a position at the University of London, where she met her future husband, fellow mathematician John Todd. They were married in 1938.

Both Todds worked for the British Ministry of Aircraft Production during World War II. After the war they came to the United States, working for the National Bureau of Standards for 10 years, most of the time in Washington but with periods at the bureau's field station at UCLA. Olga and Jack Todd received appointments to Caltech in 1957. She wrote: "When the invitation to Caltech came, I felt very pleased and honored, and I knew that I had stayed at the bureau long enough. Coming from a civil service job back to academic life meant a tremendous change, almost as much as the opposite change, which we had made years before. First of all, Caltech is a teaching institution, however high its research standards are. . . . I simply love to teach and feel that I have a good bit of natural talent for it." She was named professor of mathematics in 1971, having received tenure (the first woman to do so) in 1963. She became professor emeritus in 1977.

At a 1976 symposium at Caltech, Taussky-Todd was honored as one of the foremost living female mathematicians. She was elected to the Council of the American Mathematical Society in 1972 and elected vice president of the society in 1985; she was a fellow of the American Association for the Advancement of Science, a corresponding member of the Austrian Academy of Sciences and the Bavarian Academy of Sciences, and a recipient of the Golden Cross of Honor, First Class in Arts and Sciences, from the Austrian Government. The University of Southern California awarded her an honorary D.Sc. in 1988, and in 1963 she was selected as one of the 10 Women of the Year by the *Los Angeles Times*.

About this honor she later wrote: "Apart from the strain that the ceremonies and interviews inflicted on me, it gave me great pleasure. I knew that none of my colleagues could be jealous of it (since they were all men), and that it would strengthen my position at Caltech. My husband was delighted about it and enjoyed the ceremonies. Otherwise it did nothing to me. Recognition that has pleased me far more were those instances where a specific piece of my research or a lecture I had given were involved, or where something I had done for a student was involved." □

One Down, Six To Go

The astronomers identified brown dwarf candidates on the 60-inch telescope and examined them more closely with the Hale Telescope.

A team of astronomers from Caltech and the Johns Hopkins University have taken pictures and spectra of what they believe is a brown dwarf—one of a class of objects intermediate in mass between the smallest stars and the "gas giant" planets such as Jupiter. Astronomers have long theorized that brown dwarfs must exist, and in recent years several candidates have been nominated by various observers, but until now the proof has always been indirect and never 100 percent convincing. The team includes Professor of Astronomy Shrinivas Kulkarni, Senior Research Fellow in Astronomy Tadashi Nakajima, Member of the Professional Staff Keith Matthews, and grad student Ben Oppenheimer, of Caltech, and Sam Durrance and David Golimowski of Johns Hopkins.

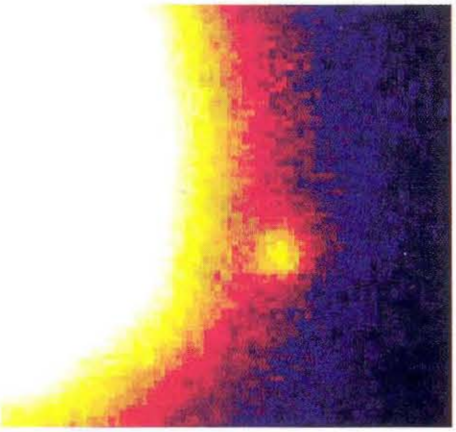
The brown dwarf, known as GL 229B, lies in the constellation Lepus, near Orion, and orbits a small, dim star called GL 229 that's 17 light-years—about 100 trillion miles—away from us. This is the first detection of so faint and cool an object outside the solar system.

Brown dwarfs are made of the same gaseous material as stars, but are much less massive. Current theories put the upper limit to the mass of brown dwarfs at about one-twelfth the mass of the sun. Above this limit, the energy released by the contracting gas generates enough heat to ignite and sustain nuclear fusion, and a star shines forth. Below this limit, the gas never gets hot enough to "burn." Young brown dwarfs can shine quite brightly for a little while, due to frictional heat from gravitational contraction, but this source of energy isn't nearly as long-lasting or as powerful as fusion. So brown dwarfs fade rapidly until they glow only from their meager internal heat, and are much cooler, dimmer, and harder to see than stars.

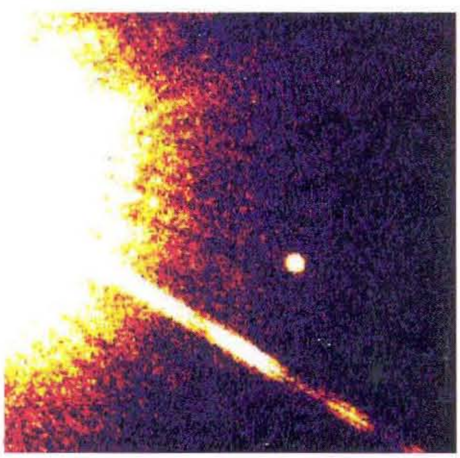
Astronomers want to find brown dwarfs for two reasons. First, they want to determine the smallest-mass object that can form in a starlike manner by condensation of interstellar gas clouds, and whether enough of these hard-to-detect objects exist to solve a difficult cosmological puzzle. Our galaxy is spinning so fast that it would fly apart if the gravity from its known contents were all that was holding it together. Could brown dwarfs account for some of the missing "dark matter?" Second, astronomers want to study the atmospheres of brown dwarfs and learn how they are related to the atmospheres of planets. Such understanding is important to the search for other planetary systems.

Because of their importance both to cosmology and to finding other planets, astronomers have searched diligently for brown dwarfs, especially young ones that are still hot, relatively bright, and more easily seen. Young brown dwarfs are most likely to appear in star clusters, the "nurseries" where stars form.

But the Caltech/Johns Hopkins team took a different approach. Instead of scouring relatively distant stellar nurseries for young brown dwarfs, they looked for older, cooler ones as companions to stars within our local neighborhood—within 45 light-years, or about 265



Above: The brown dwarf (dot in center) was first observed on October 27, 1994, using Johns Hopkins University's Adaptive Optics Coronagraph and the 60-inch telescope at Palomar. (Photo by Nakajima and Golimowski.) Below: This follow-up image was made on November 17, 1995, with the Hubble Space Telescope's Wide-Field and Planetary Camera 2. (Photo by Kulkarni and Durrance.) In both cases, the main star is out of the field of view to the left, but residual glare extends out nearly to the brown dwarf.



trillion miles, of the sun. These stars are middle-aged, on average about five billion years old.

There are two advantages to searching for nearer, older brown dwarfs. First, scientists know the distances to nearby stars pretty accurately, so a brown dwarf candidate's intrinsic luminosity can be deduced. The lowest luminosity of any normal, hydrogen-fusing star is one ten-thousandth that of the sun, so if the candidate's calculated luminosity is less than this limit, the object can't be a star. But brown dwarfs can have much lower luminosities.

Second, an aged brown dwarf's spectral features may help unmask it. A star's minimum surface temperature is about 1800 K, while old brown dwarfs can have much lower temperatures. Thus their cool atmospheres are similar to those of the gas-giant planets in our solar system. (In fact, prominent absorption features are seen in Jupiter's spectrum that do not appear in the spectrum of any star.) And Takashi Tsuji of the University of Tokyo has found that below 1000 K, carbon prefers to attach to hydrogen and form methane, instead of reacting with oxygen to form the carbon monoxide seen in cool stars. So the presence of methane absorption lines in a candidate's spectrum are a sure sign of less-than-stellar temperatures.

The astronomers first made an image of each of the stars in their survey with a "coronagraph," a camera with the ability to see faint objects in the glare of an adjacent bright star. The coronagraph blocks light from the star so that dimmer nearby objects become visible. This coronagraph, used at optical wavelengths, was made by the Johns Hopkins team and has been used extensively at the 60-inch telescope at Caltech's Palomar Observatory. A similar device built by Matthews to detect infrared wavelengths has been commissioned recently at the 200-inch Hale Telescope at Palomar.

The astronomers looked at each star twice, at an interval of one year. All stars move relatively quickly, so unrelated objects that were lined up by chance in the first look will have drifted apart by the second. But true companion stars will remain together. The astronomers identified brown dwarf candidates on the 60-inch telescope and examined them more closely with the Hale Telescope.

This method paid off with GL 229. Its putative companion, christened GL 229B, moved in tandem with it, and the two appear to be in orbit around each other. Using the known distance to GL 229, the astronomers calculated GL 229B's luminosity to be only seven millionths that of the sun, almost 10 times less than the faintest known star. And absorption lines betraying the presence of water were found in the spectrum of GL 229B, showing that its surface temperature is less than 1,000 K—800 K lower than the coolest known star. Methane absorption lines were also found, confirming the object's substellar temperature.

This discovery is an important first step in the search for planetary systems beyond the solar system. GL 229B's strange colors—extremely red in the optical wavelengths and blue in the near-infrared—and the presence of methane suggest new strategies to search for brown dwarfs and massive Jupiter-like planets. The spectra of faint objects could be screened for these unusual characteristics, allowing astronomers to concentrate on the most likely brown dwarf candidates.

GL 229B is about four billion miles from its main star, a bit farther than Pluto is from the sun. Although its mass is some 20 times that of Jupiter, it is so dense that its diameter is about the same—80,000 miles. It's unclear whether GL 229B formed like a star, by direct condensation of interstellar gas, or like a planet, by condensation of material within a protoplanetary disk that formed around the star. However, the proximity of the parent star to the companion suggests that it formed in a planetary disk rather than directly from the interstellar medium. The astronomers are continuing to observe GL 229B, and have obtained images and spectra of it using the Hale Telescope, the 10-meter Keck Telescope, and the Hubble Space Telescope.

The results appeared in the November 30 issue of *Nature* and the December 1 issue of *Science*. □

Below: The arrow on this Hubble Space Telescope image marks where the probe hit, at approximate Jovian latitude 6.5° north and longitude 4.4° west. This photo dates from October 5, however, and the cloud patterns may have changed significantly in the interim.
Right: Talk about your visual aids! In the JPL pressroom on Arrival Day, TV reporters did their standup routine under a life-sized replica of the spacecraft.

Galileo Hits the Spot

Well, no, it didn't—not the Great Red Spot, anyhow. But on December 7, after a six-year voyage filled with dramas too numerous to mention, the Galileo spacecraft buzzed Jupiter's pizza-faced volcanic moon Io at an altitude of some 890 kilometers, skimmed the giant planet's cloud tops by a distance of three Jupiter radii, and fired the main engine to plop itself into permanent orbit around its new home. Meanwhile, an atmospheric probe dropped from the spacecraft back in July did hit a spot, or very nearly, plunging into the planet's roiling skies close to the outskirts of a "hot spot" visible in infrared light.

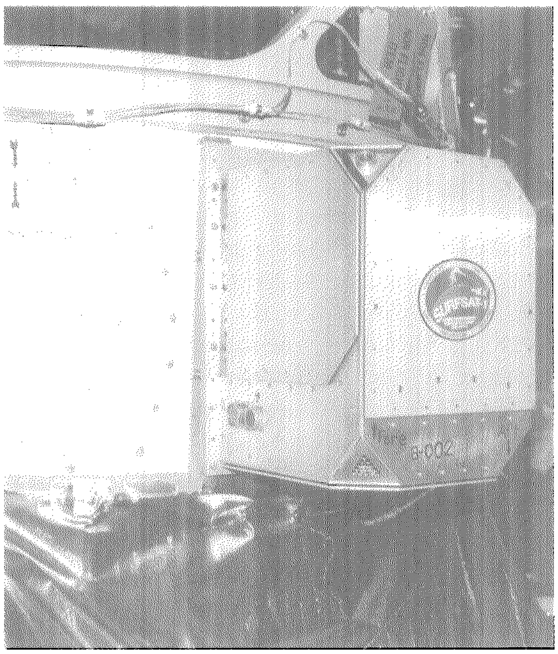
The probe, which is managed by NASA Ames, entered the top of Jupiter's atmosphere—defined as 450 kilometers above the altitude where Jupiter's atmospheric pressure equals that on Earth's surface—at an angle to the horizon of roughly 8.3°. This was the center of a very narrow safe zone—a degree and a half shallower, and the probe would have skipped off the atmosphere like a pebble across a pond; a degree and a half steeper, and the probe would have taken too great a jolt for its instruments to withstand. Not that they're wimpy, mind you—they rode out a deceleration shock some 215 times the force of gravity here on Earth.

Then, heat shields jettisoned and parachute deployed, the probe's instruments took the measure of Jupiter's atmosphere—its pressure, temperature, and chemical makeup—and sent 57 minutes' worth of data to the orbiter.

Due to the famous antenna problem, however, the data could not be relayed to Earth in real time, but were stored on tape for later playback. A compressed version of the data was also stored in Galileo's computer, in case the tape recorder decided to do something exciting again. This meant that plans to take extreme close-ups of the moon with the worst case of acne in the solar system had to be scrapped. This is unfortunate, because Galileo won't come close to Io again—the radiation environment there is just too hot for the spacecraft.

The first 40 minutes of probe data were relayed to Earth on December 10–13, but the full playback won't start until January, 1996, once Jupiter clears the sun. (Jupiter hits superior conjunction—meaning that it's diametrically opposite us, behind the sun—on December 19. And as the sun gets in the way, radio communication with Galileo goes from normal to lousy to nonexistent.) Stay tuned... □—*DS*

Radical Stick, Dude!



Above: The SURFSAT boxes were mated to the rocket on pad SLC-2W at Vandenberg Air Force Base about a week before launch. The job isn't quite done yet in this photo—the metal panels protecting the solar cells have yet to be removed.

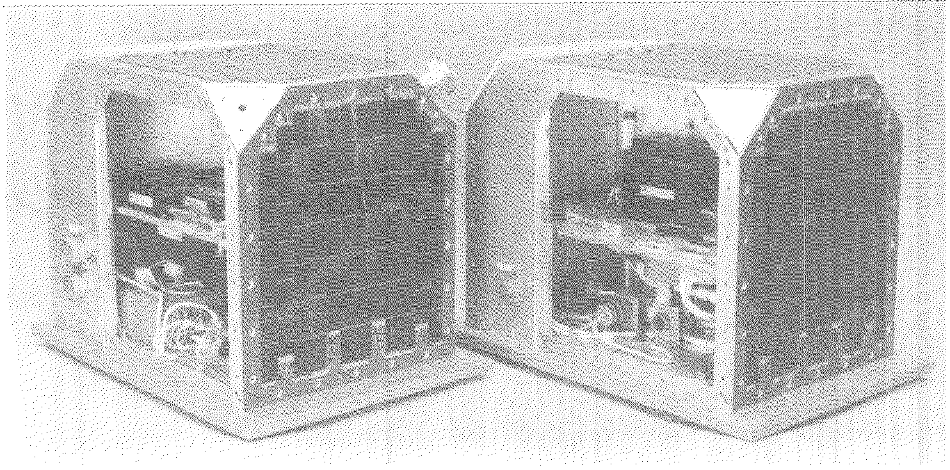
Some folks surf at Zuma, some surf the Internet, and now there's a SURFer dude in space. His buzz-cut, sunglassed visage adorns the Delta II rocket that lofted the Canadian RADARSAT into polar orbit from Vandenberg Air Force Base last November 4. Bolted to the Delta's second stage are two aluminum boxes, each the size of the proverbial bread box, that were designed and built by nearly a decade's worth of SURF (Summer Undergraduate Research Fellowship) students and others. The boxes, collectively known as SURFSAT-1, are being used by engineers at Caltech's Jet Propulsion Laboratory (JPL), to test new technology for use in the Deep Space Network, the worldwide system of antennas that is our communications link with JPL's armada of far-flung spacecraft such as Voyager and Galileo.

Commands are sent to these spacecraft, and data returned, over a chunk of the radio spectrum called the X band. But in 1979, JPL was allotted another region called the Ka band, which can, in theory, carry up to 14.4 times more data. (The names were assigned by the bands' original military users, and serve primarily to confuse people.) However, the Ka band demands that ground stations using it point their antennas much more accurately, and it's also more sensitive to atmospheric effects during bad weather.

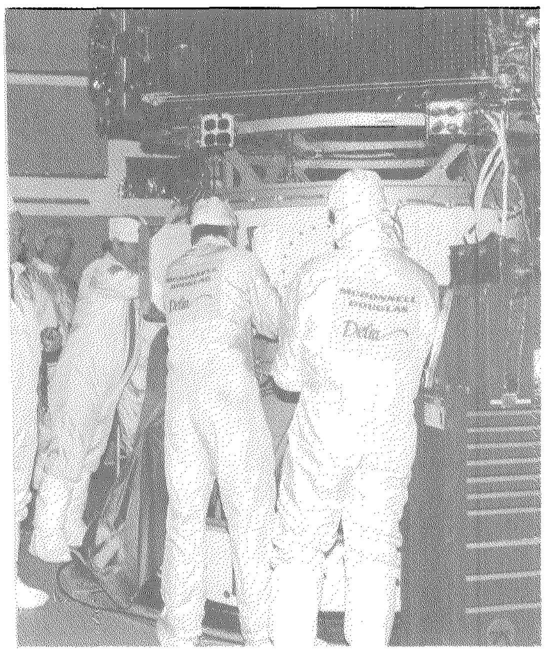
So before outfitting all future deep-space probes with Ka-band radios, the engineers wanted to see how their tracking equipment would really perform. What they needed for this was an orbiting Ka-band source that emitted a very weak signal. "What we needed," recalls Project Manager Joel Smith, "was a small, cheap satellite. And, of course, we were laughing, because there were no such things, and in comes Ed Posner [then a visiting professor of electrical engineering at Caltech, and JPL's chief technologist for telecommunications and data acquisition], who said, 'Hey guys, I've got a bunch of students coming this summer. Do you have any good jobs for them?' And we said, 'You bet!'" Six SURF students were hired that year—1987—to begin designing the spacecraft. Posner eventually cosponsored 43 SURFSAT students before his untimely death in 1993. In all, more than 60 students from 14 colleges in the US and UK were involved.

The original plan was to fly an X-band and a Ka-band transmitter that would continuously broadcast weak (microwatt) signals that the ground stations would try to lock on to and track. The performance of the two carriers could then be compared.

Meanwhile, the Deep Space Network was gearing up for a pair of international orbiting very-long-baseline interferometry missions. The idea here is to have a spaceborne radio telescope—either Russia's RadioAstron or Japan's VSOP satellites—observe a distant quasar, or whatever, at the same time that a ground-based antenna does. The signals are combined in a computer, and the result is a "virtual" radio dish whose effective diameter is the distance between the ground station and the satellite. In order to combine the signals properly, however, you need to know their arrival times at both receivers to within a billionth of a second, which means that a time signal has to be sent continuously from the ground station to the spacecraft. This had never been done routinely before, so SURFSAT was also volunteered as a test receiver for the time signals. Its transmitters were traded in for transponders to permit two-way communication—otherwise the ground crew would have no way of knowing if



Above: The box at left is the primary payload, housing the X-band and Ka-band transponder. The other box contains the Ku-band unit. The X band spans the frequencies between 7,000 and 8,500 million cycles per second, the Ka band runs from 31,800 to 32,300 million, and the Ku band is at 14,100–15,300 million.



Above: Antsos is second from the left in the crowd, through which one of the SURFSAT boxes can be seen at shoulder height. The entire second stage is about six feet in diameter and 18 feet tall.

the time signals were getting through. (The Russian satellite uses the X band, and a new, Ku-band transponder was added to accommodate the Japanese.)

At that point in late 1993, SURFSAT acquired a task manager, Steve Johnson, and became a real Flight Project. It was his job to shepherd SURFSAT through the fabrication, test, and review process and turn the design into real hardware. Then he had to get the hardware flight-qualified and mated to the rocket. This meant leading a joint effort with folks at McDonnell-Douglas Aerospace, which manufactures and launches the Delta, and Darren Bedell and Marissa Achee of Orbital Launch Services at NASA's Goddard Spaceflight Center, who made sure that nothing SURFSAT could do would harm RADARSAT in any way, and figured out how to attach SURFSAT to the rocket.

SURFSAT's design is an essay in simplicity. Each of the two boxes is a self-contained payload—one houses the X- and Ka-band transponder and the other the Ku-band transponder. Each box is shingled with solar cells—the only on-board power source—which are kept illuminated by the spacecraft's sun-synchronous orbit. There are no thrusters, no attitude-control gyros; the transponders' fixed-view, wide-angle antennas are trained on Earth by gravity-gradient stabilization, which essentially means that the spent booster section that carries them is nose-heavy, so the front-facing antennas point down. And riding a dead rocket means that the SURFSAT team gets its position information for free. "The Air Force has to track us as space debris," Smith explains. "And they've agreed to tell us where we are once a day, so we can never get lost."

There isn't even an onboard computer. "We worked *hard* to keep it that simple. The students would want to make it more complicated. 'What if we put a boom out here? What if we added a battery? What if we had a timer?'" But sticking to basics and using commercially available, mil-spec parts kept the project's cost to under three million dollars—the least JPL has spent on a satellite since Explorer One was launched for two million in 1957. (Adjusting for inflation, SURFSAT even beats that.)

As the project matured into actual flight hardware over the last two years, it outgrew the SURF students—ten weeks a summer per student just wasn't sufficient. The project turned to COOP students—who work six months a year full-time at JPL and are full-time college students the other six months—and to JPL staff. One of the latter was Dimitri Antsos (BS '90, MS '91, PhD '93 [under Posner]), who had started on SURFSAT in 1988 as a sophomore, and had been working on it on and off ever since. JPL hired him outright the moment he got his PhD, and building the transponders became his first official responsibility. He; Greg Carr, who built the power systems; Jim Springett of Neocomm, who designed the other electronics; and JPL's Johnny Duong, who built them, put in a lot of overtime. So did scores of people at McDonnell-Douglas, at Vandenberg, and elsewhere, because, of course, things never go as smoothly as you'd expect. "SURFSAT really brought out the best in everybody," Smith says. "It was a marvelous program. Every time we got in trouble, people just came out of the woodwork to help the student spacecraft."

Now that SURFSAT is flying, everything is working just fine. The mission-operations crew expects to get all the data they need on the relative merits of the X and the Ka bands within a year, which coincides with Radio-Astron's and VSOP's planned launch dates. The crew originally planned to turn SURFSAT off when they were done with it, but now other people who have antennas to test want to borrow it. So JPL may leave it running as a kind of public resource for whoever wants to use it. Ed Posner would be proud. □—*DS*

Peter Goldreich, the Lee A. DuBridge Professor of Astrophysics and Planetary Physics, received the 1995 National Medal of Science from President Bill Clinton and Vice President Al Gore in Washington on October 18.

Honors and Awards

Fred Anson, the Elizabeth W. Gilloon Professor of Chemistry, has been named a fellow of The Electrochemical Society, Inc., for his work in the field, including new electrocatalysts for the electroeduction of dioxygen.

Jacqueline Barton, professor of chemistry, was selected to deliver the 1995 Havinga Lecture and to receive the Havinga Medal from the Stichting Havinga Foundation.

Erick Carreira, assistant professor of chemistry, has been awarded a $500,000 Packard Fellowship by the David and Lucile Packard Foundation. Carreira studies the use of asymmetric catalysts to produce compounds with the potential to be useful drugs.

Tom Heaton, professor of engineering seismology, has been granted the Meritorious Service Award of the Department of the Interior for his "contributions to technical developments and seismological research for the National Earthquake Hazards Reduction Program of the U. S. Geological Survey."

Michael Hoffmann, professor of environmental chemistry, has received the 1995 E. Gordon Young Award from the Chemical Institute of Canada for his scientific contributions in the field of environmental chemistry.

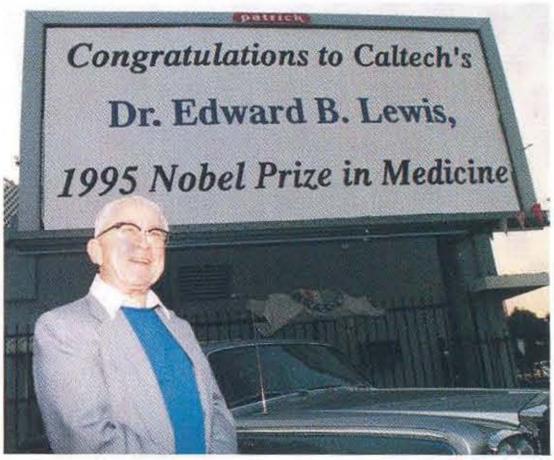
George Housner, the Carl F Braun Professor of Engineering, Emeritus, received the 1995 Lifetime Achievement Award at the Los Angeles Area Chamber of Commerce's 60th annual construction industries luncheon.

Wolfgang Knauss, professor of aeronautics and applied mechanics, has been elected a Fellow of the American Society of Mechanical Engineers.

Rudy Marcus, the Arthur Amos Noyes Professor of Chemistry and Nobel laureate, has been selected to receive the Treasure of Los Angeles award from the Central City Association. He has also been named honorary professor of the Institute of Science of the Chinese Academy of Sciences, and adviser to the Center for Molecular Sciences of the Academy and to the State Key Laboratory for Structural Chemistry of Unstable Species in Beijing.

Peter Wyllie, professor of geology, was elected president of the International Union of Geodesy and Geophysics.

Ahmed Zewail, the Linus Pauling Professor of Chemical Physics and professor of physics, has been selected the 1996 recipient of the Peter Debye Award in Physical Chemistry, sponsored by E. I. duPont de Nemours and Company, Inc.



Meanwhile, back at home, the city of Pasadena commissioned a billboard announcement of Ed Lewis's honor. Lewis also switched on the Christmas tree lights at the Pasadena city hall just before leaving for Stockholm.

Vice President Gore seems to have spent quite a bit of time with Caltech folks this fall. Here, in a White House reception for this year's crop of Nobelists on November 15, he greets Pamela Lewis, while her husband, Ed, who won the 1995 Nobel Prize in physiology or medicine, looks on.