California Institute
of Technology

Engineering & Science

Winter 1991

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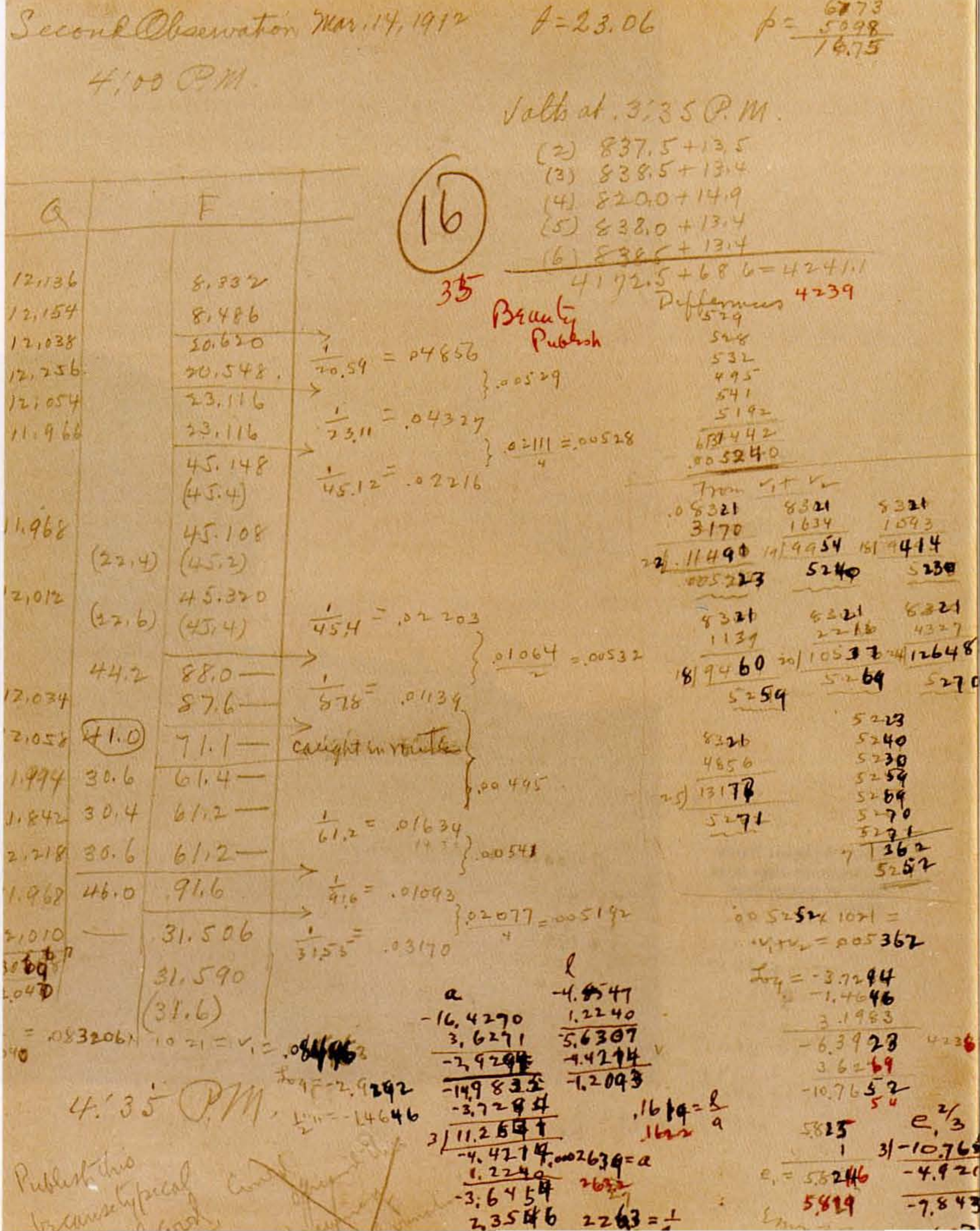
First Lights

Scientific Fraud

Technology Transfer

Semiconductor Quality Control



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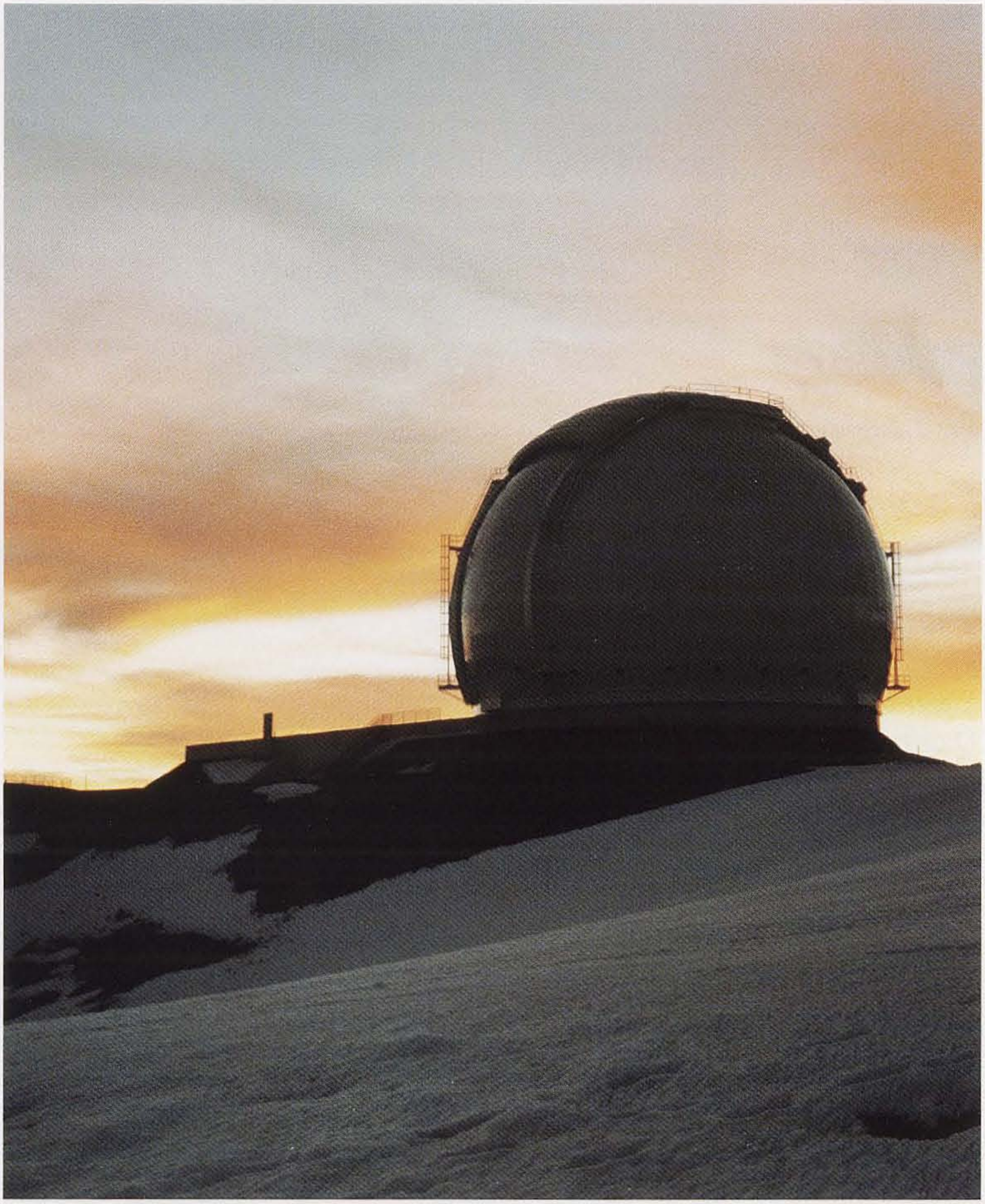

The 10-meter Keck Telescope saw first light in November 1990.

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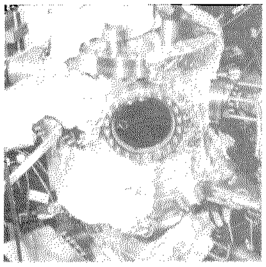

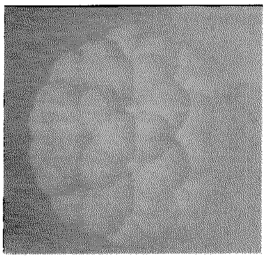

On the cover: A page from Robert Millikan's lab notebooks for his oil drop experiments measuring the charge of the electron shows that this one worked out to his expectations and would be a good one to publish. Is this fraud? No, says David Goodstein in an article beginning on page 10. Note that Millikan initially wrote at the bottom "Could discard this because of bad variations in v_1 & v_2" and then crossed that out and decided to "publish this because typical and good."

Departments

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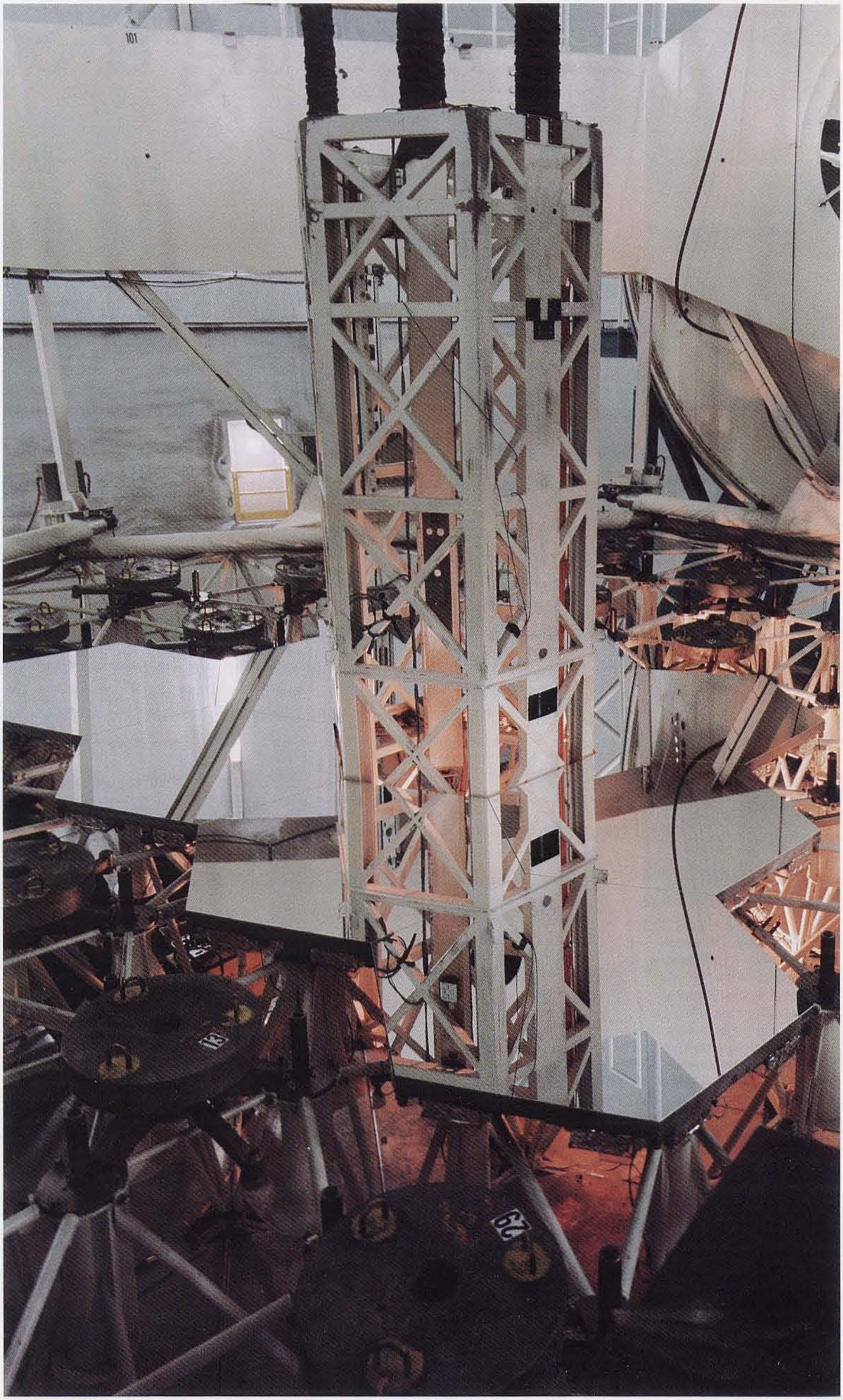
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First Lights

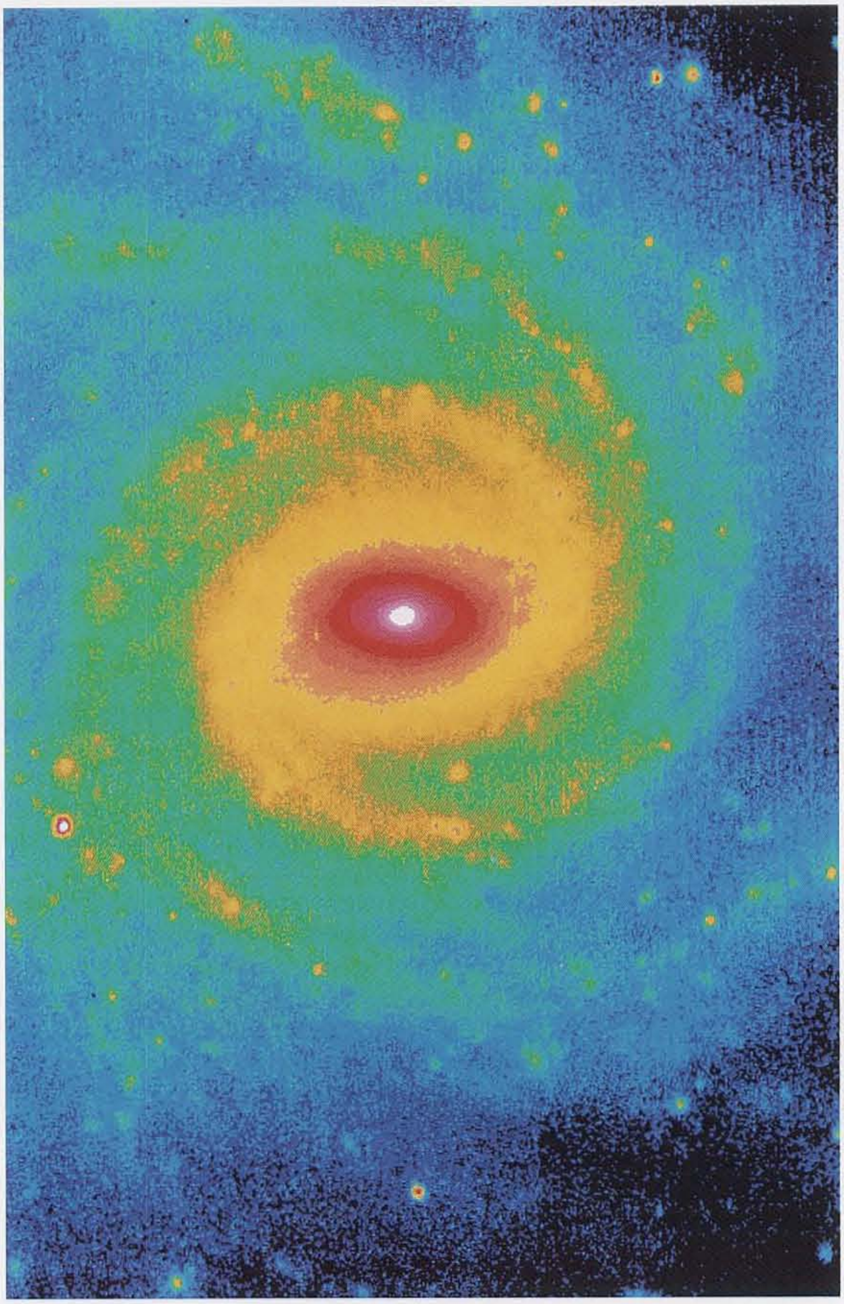
"Where was the gambler that would stake so much . . . on a single throw?"

With 9 hexagonal segments in place out of an eventual total of 36, the 10-meter mirror of the Keck Telescope demonstrated that its revolutionary concept would work.

Their hundred inch reflector, the clear
pool,
The polished flawless pool that it must be
To hold the perfect image of a star.
And, even now, some secret flaw—none
knew
Until to-morrow's test—might waste it all.
Where was the gambler that would stake
so much,—
Time, patience, treasure, on a single throw?
The cost of it,—they'd not find that again,
Either in gold or life-stuff! All their youth
Was fuel to the flame of this one work.
Once in a lifetime to the man of science,
Despite what fools believe his ice-cooled
blood,
There comes this drama.
If he fails, he fails
Utterly.

So English poet Alfred Noyes, in his epic poem *Watchers of the Sky*, versified George Ellery Hale's invitation to attend "first light" at the 100-inch Hooker Telescope on Mount Wilson in 1917. What poet could resist such an opportunity? First light—the moment when starlight first falls upon a telescope's mirror—was a more clearly defined event in those days, and it was a simple task to bring a poet along to capture the emotion of the moment. But, while the first reflection of starlight might appeal to poets, it's not necessarily very interesting to scientists today. With the complex technology of current instruments like the 10-meter Keck Telescope, "first light" is no longer simply a matter of opening up the dome and taking a peek. "First light really is the first time the telescope works as a system," says Edward Stone, Caltech professor of physics and chairman of the board of the California Association for Research in Astronomy (CARA). "It tells you that you have a concept that works." "It's the first time you can see astronomical objects well enough to know you can do research," says physicist Terry Mast (BS '64), one of two scientist members of the Keck project.

Even when scientists can define the scheduling of first light, it's still a big gamble on a single throw—especially when it involves a revolutionary design and costs $94.2 million. The Keck Telescope, when completed at the end of this year on Mauna Kea in Hawaii, will be the largest in the world. Its 10-meter (33-foot) segmented mirror consists of 36 hexagonal mirrors, each about 6 feet wide, 3 inches thick, and weighing 880 pounds, packed closely together in a honeycomb arrangement. Because the mirrors have slightly different curvatures dictated by their respective places in the total hyperboloid, an innovative procedure called stressed mirror polishing (which involves forcibly distorting the mirrors, polishing them, and then allowing them to relax into the desired aspherical shape) had to be developed. Cradled in position by devices that minimize mechanical stresses, the mirrors have their alignment controlled electronically to an accuracy of a millionth of an inch to act in concert as a single optical surface. Exquisitely delicate sensors and actuators on the back of the mirrors perform this alignment twice a second. Jerry Nelson (BS '65), professor of astronomy at UC Berkeley and the Keck's project scientist, had been developing the innovative design for

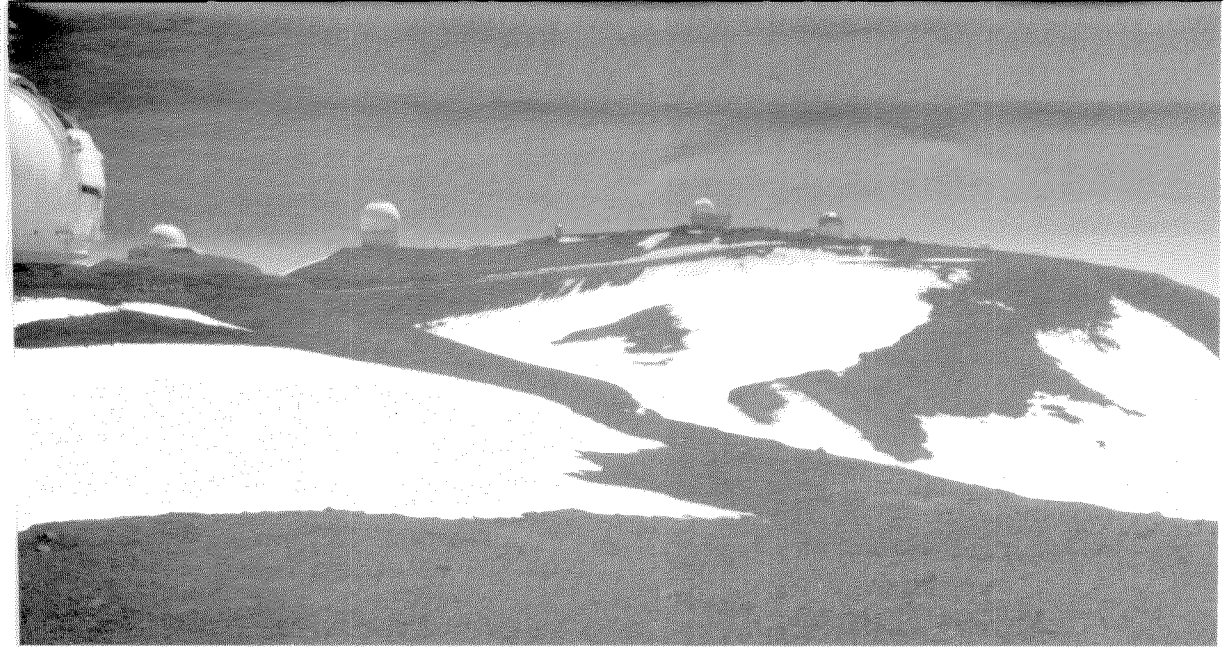


For its first celestial photograph, taken with a CCD engineering camera at the prime focus, the Keck Telescope was trained on NGC 1232, a spiral galaxy 65 million light years away. This is a color-enhanced mosaic of four successive exposures. Blue represents the faintest regions, while white indicates the galaxy's brightest area. Bright spots in the spiral arms are compact star-forming regions.

eight years before a $70 million gift to Caltech from the W. M. Keck Foundation made its construction possible. Much doubt and criticism has been expressed along the way. "The exciting thing about building this telescope," says Mast, who has been with the project since its beginning, "is that you're not sure the parts will all play together, even though you've tested them individually."

They found out on the night of November 24, 1990. With a telescope that "grows" mirror by mirror, it's difficult to say exactly at what point it should be considered ready for first light. How many mirrors were enough to demonstrate that the whole system worked? Whatever the decision, there would be a "moment of truth" when it would be clear whether it was successful. CARA decided to regard the first astronomical image with the 9-segment array of mirrors as first light. This was one quarter of the instrument's eventual size and equal in light-gathering power to Caltech's 200-inch Hale Telescope on Palomar Mountain, providing a valid comparison for determining the success of the new technology—the optics, the polishing procedure, and the control system.

On the night when all this technology came together the Keck scientists picked one of a list of photogenic (rather than scientific) objects to look at. NGC 1232 (also known as Arp 41), a spiral galaxy 65 million light years away, was located in a region of the sky within the telescope's range (the drive and control system for pointing the telescope was not yet fully operational), and it would do just fine for revealing

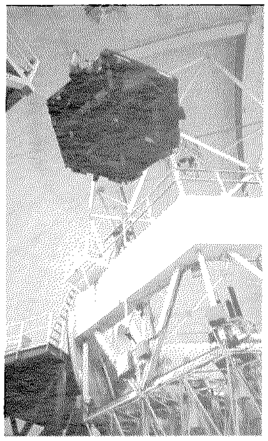


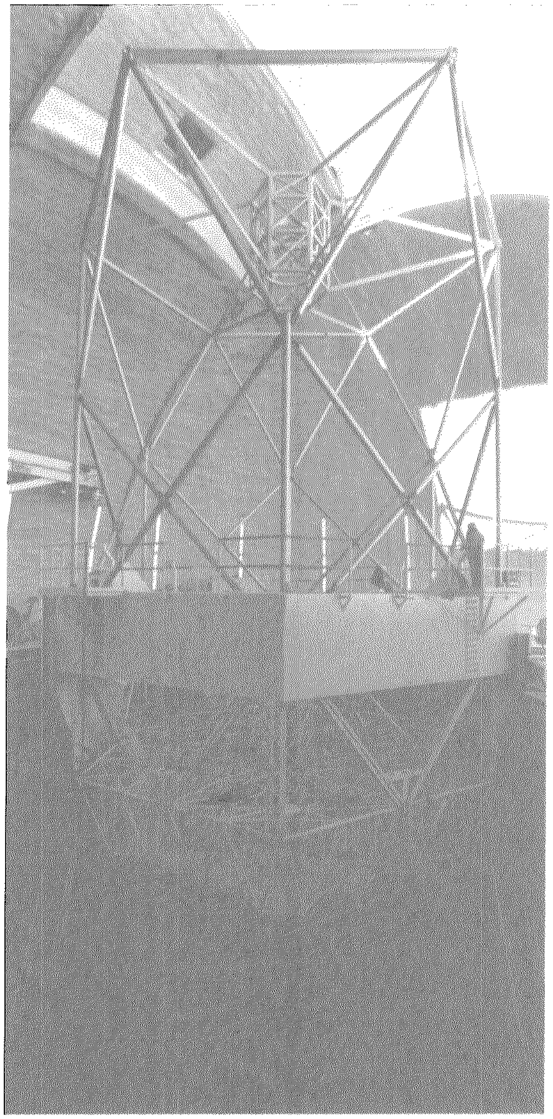
The W. M. Keck Observatory (left) sits with a group of international companions atop Mauna Kea, an extinct Hawaiian volcano, where the astronomical "seeing" is ideal.
Below: The telescope's structure, still lacking mirror segments (the mirror's skeleton is at bottom), is positioned inside the dome.
Below left: A mirror segment is hoisted into place.

the mirror's optical qualities. Nelson describes the evening as frustrating, as the group struggled with bad weather and failing computers. But when the galaxy's image from the engineering camera emerged on the monitor, "we were elated," says Nelson. "We were all hopping up and down." Already the picture is comparable to the best images from Palomar, according to Stone. And telescope performance can only get better when the telescope is tuned up and equipped with scientific cameras.

Had no one sneaked a peek before November 24? Mast admits that the real moment of truth had come when 5 mirrors were in place. "We knew with 5 segments that it was going to work." But tests with the 5 allowed the scientists to solve a number of problems which then enabled the 9-mirror configuration to demonstrate conclusively that the system worked. The high-quality image was proof. "We're convinced that if it works with 9 mirrors, it will work with all 36," said Project Manager Jerry Smith. It was a milestone, according to Nelson (even if not absolutely the first milestone)—and one obviously worth hopping up and down about.

For the Keck's predecessors in the role of world's largest telescope the milestones were not very precisely placed either. George Ellery Hale first aimed Mount Wilson's 100-inch Hooker Telescope at Jupiter in 1917. In 1903 Hale had decided on Mount Wilson as the site of his new solar observatory, and when the Carnegie Institution of Washington provided support for his 60-inch telescope, he built that on the mountain above Pasadena also. Convinced that bigger and





Right: George Ellery Hale uses his spectroheliograph in the Hale Solar Laboratory. Below: The 100-inch and 60-inch telescopes nestle side by side on Mount Wilson.

bigger telescopes would open undreamed-of vistas to astronomers, Hale persuaded local hardware magnate John D. Hooker to donate $45,000 for a 100-inch mirror in 1906. Carnegie again chipped in the rest. A decade of difficulties, not the least of them World War I, retarded construction. (Hale was also busy creating Caltech and luring its other two founders to Pasadena—Arthur Amos Noyes first came in 1913 and Robert Andrews Millikan in 1916.

All was ready on the night of November 2, 1917. W. P. Hoge, night assistant on the 60-inch, provided a prosaic description in the telescope's observing logbook (now part of the Mount Wilson Observatory collection in the Huntington Library) for that night: "The 100 inch telescope was pointed to the sky for the first time on this night—Visual observations were made of a star image, Jupiter, moon and Saturn . . ." Hoge records as present on the occasion (besides Hale) Walter S. Adams, the assistant director; Francis Pease, who designed the telescope; and an assortment of astronomers, designers, instrument makers, machinists, electricians, and carpenters. "Alfred Noyes celebrated English poet was the only visitor on this occasion."

Noyes was inspired by the event to compose his 281-page poem on the history of astronomy, *Watchers of the Sky*, which supplied the drama that the scientists had eschewed. In supplying perhaps a bit too much drama, he appears to have missed the big scene. The poem's prologue describes Noyes's "unforgettable experience" on Mount Wilson:

> Then, into the glimmering dome, with bated
>   breath,
> We entered, and, above us, in the gloom
> Saw that majestic weapon of the light
> Uptowering like the shaft of some huge gun
> Through one arched rift of sky.
>
> . . . .
>   The switchboard shone
> With elfin lamps of white and red, and
>   keys
> Whence, at a finger's touch, that monstrous
>   tube
> Moved like a creature dowered with life
>   and will,
> To peer from deep to deep.
>   Below it pulsed
> The clock-machine that slowly, throb by
>   throb,
> Timed to the pace of the revolving earth,
> Drove the titanic muzzle on and on,
> Fixed to the chosen star that else would
>   glide
> Out of its field of vision.

Noyes goes on to describe in elaborate detail seeing a moon of Jupiter. The sight was

> . . . clearer far
> Than mortal eyes had seen before from
>   earth,
> O, beautiful and clear beyond all dreams . . .

Perhaps tears of emotion blurred Noyes's vision at the proper moment, for Walter Adams, who became Mount Wilson's director on Hale's retirement from that position in 1923, told quite a different story (*Publications of the Astronomical*

Society of the Pacific, vol. 59, 1947): "Soon after dark the telescope was swung over to the eastward and set on the planet Jupiter, and we had our first look through the great instrument. The sight appalled us, for instead of a single image we had six or seven partially overlapping images irregularly spaced and filling much of the eyepiece. It appeared as if the surface of the mirror had been distorted into a number of facets, each of which was contributing its own image."

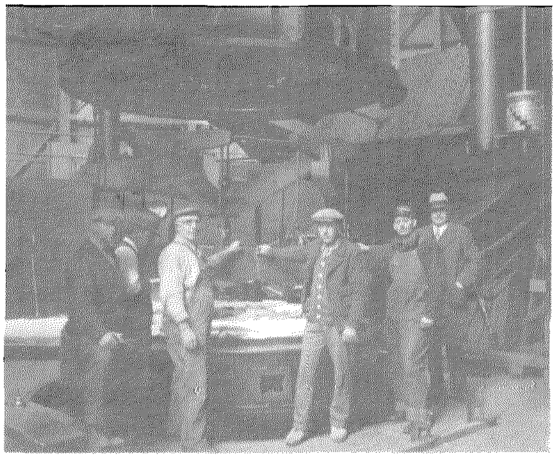
Guessing that the sun shining on the mirror or its cover through the open dome may have distorted its surface, Hale and Adams met again at 3:00 a.m. According to Adams, they trained the telescope on the star Vega. "With his first glimpse Hale's depression vanished: the mirror *had resumed its normal figure during the long* cool hours of the night, and the image of the star stood out in the eyepiece as a small sharp point of light, almost dazzling in its brilliancy." Whew! Helen Wright's 1966 biography of Hale, *Explorer of the Universe*, repeats this account, quoting Adams.

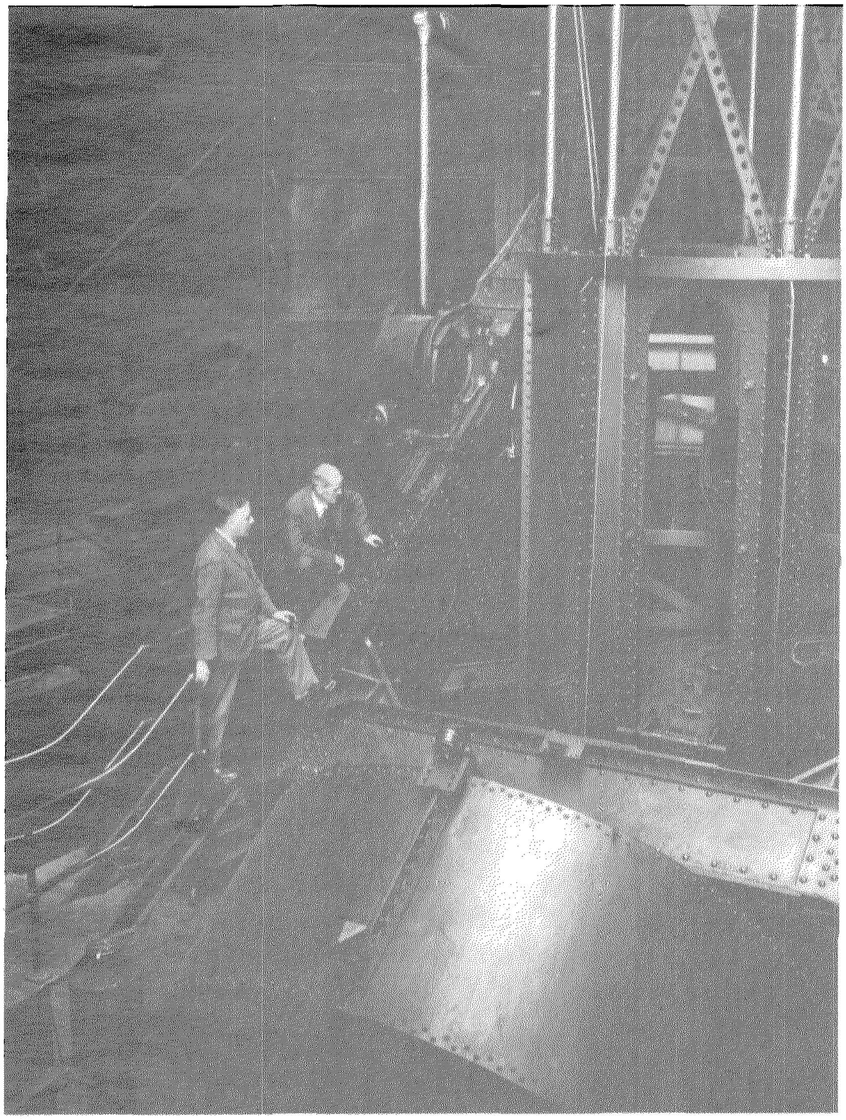
Adams's own memory may have been somewhat distorted by the 30 years between the happening and the telling. Bob Eklund, a volunteer with the Mount Wilson Institute, the observatory's current operator, remembers a recent discussion among a group of amateur astronomers about Vega's position on that night. Eklund, who knew that Vega was overhead in July, took his "handy-dandy little star finder" and figured out that Hale and Adams couldn't possibly have seen Vega, which at 3:00 a.m. on November 3 was below the horizon. Ron Brashear, assistant curator of science at the Huntington Library, confirmed this with a calculator program. Hale himself mentioned neither Vega nor the distortion problems. His diary (quoted in Wright's biography) *notes for the evening of Friday*, November 2: "With Alfred Noyes to Mountain. First observations with 100"—Jupiter, Moon, Saturn."

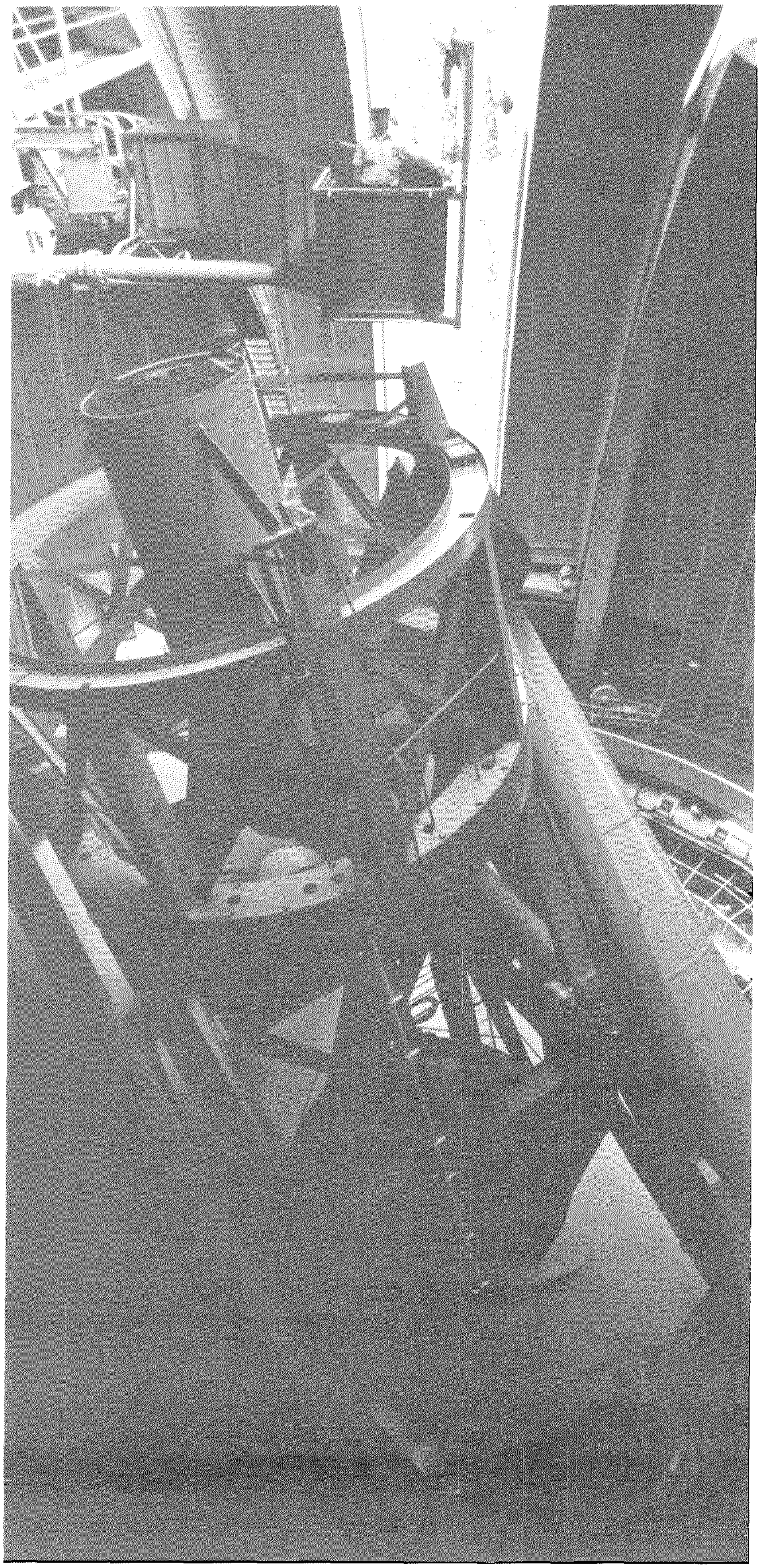
Hale was not long satisfied with the 100-inch telescope's limited reach into the heavens, and by 1923 (in an article in *Popular Astronomy*) was already advocating bigger mirrors. Although Pease, designer of the Hooker, was pushing for 300 inches, Hale settled for a more easily funded 200-inch mirror and in 1928 persuaded the Rockefeller Foundation to provide $6 million to build it under the joint administration of Caltech and the Carnegie Institution of Washington. Hale died in 1938 before its completion, which war had again delayed.

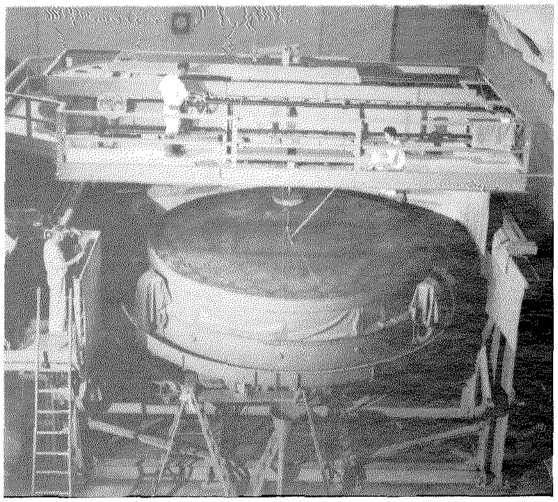
In December 1947 the first stars were seen reflected in the mirror of the newly christened

Right: The 100-inch mirror is removed for resilvering in 1931. Francis Pease, who designed this mirror as well as the 200-inch, stands at far right.
Below: English mathematician and physicist Sir James Jeans (left), who was a research associate at the Mount Wilson Observatory for 20 years, and Walter Adams, who succeeded Hale as observatory director in 1923, perch on the structure of the Hooker Telescope.





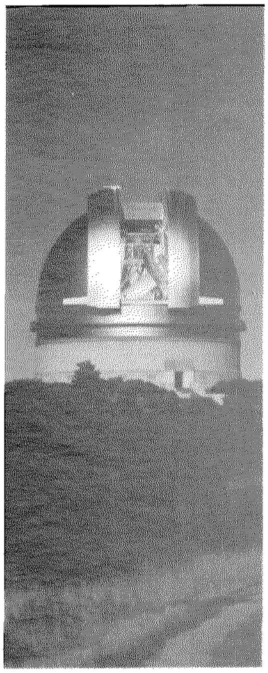


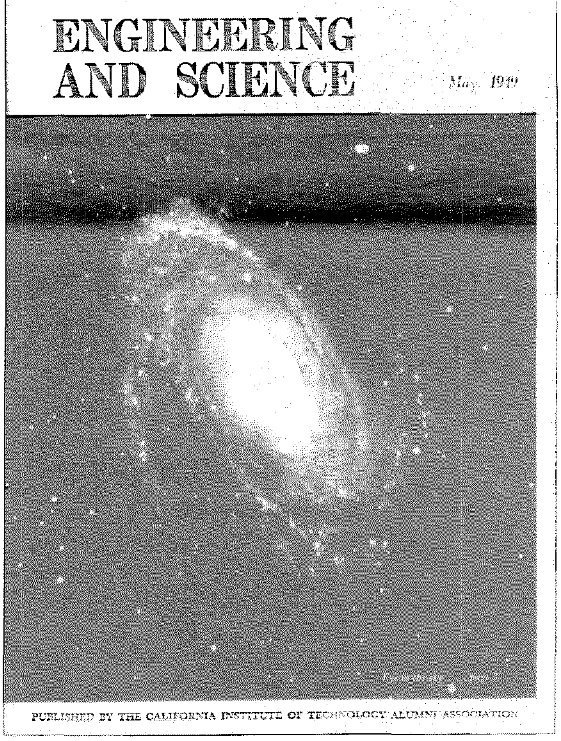


Hale Telescope on Palomar Mountain. John A. Anderson, executive officer of Caltech's Observatory Council and under whose direction the mirror was ground, polished, and figured, was the first to look. According to the June 1948 issue of *Engineering and Science Monthly*, "There was nothing spectacular about the 'first look.' Dr. Anderson used a small reading glass for an eye piece and peered into the big mirror. Asked what he saw, his noncommittal answer was, 'Oh, some stars.'" (This anecdote is also recounted in the forthcoming history of Caltech, *Millikan's School*, by Archivist Judith Goodstein.)

According to Brashear at the Huntington Library, the telescope's designers and builders had been concerned before the first look that the big mirror turned up a bit too much at its edges, but had decided to wait to see how it performed with the mirror support system. Unfortunately, the telescope mount sagged under the 14.5-ton mirror so that, even though Anderson could see some stars in it, the mirror couldn't be tested. A new support system was finally installed in October of 1948, and only then was it discovered that the mirror's figure (its light-focusing shape) was indeed slightly off. But because Ira Bowen, director of the Palomar and Mount Wilson Observatories, was under some pressure to show off the world's largest telescope, says Brashear, what could be called "second light" took place in January 1949. The May 1949 *E&S* reported: "Quietly and without fanfare, on the night of January 26th, 1949, at 10:06 p.m. P.S.T., Dr. Edwin Hubble pulled the slide of the plate holder starting the exposure on plate

Far left: The prime focus cage at the top of the Hale Telescope's tube is 55 feet above the mirror. Left: Before installation the 200-inch mirror is given its final polishing in Caltech's optical shop. The mirror occupied the optical shop from 1936 till 1947. Below: The Hale Telescope's shutters open on a moonlit night on Palomar Mountain in 1950. Right: One of the first pictures to be taken with the 200-inch graces a 1949 E&S cover. Another pretty spiral galaxy, this one is called Messier 81 and is 3 million light years away.





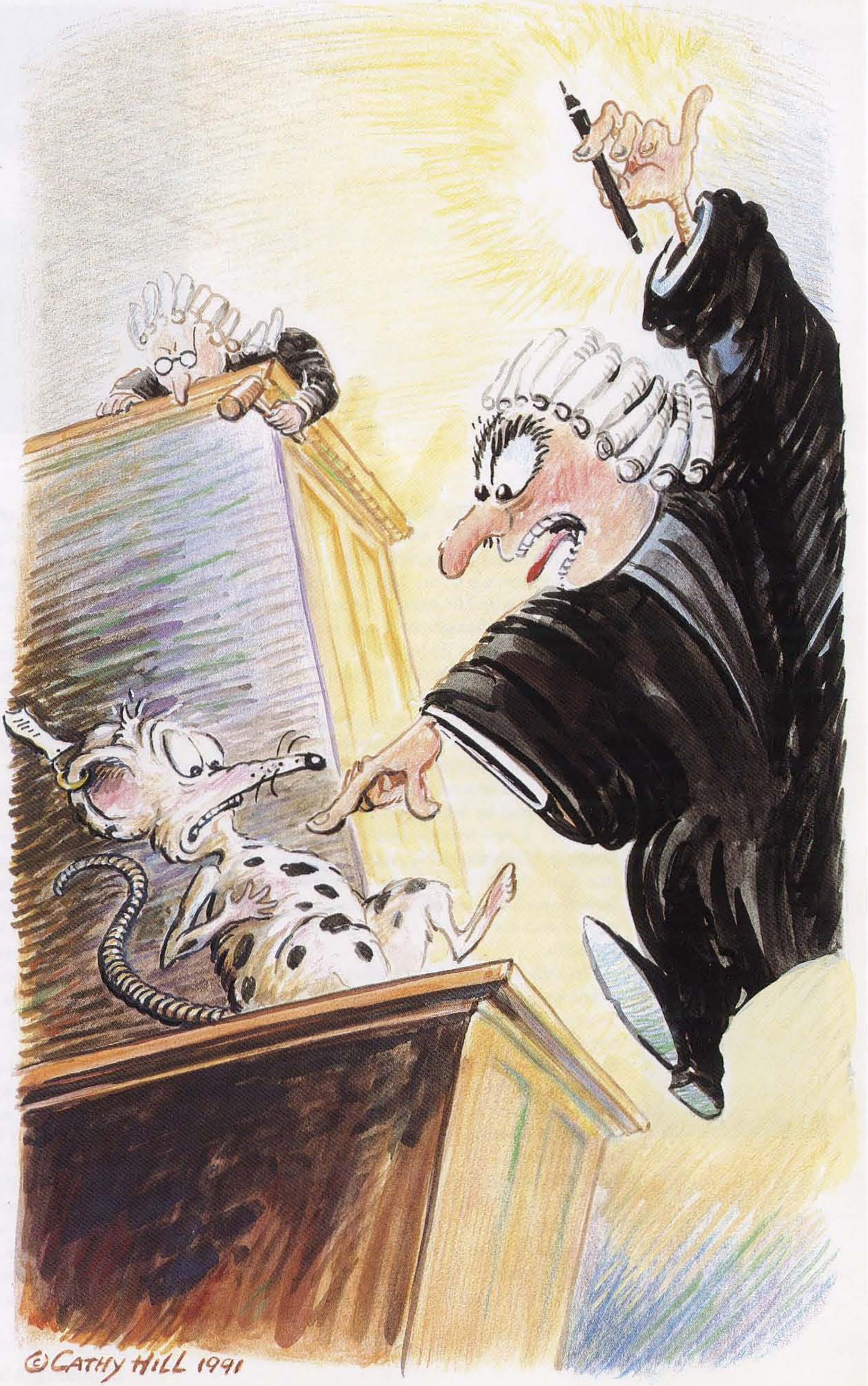
number P.H.-1. The P stands for Palomar, the H for Hale, and the 1 means that this was the first astronomical photograph taken on the first observing schedule of the 200-inch Hale telescope at Palomar Mountain." (Actually, Bowen had taken test photographs a year earlier, already logged in as P.H.-1 through P.H.-5, which Brashear unearthed recently at Carnegie's Santa Barbara Street headquarters.)

No wonder the ceremony took place "quietly and without fanfare." At about the same time that *E&S* was exhibiting the Hale's first pictures, the mirror was removed to be refigured. This was completed by September 1949, says Brashear. It was realuminized in October, and finally in November 1949, nearly two years after first light, the 200-inch began normal operation. Bowen was lucky his telescope was on the ground and not in orbit 381 miles above the earth.

Its difficult birth did not affect the Hale Telescope's ultimate success, and for decades the 5-meter instrument reigned as the largest effective optical telescope in the world. By extending man's view of galaxies as far as 8 billion light years—or, in terms of time, halfway back to the beginning of the universe—it has been the site of most of this century's discoveries in astronomy and cosmology. Of course, astronomers want to look still farther, but a bigger (and consequently heavier) mirror would be impossible to manufacture and support. Looking farther back in time would require a new telescope technology—and money. The contributions of Rockefeller, Carnegie, and Hoker seem almost paltry compared to the cost of a giant telescope today. (And the 60-inch mirror for Hale's early venture on Mount Wilson was donated by his father.)

Technology and funding came together in January 1985 with the announcement of the W. M. Keck Observatory. A mutually advantageous alliance was forged between the University of California, which had been working on the segmented design of a 10-meter telescope since 1977, and Caltech, which was the recipient of a $70 million gift from the W. M. Keck Foundation, the largest private gift ever made to a single scientific enterprise. The two institutions work together under CARA, directed by Ed Stone (now also the new director of the Jet Propulsion Laboratory). UC will supply the operating expenses and funds for the initial complement of scientific instruments, and observing time will be shared (also with the University of Hawaii, which provided the site). When the telescope begins operating in another year, astronomers expect to be able to see galaxies as they were 12 billion years ago—only 3 billion years or so short of the Big Bang.

But, like Hale, today's astronomers are already looking ahead to seeing still farther. The Keck Telescope's successful first light brings closer the possibility of building a twin 10-meter telescope on Mauna Kea. The two would act as an interferometer, in effect forming a mirror as large as the distance between them. With the staggering potential of seeing galaxies only 1 to 2 billion years after the Big Bang (and quasars even earlier), one can only hope that a good poet will be lined up for the occasion. □—*JD*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Scientific Fraud

One of the reasons that nobody knows the exact extent of scientific fraud is that nobody knows exactly what scientific fraud is.

by David Goodstein

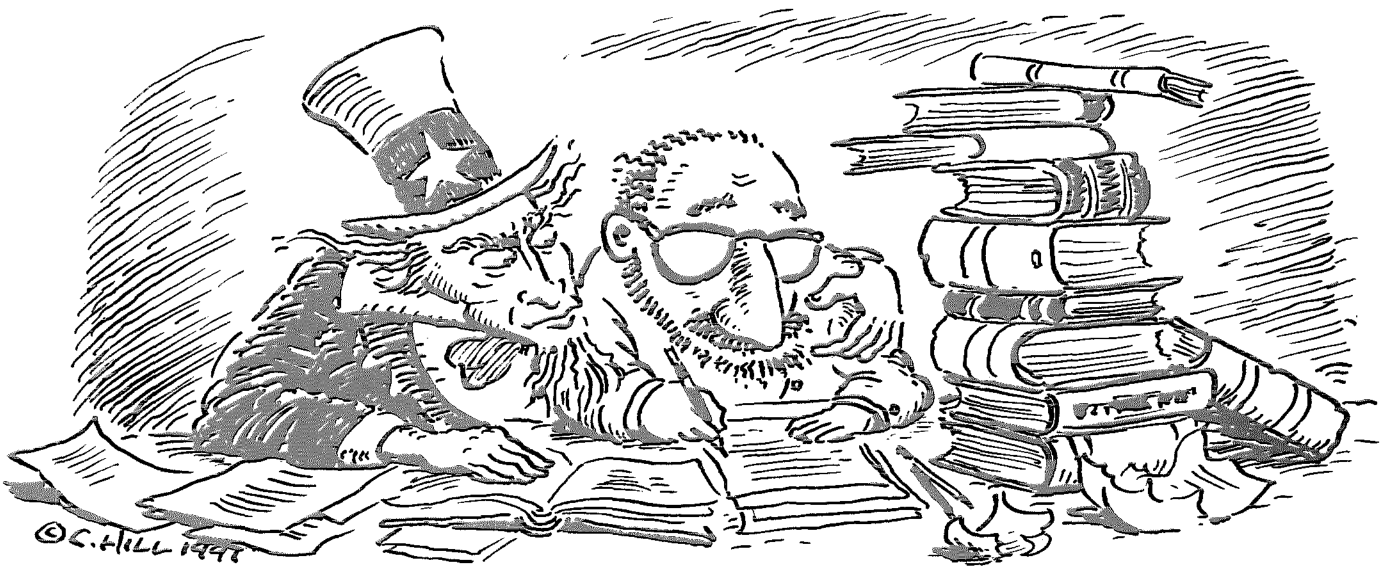
Most scientists have traditionally believed scientific fraud to be rare or nonexistent. Nevertheless, it has recently become a very hot topic. And there certainly have been some well-documented cases in the past. Perhaps the most famous incident of scientific fraud in this century was the case of Piltdown man—a human cranium and ape jaw that were found in a gravel pit in England in 1908 and 1912. Substantial academic reputations were made by discerning human characteristics in the jaw and ape characteristics in the cranium. However, this missing link was exposed as a fake in 1954. Another famous case was that of Sir Cyril Burt, a psychologist who worked on the heritability of intelligence by studying identical twins who were separated at birth and brought up in different environments. Unfortunately, there were very few cases of such convenient subjects for research, so Burt obligingly invented 33 more and further helped matters along by inventing two assistants to help him study them. Burt died in 1971, but his hoax was not discovered until 1974.

In 1974 William Summerlin was doing research at the Sloan-Kettering Institute in Minnesota that required nature to produce for him some rats with black patches on their skin. Since nature was not sufficiently cooperative, he helped her along with a black, felt-tip pen and was caught in the act. In another case, John Darsee, a brilliant young cardiologist at Harvard Medical School, was producing approximately a hundred papers a year. Until he was caught red-handed fabricating data in 1981, it didn't occur to anyone that with that rate of production maybe he didn't have time to do the actual experiments. In yet another case, Stephen Breuning made headlines in 1987, when it was revealed that he had fabricated data in his research at the University of Pittsburgh on the effects of psychoactive drugs in children.

The most recent notorious case involves a paper in which Nobel-prizewinning biologist David Baltimore, now president of Rockefeller University, was one of the authors. A postdoc in the group, Margot O'Toole, without accusing anybody of fraud, claimed that the evidence did not support the conclusions in the paper. The particular work under criticism was actually done by one of Baltimore's collaborators, Thereza Imanishi-Kari, but because of his name on the paper, the case attracted only slightly less journalistic attention than the Persian Gulf situation.

I started to become personally more involved with fraud about three years ago when, as the new vice provost, I had to dig through the avalanche of paper on my desk that reported on what was going on in Washington. As I read some of that material, it started to become obvious that Caltech was going to be forced to have a set of formal regulations on what to do in the unthinkable event of scientific fraud. So, in order to prepare myself, I started to collect information on fraud. I now have a file that fills a whole file drawer, and Caltech now has regulations on scientific fraud. The file tells, among other things, the political history of this issue.

The first serious congressional attention to the problem seems to have been in 1981, when the investigations subcommittee of the House Com-



This was something beyond their understanding and they should keep their grubby hands out of it.

mittee on Science and Technology was prompted to look into a Harvard Medical School case. Albert Gore, then representative and now Democratic senator from Tennessee, was chairman of the committee. Philip Handler, then president of the National Academy of Sciences, made a presentation to the committee in which he told them pretty much what most scientists today would say to most congressmen—that this was something beyond their understanding and they should keep their grubby hands out of it. This was not exactly well received by Congress, which felt that the scientists, after all, were being supported by the public and ought to accept congressional oversight. Nevertheless, these hearings did not lead to any congressional action. During the early eighties, Orrin Hatch, Republican senator from Utah, started poking into the National Cancer Institute, also without permanent effect. But more recently, and with greater publicity, two Democratic representatives, John Dingell of Michigan and Ted Weiss of New York, tried to get into the act of investigating the Baltimore case (and conceivably benefiting from it politically) by holding hearings in their respective subcommittees.

Dingell had succeeded Gore as chairman of the investigations subcommittee of the Science and Technology Committee, and Weiss was head of the subcommittee on human resources and intergovernmental relations of the Government Operations Committee. In April 1988 these two competed in a somewhat unseemly race to be the first one to hold hearings. Dingell's hearings were to lead to a much-discussed report that has not yet appeared at this writing. Just last October Weiss's committee issued a booklet containing an analysis of 10 cases of scientific fraud, entitled "Are Scientific Misconduct and Conflict of Interest Hazardous to Our Health?" The title says a lot about the slant of the booklet, which is especially critical of the universities for their handling of these cases. The committee report was not well received in the press, which pointed out that it was based largely on an analysis of cases that had occurred in the early 1980s. Much has happened since then, and the universities have improved in their handling of fraud cases, so that the report is by now largely irrelevant. It seems to have dropped out of sight.

Meanwhile, at the National Institutes of Health, a couple of biologists named Ned Feder and Walter Stewart have set themselves up as a kind of self-appointed truth squad. According to their critics they had not been very productive biologists and were trying to find a way of holding on to their laboratory and office space. They hit upon the fraud issue and were particularly visible in the Baltimore case. In many other cases too, they have become the lightning rod for whistle blowers. Anyone can call to report an instance of scientific fraud. These two now have official permission from their superiors to spend a certain percentage of their time pursuing wrongdoers.

In 1988 and 1989 the National Institutes of Health (NIH) and the National Science Foundation (NSF) each published in the Federal Register formal sets of regulations regarding scientific

In tort law, proving fraud is quite a different matter from what we regard as sufficient indication of fraud in science.

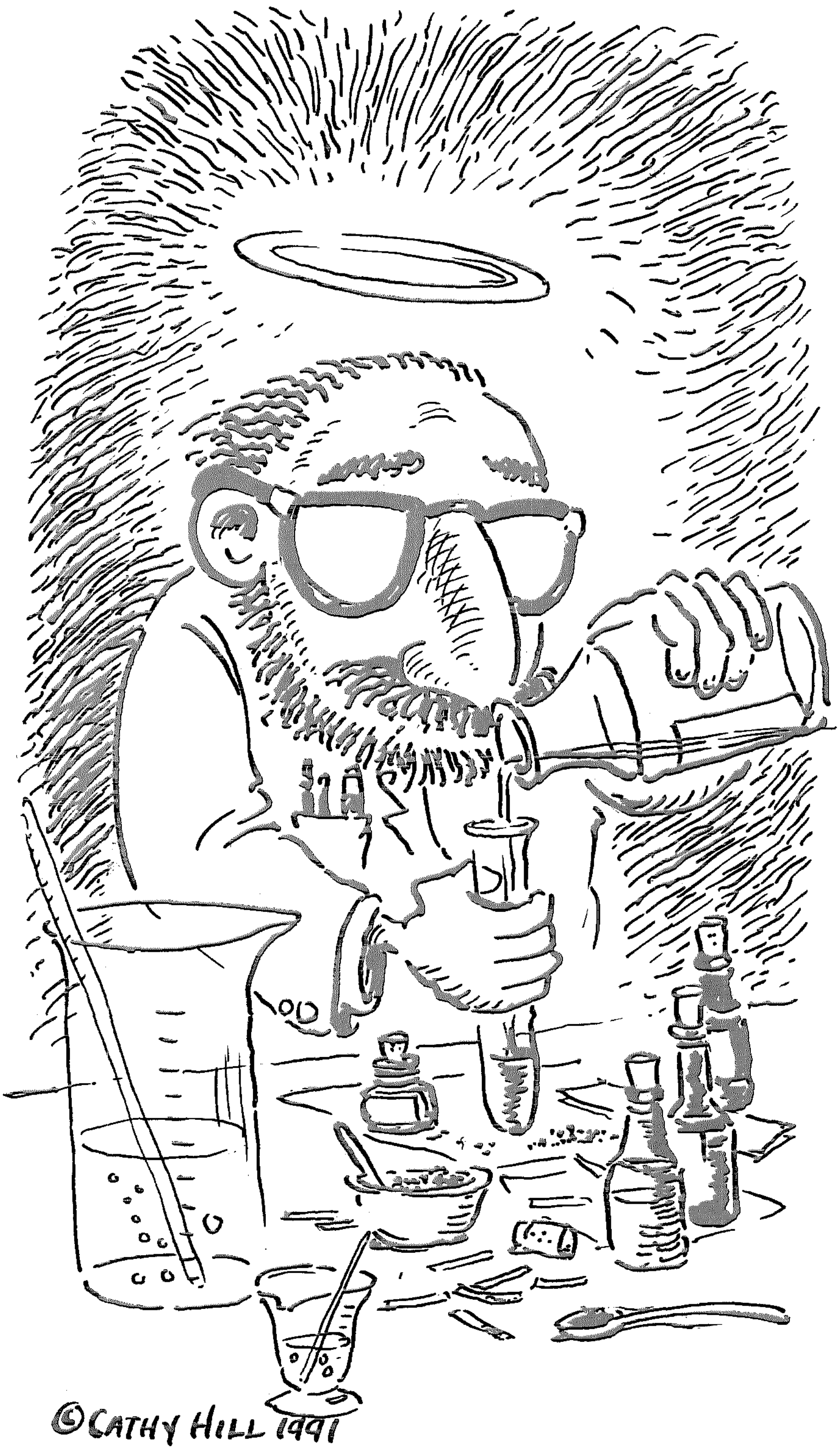
fraud. These two sets of regulations, many pages long, are virtually identical. Both of them call on the university (if the fraud has been committed at a university) to investigate the situation first and only later to hand it over to the agency. A rule was declared in late 1989 by the Public Health Service, the parent organization of the NIH, stating that after January 1990 no research proposal would be accepted from any university that did not certify that it had in place a formal set of regulations on how to handle research fraud. That was the point at which it became necessary for Caltech to have such regulations. An Office of Scientific Integrity has been established within the NIH. The very name calls up images of "1984." (1984 is now a date in the past, but it was once a date in the future.) The NSF doesn't yet have such an office, but it has an Inspector General who seems to serve much the same function. These entities are concerned with fraud, misconduct, and conflict of interest —three types of misbehavior that may not always be so easily distinguishable.

One question any thoughtful person must ask is: How common is scientific fraud? How often does it happen? Is it something that's so rare we shouldn't worry about it? Or is it really quite common and a major threat to the scientific enterprise? One of the reasons that nobody knows the exact extent of scientific fraud is that nobody knows exactly what scientific fraud is. What do we mean by the phrase? For an answer, we turn first to the most authoritative possible source—the Caltech regulations. They define science fraud or research fraud as "serious misconduct with intent to deceive, for example, faking data, plagiarism, or misappropriation of ideas." That's a clear definition. Barbara Mishkin, a Washington lawyer often quoted in this context, has listed three types of scientific misconduct: 1) knowing misrepresentation of data, procedures, or analysis; 2) plagiarism and other authorship misdeeds, such as guest authorship and the like (guest authorship means putting the boss's name on the paper even though he didn't really do any research); and 3) outright violation of laws, such as laws regarding human subjects, recombinant DNA, and so on. The Caltech regulations address the first two of these but explicitly rule out the third as not coming under their jurisdiction. If you violate a law—for example on the handling of human subjects—there are means and procedures already in place for dealing with that.

Personally, I don't think these definitions cover the whole map. In my 25 years as a working scientist, by far the most serious instances of misconduct that I have seen at first hand in my own field have come in the arena of anonymous reviews of journal articles and research proposals. This type of thing is never mentioned at all by anyone who deals with the subject of scientific misconduct—the lawyers, the philosophers, or the sociologists. But they're not the scientists in the trenches. Seen from my own narrow trench in physics, that's where you find the misconduct.

In tort law, proving fraud is quite a different matter from what we regard as sufficient indication of fraud in science. First of all, the law envisions a plaintiff and a defendant; someone has to bring the case to court. In order to prevail, the plaintiff must prove five points: 1) that a false representation was made—in other words, that the defendant cheated; 2) that the defendant knew it was false (or recklessly disregarded whether it was); 3) that there was intent to induce belief in this misrepresentation; 4) that there was reasonable belief on the part of the plaintiff; and 5) that there was resulting damage.

In science fraud nobody pays attention to the fourth and fifth points—that there was reasonable belief and actual damage. Nobody pretends that we have to prove that in order to, in effect, convict someone of research fraud. The Caltech regulations, which spell out "serious misconduct with intent to deceive" and so on, seem to encompass the first three: false representation, knowledge that it's false, and intent to induce belief. But I think that a clever lawyer taking on a real case of fraud, such as some of the recent examples I've cited, could argue that there



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The Noble Scientist is somehow supposed to be more virtuous and upright than ordinary people and therefore can be expected not to misbehave even in the smallest way.

was not false representation in the ordinary sense. In most cases of science fraud, the person committing the fraud was not trying to perpetuate an untruth, but rather was trying to help along what he believed to be the truth. I'll get back to this point later on, when I discuss some specific historical cases.

In any case, the barrier against proving fraud is much higher in legal precedent than it is in the standards we apply to scientists. Now, we scientists of course tend to be arrogant; we think we know what is right. But the law has a great deal of experience with the behavior of real people, and since science is a very human activity, perhaps we have something to learn from lawyers about the standards for proving such serious allegations as fraud. The five legal points necessary to prove fraud are based on long experience with the way people really behave, whereas the idea of what constitutes science fraud is based on what I call the Myth of the Noble Scientist. The Noble Scientist is somehow supposed to be more virtuous and upright than ordinary people and therefore can be expected not to misbehave even in the smallest way. This myth only makes us more vulnerable to misunderstanding what we do and what actually constitutes fraud. The effects of this can be seen in an analysis of journalistic accounts of fraud in science.

Betrayers of the Truth, published in 1982 by Simon and Schuster, was written by William Broad and Nicholas Wade. Both were reporters for *Science* magazine, and Wade is now on the editorial board of *The New York Times*—hardly schlock journalists. Rather than try to analyze

Newton's theory was so good he was able to calculate the speed of sound and then compare it with measurements. When he did, they disagreed by about 10 percent.

my own file drawer full of newspaper clippings, it makes sense to take this book as an example of a serious study of science fraud by the best journalists, since they understand science better than others of their profession and are probably more dependable in what they write.

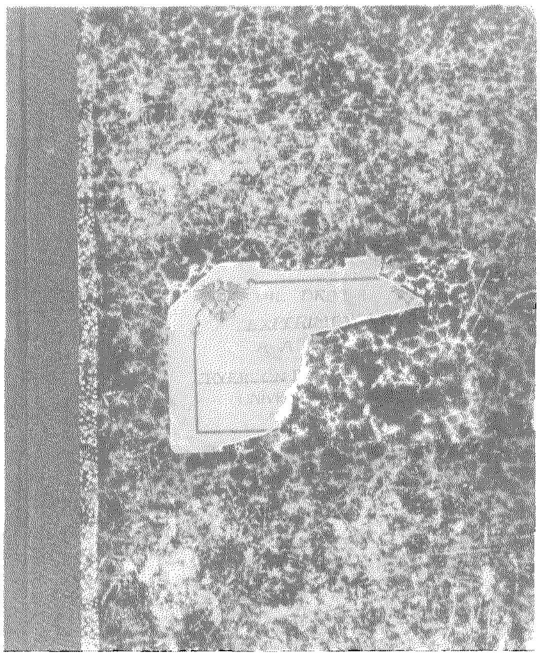
The book has an appendix entitled "Known or Suspected Cases of Scientific Fraud," which includes the case of Claudius Ptolemy, the Alexandrian astronomer of the second century A.D. who wrote the *Almagest*, upon which all of astronomy was based until the time of Copernicus. Broad and Wade claim that Ptolemy committed fraud because he could not possibly have made the astronomical observations he claimed he made. By techniques of archaeoastronomy—using knowledge of how the sky works to run it backwards to see what the sky looked like at a particular time in the past—researchers have found that the observations Ptolemy reports were not made in Alexandria in the second century A.D., but rather, at the latitude of Rhodes in the second century B.C. So they concluded that the actual readings were taken by Hipparchus of Rhodes.

Another person on the list of "Known or Suspected Cases of Scientific Fraud" is Hipparchus of Rhodes, whose observations, Broad and Wade say, were actually made by the ancient Babylonians. The authors make no comment about this impossible contradiction. Both accusations cannot be correct. But obviously they hold themselves to a less stringent standard than they apply to scientists.

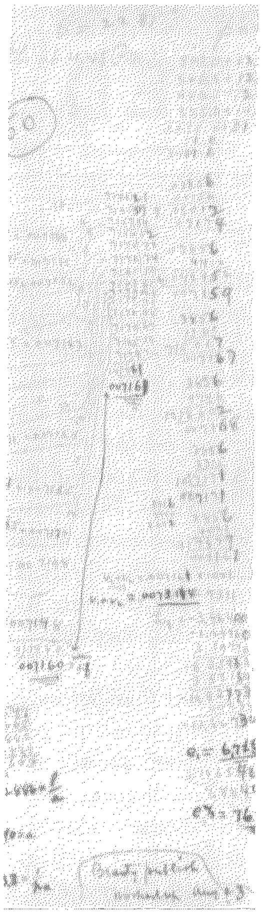
Among the other scientists they accuse of being "Known or Suspected Cases of Fraud" are Galileo, Newton, Dalton, Mendel, Millikan, and quite a number of others.

Among the other scientists they accuse of being "Known or Suspected Cases of Fraud" are Galileo, Newton, Dalton, Mendel, Millikan, and quite a number of others. I'm not personally familiar with the case of Mendel, who studied the genetics of peas and came up with data that some people have said are too good to be true, but I am familiar at first hand with some of the others—for example, Isaac Newton. Newton explained the propagation of sound waves in air. Newton's theory was so good he was able to calculate the speed of sound and then compare it with measurements. When he did, they disagreed by about 10 percent.

Now, you have to understand that before this, there was no idea at all why sound propagates in air, and to have calculated the speed within 10 percent was a huge intellectual triumph. Nevertheless, the 10-percent discrepancy bothered Newton, and so he set out to explain the difference. The real explanation for the difference has turned out to be that sound is adiabatic, and Newton had assumed implicitly that it was, instead, isothermal. In other words, in a sound wave there's heating and cooling that pushes the sound along a little faster than it would otherwise go. Newton didn't take account of that effect, so he calculated the speed that sound would have if it were all at one temperature. That subtle difference would not be understood for another 200 years, so you certainly can't blame Newton for not knowing it. But because he was disturbed that his theory didn't quite correspond to the observation, he tried to cook up some explanation for the discrepancy. He came up with all kinds of things



Millikan's lab notebooks for his Nobel prizewinning oil drop experiment contain several examples of his comments about how publishable the data are. The page portion at left also notes that he decided it was a "beauty" on August 23; the initial observations were made on March 30, 1912.



that sound hilarious to us now: the water vapor in the air didn't participate, he had ignored the space taken up by the molecules of air, and other things like that. He made little fixes until he finally got the theory in agreement with the experiment. It's the sort of thing that every theorist does today; if you have a theory that doesn't quite agree with the experiment, you speculate on what might cause the small discrepancy. That's exactly what Newton was doing. This is an example of what these two journalists regard as fraud. In hindsight Newton's fixes are funny; it's the way people really act. But fraud? No, it's not fraud.

Another example Broad and Wade give is our own sainted Robert Andrews Millikan. The accusation is based on notations in his laboratory notebooks, which we have in our Archives at Caltech, and which I have read and shown to my students. In fact, I show these notations every year precisely because they are instructive; they tell you something about the real world. Millikan was measuring the electric charges of oil drops; he wanted to prove that the electron charge came in definite units—that it was quantized—and then he wanted to measure what that unit was. He had actually already made his preliminary measurements, and he knew very nearly what the answer was. Millikan had a rival, Felix Ehrenhaft, who believed that electric charge was a continuous quantity rather than quantized. Ehrenhaft criticized Millikan's results, so Millikan went back to the laboratory to get better data to have ammunition against Ehrenhaft. Later on he published a paper in *Physical Review* in which he says (roughly): "I've published every piece of scientific data I got without bias; I have looked at 60 drops and here are all 60 drops," or something like that.

But when you look through his notebooks, it appears a bit different. Each page has notations on one drop. Millikan would spend a whole evening watching one drop go up and down in his electric field, measuring its speed, taking down data, making calculations, getting the result for the charge. He knew, of course, what result he expected. So in some cases he would write in red (everything else is black), "Beauty—Publish," or "One of the best I've ever had—Publish." And then on one page he wrote, "Very low—something wrong." And you know that that one did not get published, in spite of the fact that he said he published everything.

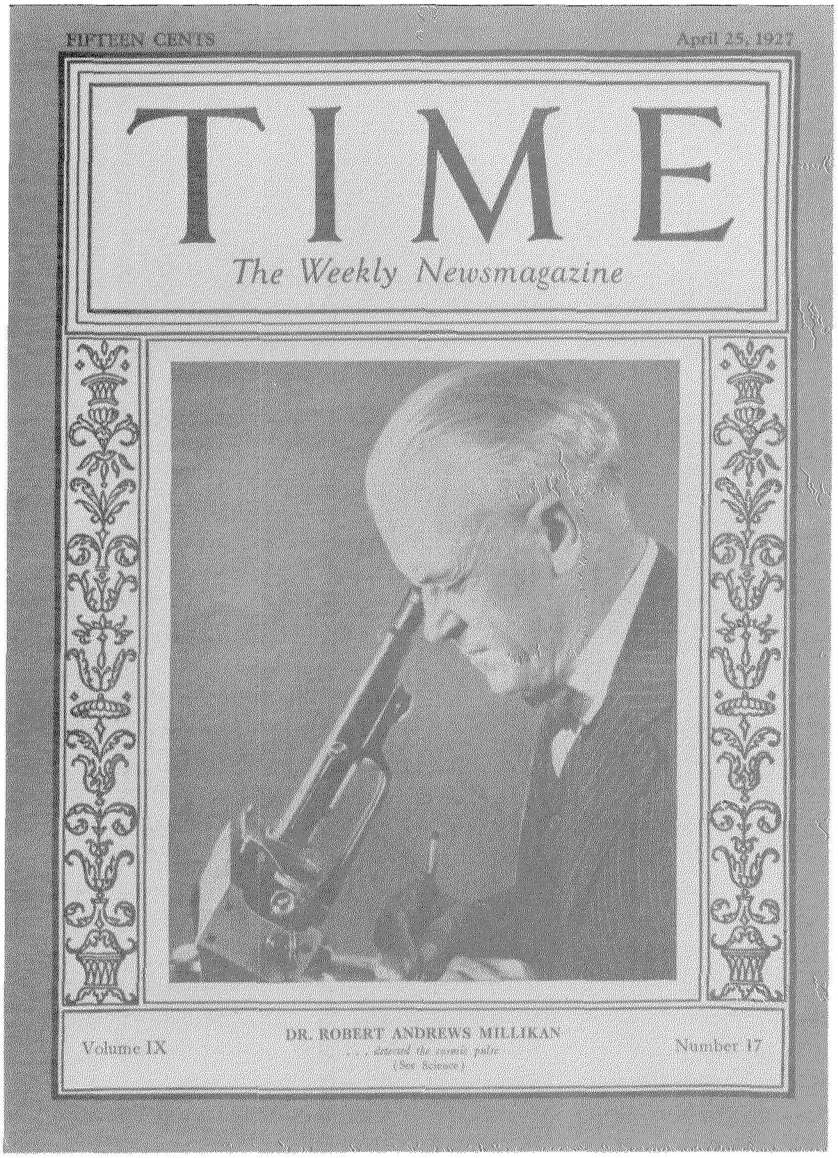
What's happening is that he has some idea of what he expects, and when he gets the wrong result it's a clue that something is wrong. But he doesn't just throw it out because he doesn't like it; he examines his experiment to figure out what mistake he's made, and when he finds the mistake, it is duly noted on the page ("distance wrong" he wrote on that particular page). People make mistakes; experiments are always pushing the limits of the possible. Scientists are always at the state of the art, and we make mistakes in the laboratory all the time. If everybody were obliged to publish every mistake, the scientific literature would be so full of garbage that you wouldn't be able to read it. It's bad enough as it is. What Millikan was doing was perfectly legitimate: he would examine the

If everybody were obliged to publish every mistake, the scientific literature would be so full of garbage that you wouldn't be able to read it. It's bad enough as it is.

"wrong" result and would find that he had made a mistake and conclude that that result had to be tossed out. Of course, he did not try quite so hard to find some reason for throwing away the results that were "right." That's really the point where bias enters his result. This kind of bias is built into all scientific research. Even though we take elaborate precautions—such as double-blind tests—to try to avoid this kind of unconscious bias, it still creeps into scientific results. But to call it fraud, as Broad and Wade do, is absolutely irresponsible. Millikan is merely another casualty here of the Myth of the Noble Scientist, which ignores the dynamics of the way real scientists work.

Millikan and Newton weren't guilty of fraud, but clearly some others are. Who are they? Patricia Woolf, a Princeton University sociologist, did a study of 26 cases of serious scientific misconduct that surfaced in one way or another between 1980 and 1987. It turned out that, of these 26 cases, two were in chemistry and biochemistry, one was in physiology, two were in psychology, and 21 were in biomedical sciences. Furthermore, of the 26, some 17 were committed by MDs rather than PhDs. So the conclusion is inescapable that scientific fraud is essentially biomedical fraud, at least in recent times. The $64,000 question is: Why is that true?

One reason some have suggested is that there is more money in biomedical sciences, and money corrupts. Fraud in the form of plagiarism, however, is not unusual in fields such as history, where there is very little money to be found. So it seems to me that money is not the



Caltech's own Noble (and Nobel) Scientist makes TIME's cover in 1927.

principal motivating force. I believe that career pressure is more important. In every case of science fraud I've looked at, somebody was advancing a career rather than seeking money. Other people have suggested that since the large majority of fraud perpetrators are MDs rather than PhDs, perhaps it's because medical doctors have a different sort of ethic from scientists—doctors care about the health of the patient rather than pure scientific truth. Being brought up in this ethic might give you a different attitude toward what's permissible and what's not. This is a subtle argument, and I don't know whether there's anything in it or not.

> *And everybody knew that God is an Englishman. If they had been discovered in those other places, there* had *to be prehistoric human remains in England.*

I used to have a theory that had to do with the reproducibility of results. In physics, and in other fields where there is little fraud, people believe that experiments are precisely reproducible, in the sense that if somebody else goes into the laboratory and does the same experiment, they'll get the same results. Now, every experimentalist knows that this is not true. Real experiments are too hard for that to be the case, but the whole field is pervaded by the idea that things are causally related in a relatively straightforward way, and therefore reproducible. So it would be foolish for me to fake a data point, because somebody else will repeat the experiment and find the data point in a different place.

Going back to the rivalry between Millikan and Ehrenhaft, this is what kept Millikan from being too cavalier and just keeping good results. He knew that if he got it wrong, his rival would bite his head off without any hesitation at all. So perhaps physicists are less likely to fake than scientists doing experiments in biology or biomedicine, where "truth" is more statistical, rather than causal or precise. I might feel in those fields that if I cheat a little bit, nobody's ever going to find out because my cheating will be within the range of uncertainty of the data.

This is what I thought before I started looking at some of these cases of fraud. What I found instead was that in every single case the person who perpetrated the fraud thought he knew the answer. That's quite different from feeling that you're in an imprecise field where things are not very reproducible. These scientists really thought that they knew what the answer was, and that by faking the data all they were doing was helping things along a little bit. They weren't perpetrating a false result; they were just taking a bit of a shortcut—leaving out some steps that weren't really necessary because they knew what the answer was. You can see that in the case of Cyril Burt, the psychologist who faked data on identical twins. He *knew* that intelligence was inherited, and to go out and find 33 more sets of identical twins that had been separated at birth would be impossibly difficult. And it was really unnecessary because he knew what the answer would be if he went through all that work. So why go through all that work, right?

You can see it even in the case of Piltdown man. By 1912 prehistoric human remains had been discovered in France and Germany, and there was some indication that there might *even* be some in Africa. And everybody knew that God is an Englishman. If they had been discovered in those other places, there *had* to be prehistoric human remains in England. It was only a matter of helping things along a little bit.

There are many mysteries about this whole business, and perhaps all of these factors play a role. It seems clear that scientists are most vulnerable to cheating or cutting corners when: 1) they are under career pressure to produce something; 2) they think they know what the answer is and feel that actually going to the trouble of taking the data just slows down the inevitable process; and 3) they think they are somewhat protected by "soft reproducibility."

There is no human activity that can stand up to the glare of relentless, absolute honesty. We all build little hypocrisies into what we do in order to make life a little bit easier to live. Because science is a very human activity, there are hypocrisies and misrepresentations built into the way we do it. For example, every scientific paper is written as if that particular investigation were a triumphant procession from one truth to

another. All of us who actually work in the trenches, however, know that every scientific experiment is chaotic—like war. You never know what's going on; you can't understand what the data mean. But in the end you figure out what it was all about and then, with hindsight, you write it up describing it as one clear and certain step after the other. This is a kind of hypocrisy, but it's deeply embedded in the way we do science. We're so accustomed to it that we don't even regard it as a misrepresentation anymore.

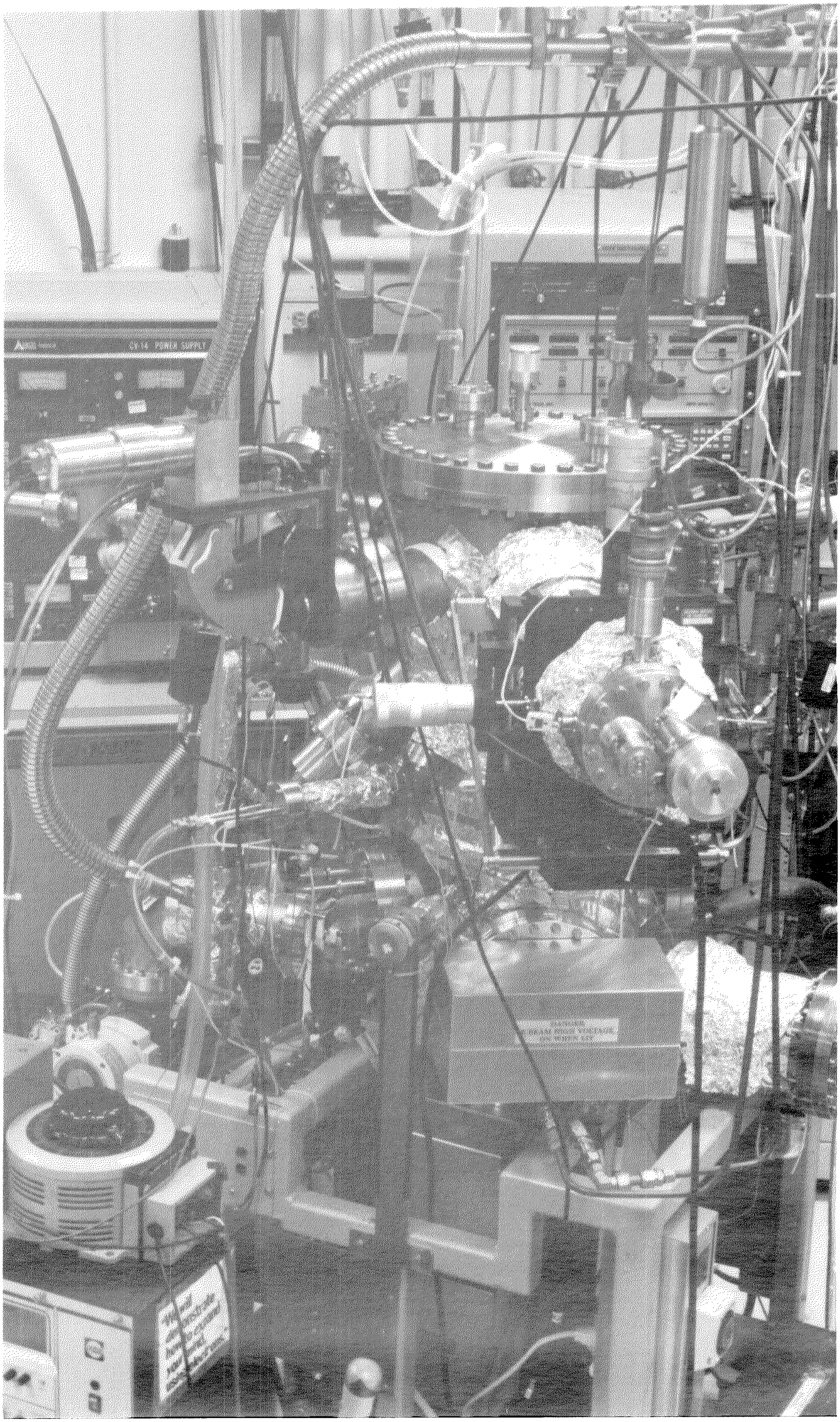
'Owing to difficulties in sample handling' really means something like 'we dropped it on the floor.'

The wry phrase "typical best case," for example, describes the routine procedure of saying that the data are typical, but presenting the best set of data that were produced. Everybody does this, and everybody recognizes that it's what everybody does. It's regarded as acceptable behavior; it's not considered fraud. There's an important distinction here: if I present my best case as typical, that's acceptable. But if I take those data and move one data point to make it look a little bit prettier, that's fraud. Scientists do recognize the difference. There's something sacrosanct about data; there's a hard line there that cannot be crossed.

Glossaries explaining the real meanings of terms found in scientific papers occasionally make the rounds of the trenches. For example, "owing to difficulties in sample handling" really means something like "we dropped it on the floor." This only recognizes that scientific papers may disguise what really happened, even though they are supposed to present things in a rigorously honest way. We don't hold classes in the rules of misrepresentation in scientific papers, but the apprenticeship that one goes through to become a scientist does involve learning them. That same apprenticeship, however, also inculcates a deep respect for the inviolability of scientific data. It teaches how one distinguishes the indelible line that separates harmless fudging from real fraud.

I believe that scientists are basically honest, even if they don't quite live up to the Myth of the Noble Scientist. Cases such as those of Summerlin and Darsee shocked every scientist I know. Although I've said here that we might have a thing or two to learn from the lawyers, I don't mean that we should go the whole legal route and insist on proving those five elements to demonstrate fraud. If someone has cheated on scientific data, we should regard that as fraud without having to prove anything else. Nevertheless, I think that the Myth of the Noble Scientist does not serve us well. Scientists are fallible human beings. So are congressmen and journalists. We could all benefit from just a little more understanding and honesty about what we really do, and how and why we do it. □

David Goodstein discussed scientific fraud at the November 28, 1990 session of a regular series of informal seminars on Science, Ethics, and Public Policy. As vice provost since 1987, Goodstein was chiefly responsible for drafting Caltech's regulations on scientific fraud, a document he is proud of. And if the above article, which is based on his seminar remarks, seems to tilt just a bit toward the physicist's point of view, it's because Goodstein is also professor of physics and applied physics (and creator of the prizewinning TV physics course, The Mechanical Universe*). Goodstein has been a member of the Caltech faculty since 1966. He earned his BS from Brooklyn College and PhD from the University of Washington.*


CV-14 POWER SUPPLY
DANGER

Deposit Insurance

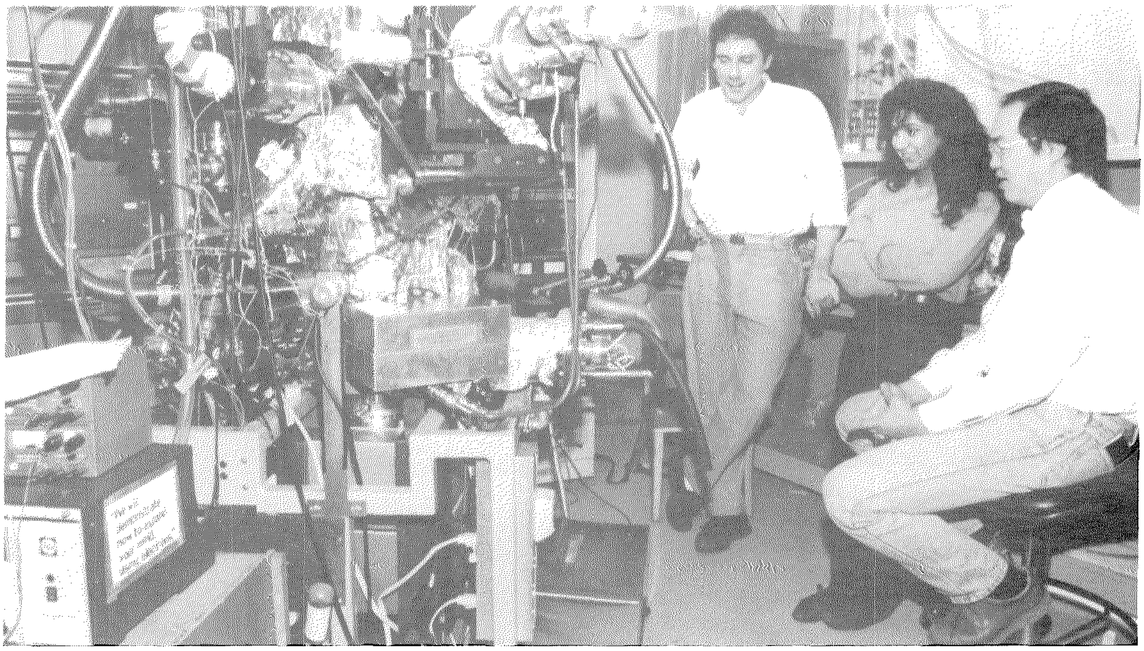
Chips are made inside sealed vacuum chambers, and controlling what goes on within is still to some degree a black art.

Looking like a mutant fire hydrant dressed up for Christmas, the vacuum deposition chamber stands in Atwater's lab.

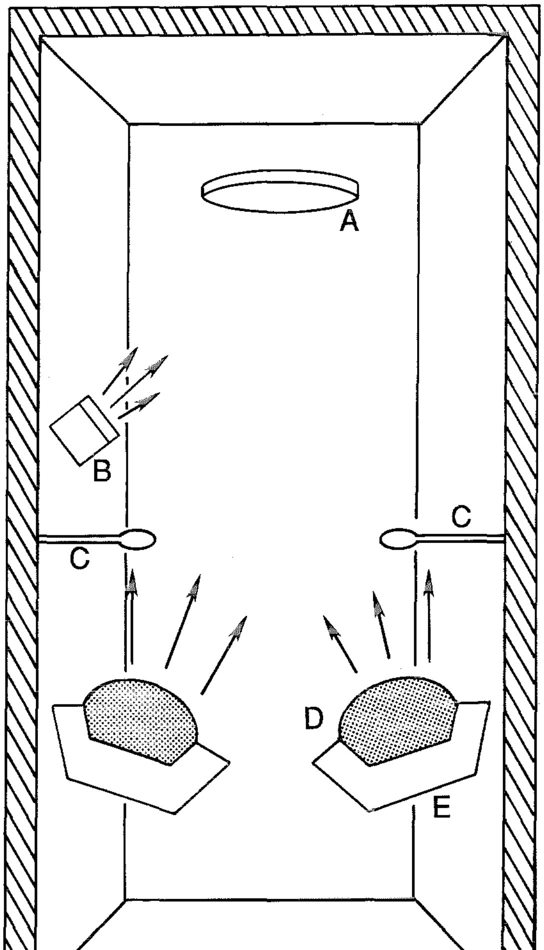
The silicon and gallium arsenide chips that carry integrated circuits or optical devices are really more like nachos. The basic slab—the "substrate"—is layered with multiple thin films of assorted semiconductors to make the finished circuit, just as tortilla chips are slathered with refried beans, sour cream, guacamole, cheese, and black olives to make nachos. The recipe for nachos is largely a matter of individual taste and what's available in the fridge. Chip recipes, however, call for a very specific composition for each layer—five germanium atoms per every 95 silicon atoms, for example. But what actually winds up there merely approximates the recipe, and the chip won't work if that approximation strays too far. Chips are made inside sealed vacuum chambers, and controlling what goes on within is still to some degree a black art. It's easy to sample the finished product, but it's far too late to rescue a bad batch by then. Tasting a layer as it is being deposited is a challenge. Most of the methods used on finished chips either won't work in the chamber's harsh conditions or get in the way of the deposition process. The methods that have been developed for minding the stove generally don't give very detailed information, or don't work fast enough, or work only with certain semiconductors.

A new method, called reflection electron energy-loss spectroscopy or REELS, reveals the composition of any kind of semiconductor layer as it grows, and can be added to a standard chip-making chamber by essentially bolting it on. The method was developed by Harry Atwater, assistant professor of applied physics; Shouleh Nikzad, a postdoc in Atwater's group; and Channing Ahn, senior research fellow in materials science and lecturer in materials science. "A lot of very sensitive tools for analyzing thin-film surfaces have been developed in the last 30 years," says Atwater, "but this one can be done during growth."

Chips are grown by a process called epitaxy. (Epitaxy means that the atoms in the growing layer align themselves with the crystal structure of the layer below, nesting like rows of Lego blocks. The layer-to-layer alignment is crucial—a typical ultra-large-scale integrated-circuit or optical-device chip has more than a dozen layers on it at any given spot, all of which must be seamlessly stacked for the device to function.) The substrate, a three-inch-diameter wafer, hangs upside down in the high-vacuum chamber. Below the wafer sit hemispheric lumps of pure silicon, gallium, arsenic, or whatever—the source of the layer material. Each lump is roughly the size of a golf ball cut in half. Layer deposition begins after the wafer has been heated up to about 400°C and the chamber pumped down to five-billionths of atmospheric pressure. A heating element on the flat underside of each hemisphere heats the lump until atoms evaporate off of its surface. As the vaporized atoms waft upward, they stick to the substrate positioned just overhead to collect them. If the layer being grown is an alloy of two or more elements, the proper mix of atoms in the vapor is set by adjusting each lump's heater. But things start to get complicated when the atoms reach the substrate. Some atoms prefer to cling to their fel-



From left: Atwater, Nikzad, and Ahn admire their handiwork. The aluminum foil swaddling the vacuum chamber insulates it.

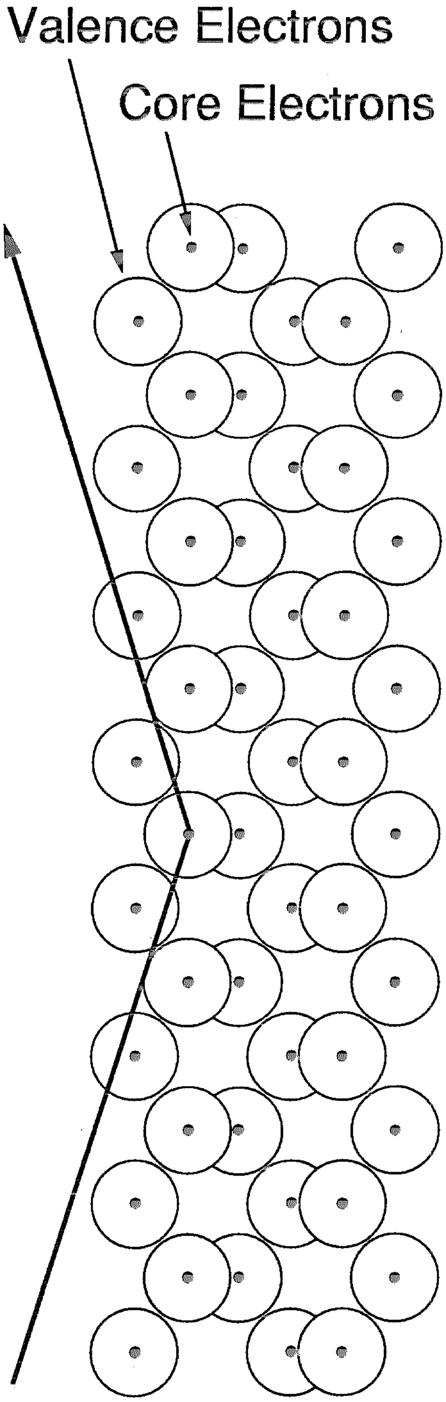


A schematic look inside the vacuum chamber. The wafer to be coated (A) hangs upside down at the top of the chamber. Stray gas molecules are swept up and stuck to the chamber walls by argon ions emitted by an ion gun (B). Thickness monitors (C), placed in the flight path of the vaporized atoms, vibrate like tuning forks. Some of the atoms deposit themselves on the monitors, increasing their mass, which lowers their resonant frequency by a measurable amount. Back-calculation gives the number of atoms deposited on the monitor, and, eventually, the thickness of the layer deposited on the wafer. A lump of the material to be deposited (D) sits in its heater (E) like a muffin in its pan.

lows instead of mixing uniformly with the other alloy components. Some components, as well as many contaminants, tend to float on the growing surface like the foam on a pitcher of beer as it fills. And some atoms just plain don't stick very well. "As in any sort of process that you're trying to control, there's often a deviation between the surface that you intend to make and the surface that's actually formed," says Atwater. "The surface composition can change at a rate equal to the material's growth rate." With real-time information about what's actually growing, the heaters could be adjusted to compensate. A thin film typically grows a fresh monolayer—a layer one atom thick—every second or so.

Many promising ideas for monitoring the process had been tried before. One method collects and counts the atoms that bounce off the substrate, in order to back-calculate how many atoms stuck. This method works well and can be used on gallium arsenide chips, but not for, for instance, silicon and germanium, whose atoms stick so well that there's not much to collect. The most obvious way to look at something you can't touch is to shine a light on it—spectroscopy. Unfortunately, those portions of the spectrum so beloved of chemists—the infrared, visible, and ultraviolet—are absorbed and emitted by the bonds between atoms rather than the atoms themselves; and photons—particles of light—tend to penetrate too deeply into the crystal, muddling data from the growing surface with "noise" from the interior. Another method uses polarized light: under some circumstances, the plane of polarization changes when the grow-

Like a brick wall leaving its imprint on the face of a cartoon character who runs full tilt into it, an atom reflecting an electron sometimes leaves an identifying mark.

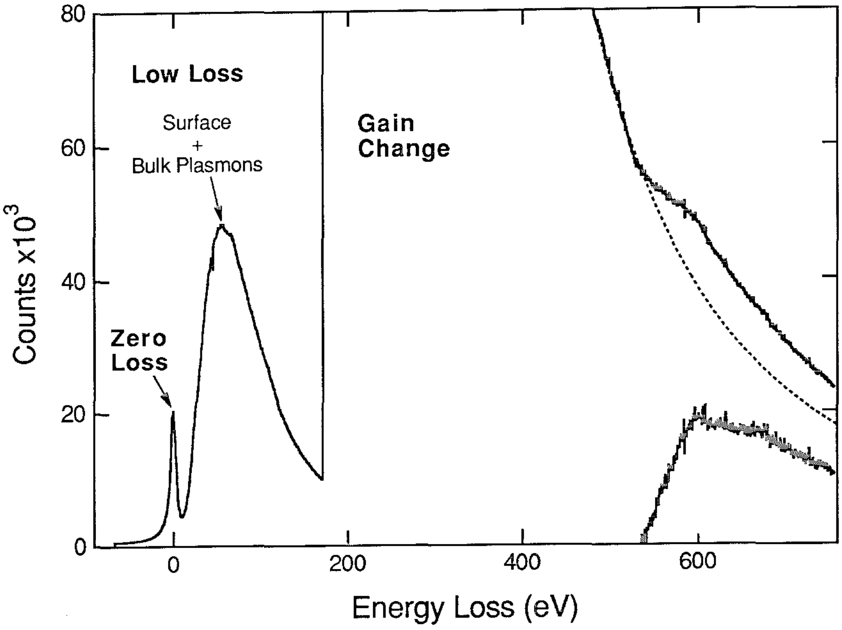


An electron (bold arrow) entering a crystal at a shallow angle can travel some distance before being reflected, yet not penetrate very deeply. Like ranchers in Montana, the valence electrons that define an atom's boundary are spread thinly over a vast region. The core electrons, on the other hand, cluster around the atom's nucleus like suburbanites around the metropolis.

ing crystal reflects the light. Crystals of gallium arsenide, for example, consist of alternating monolayers of gallium atoms and arsenic atoms, and arsenic atoms have a slightly greater affinity for electrons than do gallium atoms. Thus each arsenic atom acquires a very slight negative charge at the expense of a neighboring gallium atom, which becomes equally positively charged. These partial charges balance each other out inside the crystal, but don't always at the growing surface, what with fresh atoms coming in all the time. These locally unbalanced charges, or dipoles, rotate the plane containing the electric component of the electromagnetic wave that we call light. The amount of rotation oscillates through a regular cycle as the crystal fills itself in. One complete cycle corresponds to the deposition of one monolayer each of gallium and arsenide atoms. The way the rotation varies with time shows how fast the crystal is growing, and, indirectly, what its composition is. Unfortunately, the method only works for compound semiconductors such as gallium arsenide. It doesn't work for silicon semiconductors, which are the industry's mainstay. And, even in the best cases, "it's a very difficult measurement, because the polarization change is so small. It's only been shown to be possible at the hands of experts. The casual user probably won't have much success."

REELS uses the next best thing to photons: electrons. A beam of electrons, shining at a shallow angle to the crystal, is both reflected and diffracted, like a beam of light. The diffracted electrons are useful in their own way, but the reflected electrons carry the critical data. Like a brick wall leaving its imprint on the face of a cartoon character who runs full tilt into it, an atom reflecting an electron sometimes leaves an identifying mark. The mark isn't a physical imprint—instead, the electron loses a characteristic amount of energy to the atom.

If all the reflected electrons bore the mark, things would be very easy—a beam of known energy fired into the sample would produce a reflected beam of lower energy, and the energy difference would show what the beam hit. In fact, the reflected electrons cover a whole spectrum of energies, from the input energy—typically 30,000 electron volts (eV)—on down. (One electron volt is the amount of energy imparted to an electron passing through a one-volt electric field.) Plotting the number of electrons versus the amount of energy lost reveals that several things are happening. A sharp spike at zero electron volts shows that a goodly number of the electrons collide "elastically" with the

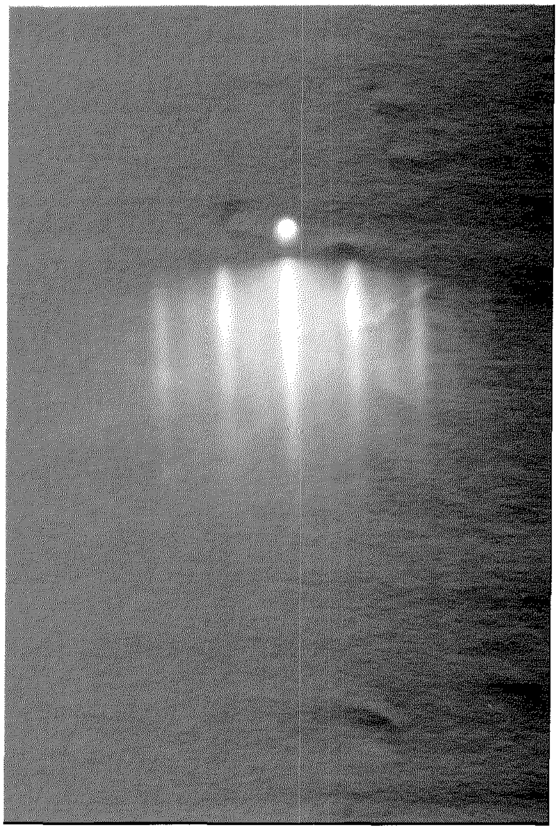


A typical energy-loss spectrum, in this case from oxygen atoms in a silicon dioxide film on a silicon substrate. The vertical line at "gain change" shows where the recorder's sensitivity was increased by a factor of about 1,000. Even so, the core-loss peak is quite small—it's the difference between the solid line and the dashed line showing the continuation of the plasmon peak's tail. Subtracting the plasmon peak out and doubling the remaining core-loss signal gives the peak in the lower right-hand corner.

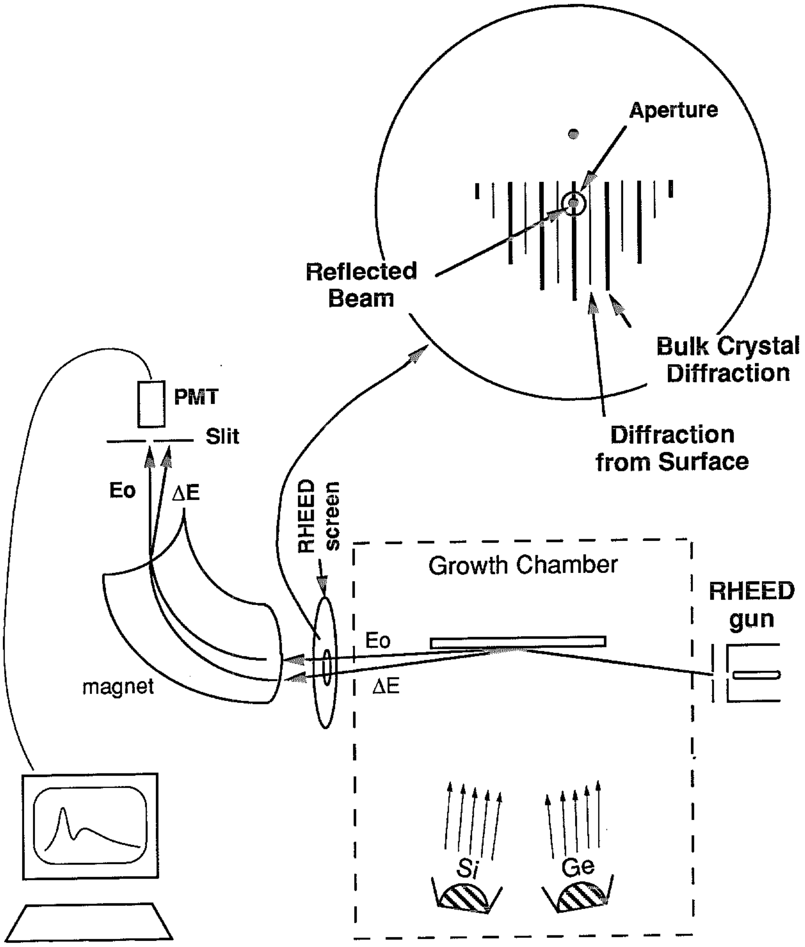
atoms, losing no energy at all. The main peak comes a little further down the energy-loss spectrum—somewhere around 20 eV—but, unfortunately, it also contains no useful information. It represents what are called "plasmon modes," in which the outermost, or valence, electrons of many atoms in the crystal get excited simultaneously to shared energy levels. These shared levels don't reveal anything about the individual atoms, but they're where the vast majority of the reflected electrons leave the atoms they hit. Moving further down the spectrum, the marked electrons—the ones with the characteristic energy loss—show up as a tiny bump on the tail of the plasmon peaks. These electrons have lost their energy to the "core electrons" that make up an atom's inner shells of electrons. Core electrons are very tightly bound to their atoms. It takes a lot more energy—hundreds or thousands of electron volts—to excite these electrons. "The excitation energy of a core electron is strongly dependent on the atomic number—the number of protons in the atom's nucleus—which also determines the atom's elemental identity," notes Atwater. "And there is often more than one observable core-level excitation energy for a given element, which allows you to determine its signature even more unambiguously. So there's a built-in cross-check."

The REELS system uses an electron gun mounted inside the chamber to fire a beam of electrons toward the growing crystal at a very shallow angle—about 2 degrees. The detector is 90 degrees' worth of curved, tubular magnet positioned so that the reflected electrons will

Below: A RHEED pattern. Electrons diffracted by atoms within the crystal cause the bright set of vertical lines, while the dim set is caused by electrons diffracted by the exposed atoms on the crystal's surface. The surface atoms do not continue the interior crystal structure exactly, since they don't have neighbors on all sides to hold them in position.



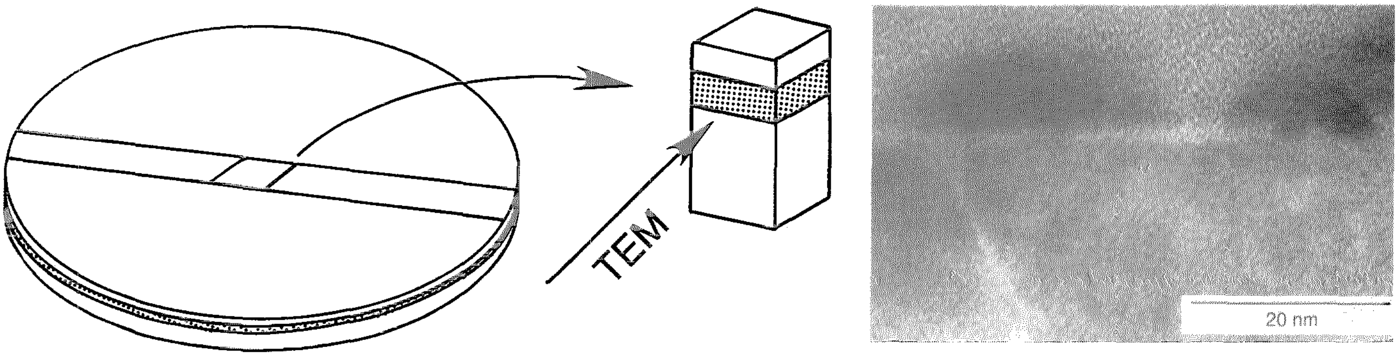
The marked electrons show up as a tiny bump on the tail of the plasmon peaks.



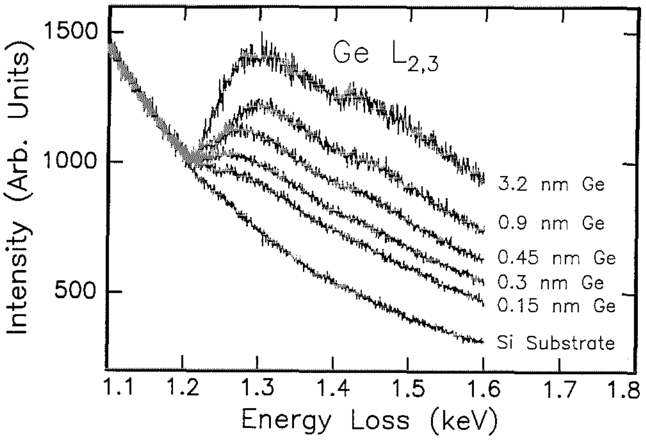
Schematic of the hybrid RHEED-REELS setup. PMT stands for photomultiplier tube. E_0 is the path of the elastically colliding electrons. ΔE is the path of electrons that have lost energy. Si and Ge are silicon and germanium, respectively.

enter it. Once inside the tube, the magnetic field bends each electron's flight path, with the degree of curvature inversely proportional to the electron's energy. An electron having just the right energy for the field strength will coast around the bend, passing through a series of focusing slits to hit a scintillator plate that emits a flash of light on impact. A photomultiplier tube picks up and amplifies this signal, and sends it on to the computer. By starting with a very weak field and then gradually cranking up the power, the REELS system records the number of electrons at all energies.

"We're standing on the shoulders of giants, in a sense, with apologies to Isaac Newton," says Atwater. "There's another method, called RHEED, that is almost a universal tool in this field." RHEED stands for reflection high-energy electron diffraction. As mentioned earlier, some electrons are diffracted by the exposed atoms on the crystal's surface, the way light is diffracted by a grating. The resulting diffraction pattern contains all the information needed to calculate how the atoms that generated the pattern are arranged. "RHEED tells you where atoms are on the surface, but it doesn't give you their chemical identity. Sometimes this is enough information for you to be able to infer the composition, but very often it's not. Suffice it to say that if it were possible to accurately measure surface composition under wide-ranging conditions with RHEED, it would have been done." RHEED also uses an electron gun at a shallow angle to the substrate, but the detector is a phosphorescent screen—like a TV screen—positioned within the chamber in the reflected electrons' path. Each point on the screen lights up with an intensity proportional to the number of electrons hitting that point, making the diffraction pattern visible. A video camera equipped with a framegrabber converts the pattern to a digital image and sends it to a computer that calculates the atoms' positions. RHEED only records the



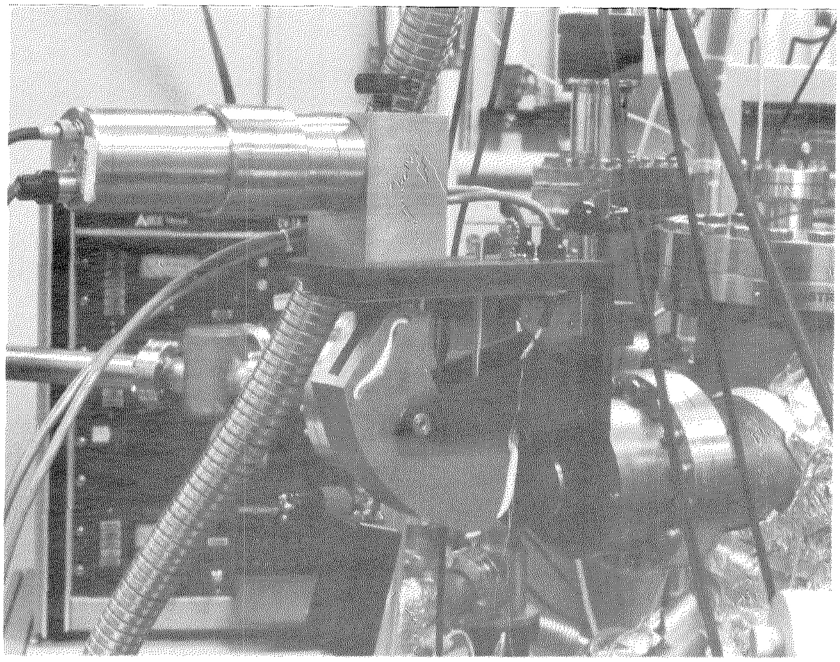
Above: Layers deposited on a chip can be seen directly—if belatedly—by doing transmission electron microscopy (TEM) on a slice, a few hundred angstroms thick, cut from the wafer. The two black lumps (above, right) are germanium islands on a silicon substrate. A nanometer (nm) is a billionth of a meter. Below: REELS picks up a noticeable signal from a layer less than a monolayer thick. A germanium monolayer is about 0.3 nm thick.



cumulative number of reflected electrons; their tell-tale energy spectrum is lost.

Atwater and Ahn realized that a hybrid system could run both methods at once. A half-centimeter-diameter hole drilled in the RHEED screen passes all the electrons hitting the screen at that point on to the REELS detector. Moving the RHEED screen—and thus the hole—around allows the energy spectrum for the electrons from any point in the diffraction pattern to be found. "Various features of the RHEED pattern correspond to different structures on the crystal. Some literally come from the first layer of atoms, others come from a very short distance below the surface. So if you focus a particular part of the pattern into the REELS spectrometer, you can infer where those electrons came from."

The electrons return useful information from a depth of less than a monolayer's thickness to at least 10 monolayers down—roughly 20 to 30 angstroms (ten-billionths of a meter)—and possibly deeper. REELS can tell how deep the electron got before being reflected, allowing the experimenters to discriminate between the monolayers. "If the chemical makeup of the third monolayer is very different from the fourth one—say, if monolayer three is germanium atoms and monolayer four is silicon—we can identify each monolayer unambiguously. If the monolayers vary gradually, then it's somewhat harder, but we can still tell approximately how deep we're looking. It surprised us that the physics was as simple as it turned out to be. We assumed—correctly—that most electrons reflect once, lose energy, and come back out.



The REELS detector. The cylinder at the upper left is the photomultiplier tube, with the curved magnet visible below it.

The mean free path that an electron at our incident energy will travel before it hits something is well known from other experiments. We estimated what the odds of our electron reflecting should be as a function of the film's thickness, and it's a beautiful fit to what we observed.

"This is essentially a material-independent technique. We've looked at gallium arsenide layers as well as silicon. Typically, of course, we're using this tool in an environment where we have a pretty good idea of what we're depositing; we just don't know exactly how much. And one always has to be wary of unintended contaminant species. This method, in fact, has shown a depressing level of sensitivity. It's been a real eye-opener."

Their current setup uses a borrowed magnet-sector spectrometer—the curved magnet—as its detector, and a conventional RHEED gun as the beam source. "Nothing has been optimized yet. We're working at a much lower beam energy than the magnet was designed for—we had to modify the power supply to keep the beam from hitting the magnet's side walls, and that wasn't trivial. Nor is this the world's fastest spectrometer—it takes us a few seconds to collect a complete spectrum. This idea is a very modest extension of existing hardware, so it should be very easy to adopt, but it's also a very powerful method. From what little we've seen so far, it's the most promising technique for watching surface growth." Adds Ahn, "It's so simple that everybody we've talked to is a little bit amazed that no one had tried it before." □—*DS*

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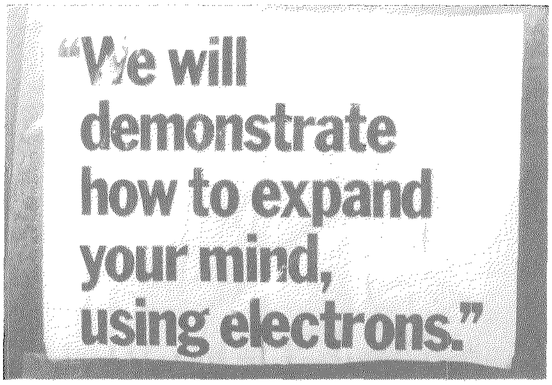

"We will demonstrate how to expand your mind, using electrons."


Tom
Everhart

Commercialization of Technology: Key to Competitiveness

by James D. Watkins

Secretary of Energy James Watkins and Caltech President Thomas Everhart enjoy the festivities at The Associates' annual dinner, where Watkins delivered this address.

The knowledge that comes from aggressive, fundamental research in science and technology is the indispensable base for the competitive posture of our nation. In energy, for example, geoscience research leads to advanced methods of oil and gas recovery, as well as to new techniques for environmental restoration. Research in materials science and engineering leads to stronger, lightweight materials that improve energy efficiency by reducing weight or by allowing machines to operate at higher temperatures. Advancing supercomputer technology and developing better algorithms enables more efficient and more effective design techniques to be used in a hundred fields. And the list goes on. At the Department of Energy, our challenge is to support the search for fundamental knowledge, and then to help translate that knowledge into practical applications for the U.S. economy and defense.

But are we losing the competitiveness race? By many measures the answer is, alas, a resounding "yes." Just look at the shelves of your local consumer electronics store. Many technologies pioneered by the United States have been "adopted" by foreign firms, improved upon, and sold back to us. Other nations are highly leveraging our initial research investments and reaping the benefits. In 1970, for example, U.S. firms had 90 percent of the color television market in the United States. By 1987, that market share had dropped to 10 percent. Similarly, while audio- and videotape recorders are U.S. inventions, U.S. companies have only a small share of the lucrative domestic consumer market today.

Why are we losing the race?

Even more disturbing to me, however, are the significant losses in the basic industrial strength that supports our national and economic security. For example, the United States once dominated the machine-tool industry. Today, the Department of Energy depends on suppliers in Japan, Germany, Switzerland, Korea, and other countries to provide the machine tools, precision measuring devices, and specialty metals needed to run our defense production lines. Our country has seen similar declines in industrial strength in steel, textiles, microelectronics, appliances, and automobiles. As a result, in the last 15 years alone, the overall U.S. merchandise trade deficit has gone from near zero to over $100 billion in the red.

Why are we losing the race? The root of the problem is certainly not in the nation's current research capability; the United States continues to be the world leader in basic scientific research, defense, and space technology.

- The National Science Foundation estimates total U.S. expenditures for research and development in 1989 at approximately $132 billion, with $62 billion provided by the federal government. This level of expenditure exceeds the combined research and development expenditures of Japan, Germany, France, and Great Britain.
- U.S. scientists are awarded more Nobel prizes than any other nation.
- In spite of recent setbacks, we still lead in space exploration.
- And, students from all over the world prefer to study in this country.

Mary Lidstrom, associate professor of applied microbiology, studies the genetics of methylotrophs, methane-eating bacteria that also devour chlorinated hydrocarbons, a principal component of soil and groundwater pollution. A DOE program that funds basic research in microbiology is helping her develop an energy-efficient process of environmental cleanup.

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So, the problem is not research and development. Rather, the problem lies in our inability to commercialize scientific and technological discoveries at a pace and scope equal to many of our international industrial competitors. The Department of Commerce recently published a survey of 12 "Emerging Technologies" that feature a combined global market potential of $1 trillion in annual product sales by the year 2000. According to that report, we are already behind Japan in five of these technologies: advanced materials, advanced semiconductor devices, digital imaging technology, high-density data storage, and optoelectronics. If current trends continue, in 10 years, we can also expect to be behind Japan in biotechnology, superconductors, high-performance computing, medical devices and diagnostics, and sensor technology—or a total of 10 technologies out of the 12. Unless we can learn to move the fruits of both public and private research into the marketplace faster and with more certainty, these R&D investments are at best "sunk costs," and at worst lost opportunities to regain badly needed economic strength for future growth.

It doesn't have to continue this way. In fact, we know that the United States can still compete effectively, not only at home, but in foreign markets as well. According to a recent *Washington Post* article, U.S. products are making significant inroads into selected Japanese markets. Our success stories include IBM computers, Kodak film, Microsoft computer software, and even Domino's Pizza.

The success of our economy has traditionally depended on the entrepreneurship of private firms. This is as it should be. But the federal government, including the Department of Energy, does have an important role to play in enhancing U.S. competitiveness. This role can range from taking steps to lower the cost of capital to developing more favorable trade policies. I believe that one of the most powerful ways for the government to help is to make it easier for U.S. industry to obtain the results of federally sponsored R&D, both through licensing and through collaborative research. And we need to facilitate that access in a way that allows industry to transfer new knowledge expeditiously into useful made-in-the-U.S.A. products and services.

In addition to its widespread support of university research, the federal government operates one of the largest and most extensive "machines" for research and development in the world. The complex of more than 700 federal laboratories accounts for one-sixth of the nation's total R&D spending. DOE is responsible for the largest of these facilities, the nine multi-program laboratories, three of which are in California: Lawrence Berkeley and Lawrence Livermore National Laboratories, which are operated for us by the University of California; and Sandia-Livermore National Laboratory, operated by AT&T. These labs represent a significant intellectual resource. All told, the DOE laboratories and production facilities employ more than 25,000 mathematicians, scientists, and engineers in nearly every scientific discipline.

These scientists and engineers have an impressive track record for excellence in innovation.

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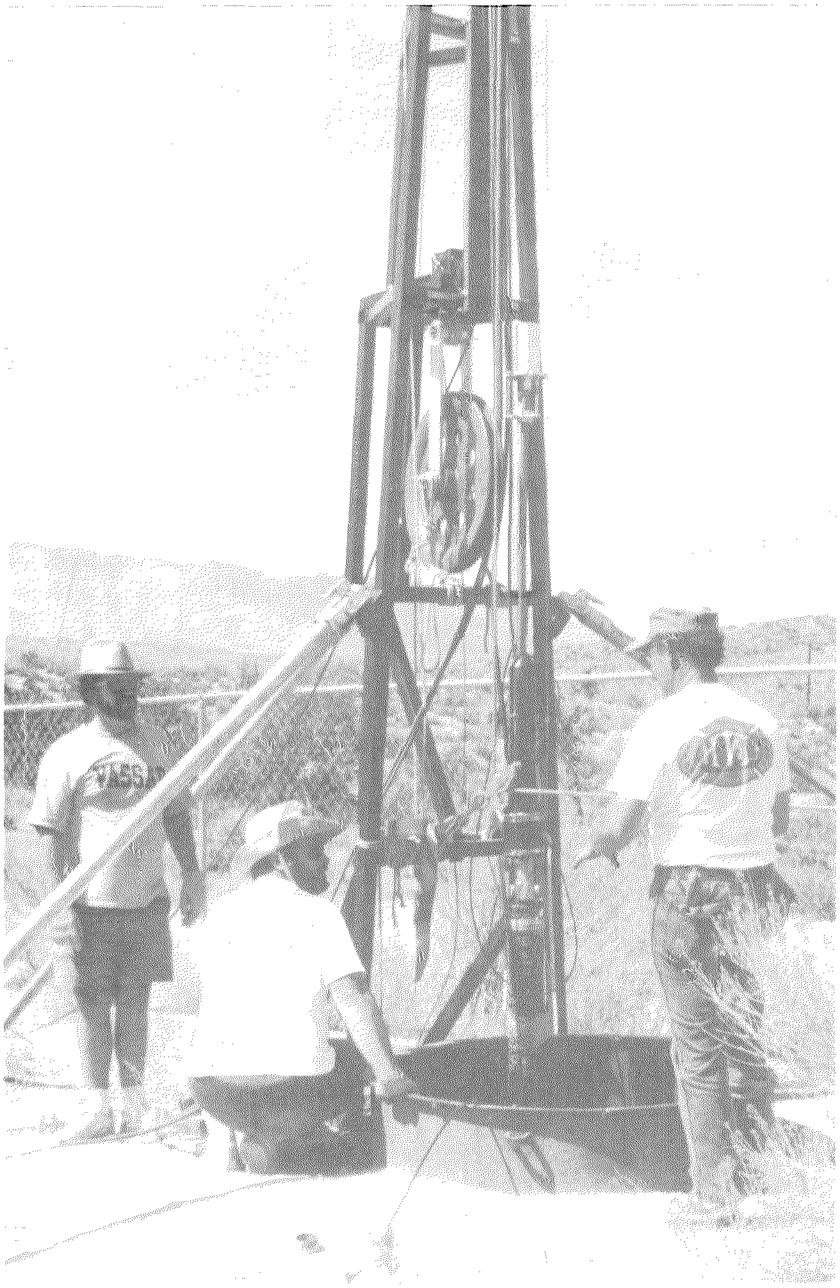


Professor of Geophysics Tom Ahrens (right), grad student Scott King (left), and technician Bob Taylor conduct field tests of their holographic stressmeter near Palmdale. Measuring stress in deep fluid-filled boreholes by interference holography provides information important for enhanced oil recovery. Other potential applications include geothermal resource exploitation and the monitoring of underground waste-disposal sites. A DOE grant is funding Ahrens's work.

The DOE laboratories are star performers in the R&D 100 Awards—an international competition that *Research and Development* magazine conducts each year to identify the 100 new products, processes, and materials deemed most significant from a technical perspective. Over the past 27 years, employees of DOE's laboratories and facilities have won more than 275 of these awards. While I don't have quite the same level of statistical detail to quote about the contributions that come from our support of research and development in universities, I know they are equally impressive.

Since 1963, 45 percent of DOE's award-winning technologies from its national laboratories have been commercialized, motivating the formation of 29 companies; laboratory employees were directly involved in establishing 76 percent of them. These are only a part of the larger universe of all spin-off companies that have started with DOE-developed technologies—more than 140 just since 1985. In the 1988 fiscal year alone some 25 new companies were reported in fields ranging from in-situ vitrification of waste to process-design technologies for the biochemical industry.

In spite of these successes we have barely tapped this wealth of talent. Numerous studies by recognized experts on U.S. R&D policy, including the President's Commission on Industrial Competitiveness, have suggested that the contribution of DOE and other government-funded laboratories to industrial competitiveness can be increased substantially. For example, although licensing of technologies from DOE

We have learned a great deal about what we on the federal side can do to help bridge the gap.

laboratories has increased significantly in the last four years, income from such licenses in the 1989 fiscal year totaled much less than one percent of the total funding provided to the laboratories. There is no question that these and other measures of technology transfer can be improved as laboratories, universities, and private industry work together more closely toward the achievement of mutually beneficial goals.

For over a year now, the Department of Energy has been immersed in the development of a national energy strategy. Over the course of this process, we have become convinced that transferring the results of federal research and development from the labs and universities to the private sector is one of the keys to achieving the nation's energy, environmental, and economic goals. We have heard oral testimony from hundreds of industry, university, and government representatives and have received and reviewed more than 20,000 pages of well-thought-out written testimony. From this we have learned a great deal about what we on the federal side can do to help bridge the gap—the gap between the point at which federal research and development typically stops and industrial commercialization typically starts. These lessons include several important factors.

Market Pull

Last March, I hosted a Technology Transfer Round Table in Washington with the Secretary of Commerce. According to the 24 participants from government and industry, market pull is essential to the success of technology transfer. DOE and its laboratories must develop a better understanding of what U.S. industry, driven by consumer demand, wants to commercialize. And industry must better understand what federal researchers are capable of providing. To do this, we need to bring industry into the federal R&D planning process much earlier.

Cost-Shared Research

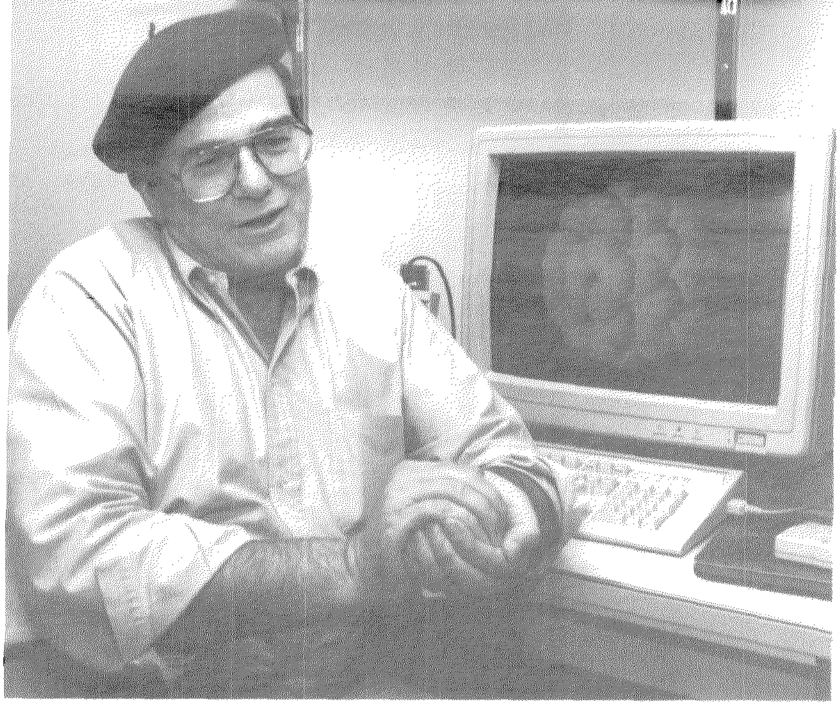
Many of DOE's more successful technology transfer programs involve the use of cost-sharing between DOE and industry. From our point of view, cost-sharing by industry serves as a measure of industry's interest in the technology as well as a way to leverage federal funds in times of budget constraints. From industry's point of view, cost-sharing by DOE serves as a means of reducing the risk of developing technologies that are potentially "market sweeping" in the long term.

One of the programs that successfully combines market pull and cost-shared research is the Energy-Related Inventions Program, which DOE runs in conjunction with the Department of Commerce. From 1974 to 1988, 88 technologies that were supported by this program generated a cumulative sales revenue of more than $400 million. This is a return of $7 for every $1 provided by the federal government. More than 700 jobs were created by the program, and in 1988 alone $3.2 million was returned to the Treasury through tax revenues.

Intellectual Property Protection

The U.S. has had a history of broad and rapid dissemination of results of its basic scientific research programs. Even results of nonclassified basic research associated with defense missions have been made widely available. This is appropriate, and we must continue to support world cooperation in understanding and advancing basic scientific principles. We know, however, that U.S. industry places a premium on protecting information with potential commercial value (in the form of patents, licenses, and copyrights), which can lead to a competitive advantage in the marketplace. We're also aware that the protection of intellectual property is a tough issue in the university community as well.

In the last few years we have "piloted" limited restrictions on the broad dissemination of information through specific applied-energy programs, such as the High Temperature Superconductivity Pilot Centers and the Clean Coal Tech-



DOE funds have contributed substantially to the new Materials and Molecular Simulation Center of the Beckman Institute. Here, William Goddard III, the Ferkel Professor of Chemistry and Applied Physics, explains a new molecule called "buckminsterfullerene"—60 atoms of carbon packed together in the shape of a soccer ball. Simulations in Goddard's lab have predicted the "bucky ball's" crystal structure and its vibrational and other properties, predictions helpful in characterizing such molecules, which may have useful applications in nanoscale devices, low-temperature tribology, and low-friction, high-temperature coatings.

nologies Program. According to several industry participants in the Pilot Center program, they would not be working with DOE today without this important protection. As a result of this success and others, Congress has extended the ability of all agencies to restrict the release for a designated period of information of potential commercial value under certain types of agreements. We feel this will be an important signal to industry that the federal government is serious about enhancing U.S. competitiveness in partnership with the private sector.

Collaborative Research

Technology transfer is a contact sport.

A phrase frequently used, and so true, is worth repeating: "Technology transfer is a contact sport." Much of the "technology" that can be transferred from the federal laboratories is not hardware on the shelf. Instead, it is in the form of knowledge contained in the minds of our scientists and engineers. In order to transfer that knowledge effectively, people must interact.

In July, we took some steps to further improve the access of industry to state-of-the-art technology developed in our national laboratories, particularly those believed by most to be serving defense programs exclusively. We signed two agreements, one with a consortium of specialty metals industries and the other with a consortium of manufacturing industries. Under both agreements, member companies will be able to work directly with our laboratories' best engineers and scientists. They will also be able to use some of the world's most sophisticated equipment to advance the state of the art in their respective manufacturing technologies. From DOE's perspective, the national laboratories, through this interaction, will gain insights into the technologies and techniques used by industry.

Speed And Certainty

We have learned that the federal government, including DOE, does not make a very good business partner. According to industry, our administrative processes are slow and cumbersome; policies are implemented differently by different agencies and even within different parts of the same agency; and the paperwork alone raises the cost of doing business. In order to encourage industry to work with the federal government through collaborative programs, we have to reduce or eliminate the administrative and legal barriers that now slow the process down and increase the economic risks of new technologies.

The president has established a Council on Competitiveness, of which I am a member, to address this problem. Through the biotechnology working group, the council has established principles that will help us reduce the administrative burdens on biotechnology companies and thus promote the rapid commercialization of new technologies. The council has also taken responsibility for important deregulatory initiatives started under the previous administration. Finally, the council recently formed a new working group on the commercialization of government research.

Under DOE funding Professor of Chemical Engineering George Gavalas (right) and grad students Michael Tsapatsis and Soojin Kim are investigating the use of an inorganic thin film membrane to separate hydrogen from coal gas. Such separation techniques would have applications in many facets of coal utilization and in petrochemical production.

It is not enough to make it easier for industry to work with the federal government. We have to make matches between what industry wants and needs and what the federal researchers can contribute.

Technology Transfer "Agents"

It is not enough to make it easier for industry to work with the federal government. We have to make matches between what industry wants and needs and what the federal researchers can contribute. Further, even when a technology match is made, industry may need additional technical and business assistance. This is where state governments, universities, trade and professional associations, and other organizations have important roles to play.

For example, a program was recently initiated by DOE's Office of Energy Research to transfer advanced materials technologies developed at DOE laboratories to small, high-technology businesses in Michigan. Faculty from two-year (community and technical) colleges play a key role as brokers for transferring information and technology to participating small businesses.

DOE's Lawrence Berkeley Laboratory is participating in a coordinated effort with the California Energy Commission to transfer expertise and technologies to California electric utilities for analyzing energy consumption in buildings of all kinds.

The same California Energy Commission also runs one of the nation's most successful programs to promote energy-technology exports. California companies developing technologies in conservation, geothermal energy, cogeneration, solar electricity, and wind power are assisted through a program that is carefully designed to be complementary to other state and federal initiatives for export promotion. From its inception in 1986 to the present, the commission has stimulated more than $14 million in export sales, and another $45 million in expected sales are projected within the next six months. This translates into $12 in export sales for every dollar invested in this modest program—an outstanding achievement on a limited budget. It also is suggestive of how much more can be done.

But, to succeed, we each have to do our part. Scientists and engineers supported by DOE, in its national laboratories and in universities, are truly a national treasure in terms of their unparalleled capabilities and achievements. I am committed to seeing that these incredible resources are used more effectively to enhance the competitiveness of U.S. industry and the quality of life for U.S. citizens. In fact, the first task of my newly established and very prestigious Secretary of Energy Advisory Board, which is under the able chairmanship of Caltech's president, Tom Everhart, is to develop plans to help me better utilize the intellectual capital in the DOE national laboratories.

These national laboratories are already moving aggressively to improve technology transfer, thanks to the National Competitiveness Technology Transfer Act of 1989. This act made technology transfer a mission of all DOE laboratories and provided an additional mechanism for laboratory-industry-university cooperation, called the cooperative research and development agreements. The act also amended the Atomic Energy Act to make technology transfer an explicit mission of DOE's defense programs. We have mobilized more than a hundred people in the department and its laboratories to develop a program that will provide a fast, flexible, and predictable environment for technology transfer from our national laboratories.

There are, however, some obstacles in the way. One of the most serious problems facing the nation over the next 10 years is the declining number of young Americans who are interested in pursuing careers in science and engineering. Those who may be interested often receive neither inspirational counseling nor adequate preparation for such careers early in their schooling. This is particularly true in the case of women, minorities, and the disabled, who will make up 85 percent of net new work-force entrants by the year 2000. This situation represents a crisis in science and math education that has serious implications for our nation's continued economic and technological competitiveness.

One of the most serious problems facing the nation over the next 10 years is the declining number of young Americans who are interested in pursuing careers in science and engineering.

Just one year ago, I hosted a Math/Science Education Action Conference at the Lawrence Hall of Science in Berkeley, California. My co-chairman for the conference was Nobel laureate Glenn Seaborg. This two-day meeting brought together more than 250 scientists, educators, policy makers, and industry representatives, as well as representatives from the administration and Congress. The conference report, which was released in May, lays out a specific plan of action for the Department of Energy and its laboratories, working in partnership with other federal agencies, such as NASA, and with the states, schools, and private-sector organizations. All of us need to be a part of the solution to this complicated problem, particularly at the pre-college level, where so many potential scientists and engineers are being lost today.

The challenge doesn't stop with basic science and math education, though. We must also teach technology management in our engineering schools and our business schools. According to the March 1990 issue of the *Engineering Management Journal*, 90 percent of high-tech managers feel they are inadequately prepared to lead innovation toward successful commercialization. This is where industry can play a critical part in working with universities to shape the curriculum in this area.

Finally, industry must take the lead in changing the management culture that keeps companies focused on short-term product improvements at the expense of longer-term technological innovation and expeditious commercialization.

I firmly believe that, if our nation can unite around the goal of renewing our competitive edge, we can enjoy unparalleled strength through a decade of advances in research and education in the 1990s. At a minimum, we can turn these advances into a source of competitive advantage for the United States in the fields of energy, environment, and trade—particularly important at a time when so many nations are struggling to enter the world of free markets for the first time.

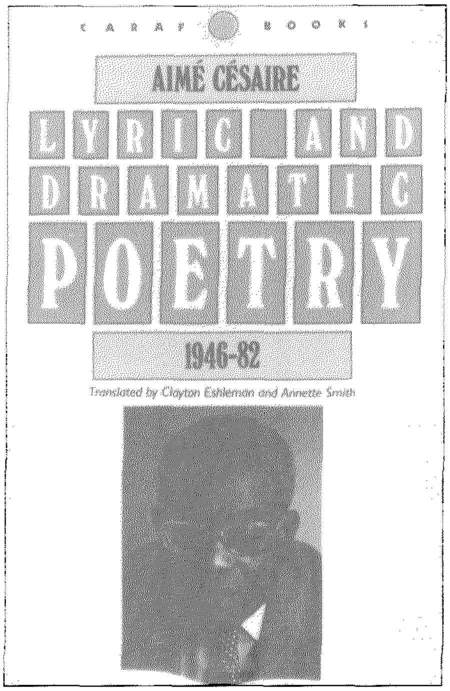
It doesn't have to cost a lot of new money, either. Much of what needs to be accomplished can be done by better leveraging private and public resources that already exist. We can create an impressive payback on our research investment through expeditious commercialization, if we but have the will to grasp the moment. And achieving success will demand a serious change in thinking about how we organize and focus our national resources and institutional processes to meet these global challenges.

There is a saying in Scripture, "Where there is no vision, the people perish." If our nation is to have a competitive future in the world, we need a vision that takes us beyond exigencies of the moment—whether it is today's budget crises in the government or the next quarterly report from the oil industry. We need to see the broader and bigger picture of where we as a nation must go. We need to reach out to each other in new partnerships and alliances, with government, universities, and industry all supporting the three pillars on which our common future rests: research, education, and economic strength through the commercialization of technology. And we need to get serious on an action program to make those partnerships a working reality.

No one of us can do it alone. But together, we stand a fighting chance of ensuring that our country remains number one in the world as we enter the 21st century. □

James D. Watkins, Admiral, U.S. Navy (Retired), Secretary of Energy, delivered the above remarks at the annual black-tie dinner of The Caltech Associates on October 5, 1990. A graduate of the U.S. Naval Academy, Watkins holds a master's in mechanical engineering. He served on the Atomic Energy Commission for three years and from 1982 until his retirement from the Navy in 1986 was Chief of Naval Operations. From 1987 to 1988 he served as chairman of the Presidential Commission on the Human Immunodeficiency Virus (AIDS) and has been Secretary of Energy since March 1989. Watkins is a native Californian and considers Pasadena his home.

Books



The University Press of Virginia, 1990
291 pages

Students of 20th-century literature have long pondered the odd fact that writers from the outlands of English-speaking culture—Missouri (T. S. Eliot), Idaho (Ezra Pound), and Ireland (W. B. Yeats)—have come to dominate our sense of the mainstream of British poetry. Modern French verse evinces a similar conversion of the liminal into the central: Aimé Césaire, a black man from the Caribbean island of Martinique, is recognized as one of the major Francophone poets of our time. Most of his poetry was made available to the English-only audience in *Aimé Césaire: The Collected Poetry*, translated by Annette Smith, professor of French at Caltech, and Clayton Eshleman, a contemporary poet and professor of English at Eastern Michigan University (and formerly lecturer in creative writing at Caltech). That standard and highly regarded work, published in 1983, is complemented by this new volume from the same two translators. It contains Césaire's *And the Dogs Were Silent*, written in the 1940s and not previously translated, and *i, laminaria . . .*, first published in French in 1982. To these major works are added a fine critical introduction to Césaire's thought and poetry by A. James Arnold, and Arnold's translation of Césaire's essay, "Poetry and Knowledge," a major statement of his antiscientific views. With these many facets, the volume will be a crucial resource for Césaire scholars and an introduction to both his early and later poetry for the more general audience.

Any consideration of Césaire's poetics cannot ignore his political convictions, for the two have interacted in complex ways throughout his career. He was one of the founders of the negritude movement, an attempt on the part of French-speaking black intellectuals to overcome the hegemony of French culture over their own lives and regain contact with their African roots. *And the Dogs Were Silent*, a verse drama with chorus, is deeply inscribed with the attempt to prepare for political revolution through cultural transformation. In contrast, *i, laminaria . . .* seems less explicitly political and more engrossed in the workings of its verbal nuances. Yet I think it would be wrong to take these changes over the course of some 40 years as an indication that Césaire has abandoned his earlier convictions. The negritude movement was inextricably involved in linguistic issues. Although its initial goal of turning completely away from French and adopting African tongues for the black community proved impractical, the desire to recapture the spirit of African cultures embedded in their languages remains largely intact in Césaire's later poetry. Although his

macumba word

the word is the father of the saints
the word is the mother of the saints
with the word *couresse* one can cross a river swarming with caimans
sometimes i trace a word in the dirt
with a fresh word one can cross the desert in a single day
there are swim-stick words for pushing away sharks
there are iguana words
there are subtle words those are stick-insect words
there are shadow words that awake sparking with anger
there are Shango words
sometimes i even sneak a swim on the back of a dolphin word

mot-macumba

le mot est père des saints
le mot est mère des saints
avec le mot *couresse* on peut traverser un fleuve peuplé de caïmans
il m'arrive de dessiner un mot sur le sol
avec un mot frais on peut traverser le désert d'une journée
il y a des mots bâton-de-nage pour écarter les squales
il y a des mots iguanes
il y a des mots subtils ce sont des mots phasmes
il y a des mots d'ombre avec des réveils en colère d'étincelles
il y a des mots Shango
il m'arrive de nager de ruse sur le dos d'un mot dauphin

work shares with surrealism its fracturing of discursive conventions, the poetry of *i, laminaria . . .* is in many ways a continuation of Césaire's politics within language. Similarly, the antirationalism of "Poetry and Knowledge" is best appreciated as cultural protest, not philosophical speculation.

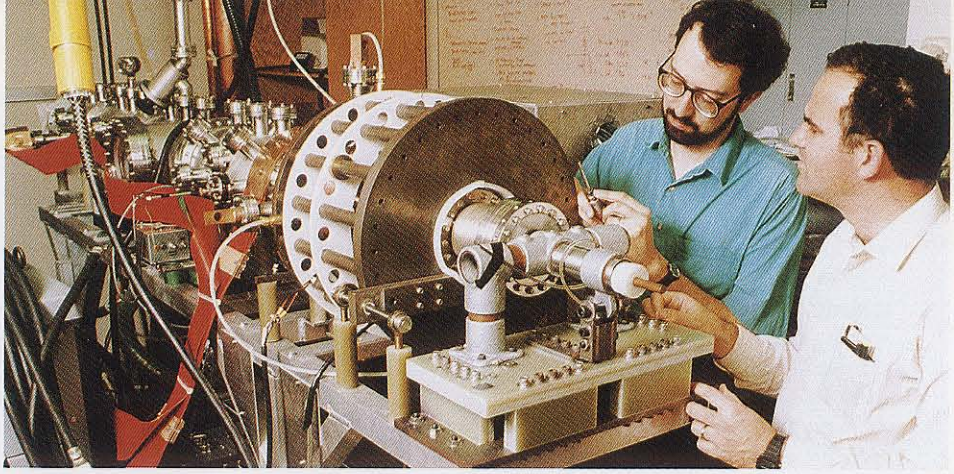
A single poem, above, from the *i, laminaria . . .* anthology of brief lyrics can serve as an epitome of Césaire's talent and interest in the politics of language. The word *Shango* immediately indicates the poem's self-contextualization in African cultures of the Americas, for it is the name of a minor deity, or *Orisha,* in Macumba, a religion among the blacks of Brazil. But the Orishas of the western hemisphere have their origin in Nigeria, thus carrying Césaire and his readers on a journey through a displaced black culture to its African inheritance. Further, the Orishas of Nigeria are often aligned (through a kind of cross-cultural extension of biblical parallelism) with Christian saints, thereby suggesting a connection with the "saints" of the first two lines. This expansive religious syncretism even reaches back to ancient Greece in the final line: the speaking "i" of the lyric compares his own mode of poetic travel to the poet Arion's journey on the back of a dolphin in classical myth.

Operating behind these mythic excursions is a more profound linguistic point in every line of the poem—the desire to return to a more primitive medium of exchange, one in which the words are not arbitrary signs of an alienated world of things, but rather a language in which the words are motivated and motivating symbols that are one with the reality they render intelligible and are capable of empowering speakers with the triumph of subjectivity over objectivity. This is the magic dimension of discourse lost to modern, western culture, as so many poets who define that modernity have indicated. Césaire's political and cultural critique has led him to nothing less than the desire to return to the origins of all languages, and thus all cultures. With the potent words of this original language, one can engender holiness, but also, in a more physical embodiment, cross rivers, seas, and deserts as surely as did Moses and the Israelites.

Smith and Eshleman wisely present the lyrics of *i, laminaria . . .* in parallel French and English texts. Césaire uses wordplay as much as cross-cultural allusion, and many crucial puns cannot be translated. The French word for dolphin, "*dauphin*," also means the eldest son of the king of France, a suggestion that such "dolphin word[s]" carry the authority of a royal edict. Thanks to French-English cognates, however, a few significant puns remain in both versions. For example, "laminaria" (*laminaire*, a species of seaweed) puns on "laminate" —a layering of languages and cultures —and perhaps also on "liminal"—the home of seaweed, on the margin between sea and land, as well as Césaire's own position between France and Africa. The parallel format also helps the translators negotiate that perilous path between a deadening literalism and a creativity swerving far from the original. Their approach is conservative, but not without its own verve and rhythms evoking rather than imitating the original. The incantatory beat of Césaire's "*il y a des mots*" in "macumba word" cannot be fully captured in English, which normally requires the placement of adjectives before, rather than after, nouns, but the translators' repeated "there are" at least calls attention to the pattern and directs even the non-French speaker to glance at the original and appreciate its richness. Lovers of poetry, whether long familiar with Césaire or hearing this major voice for the first time, owe much to Smith and Eshleman for their care, their craft, and their willingness to allow Césaire to speak through their own "*mots subtils.*"

Jenijoy La Belle
Professor of Literature

Lab Notes



Brown (left) and Bellan at the spheromak-generating end of their new gun. Brown is holding one of the fuel injectors used to squirt the puffs of gas into the plasma generator.

Plasma Donor

Faithful readers will recall Caltech's participation in the cold-fusion excitement of two years ago (*E&S*, summer '89). Although the dream of producing controlled nuclear fusion in a jar of water at room temperature fizzled, hot nuclear fusion—which makes energy the way the sun does—may be turning generators in the 21st century. Hot fusion occurs in a "plasma"—matter heated to such a degree that its atoms dissociate into a froth of freewheeling electrons and atomic nuclei. If the plasma is sufficiently hot and dense, the nuclei will slam into one another hard enough to overcome their mutual repulsion and fuse together, releasing energy. The sun's vast bulk of hydrogen acts as both pressure vessel, by compressing under its own weight, and fuel supply, by providing a constant flow of dense, ready-to-burn plasma to the solar interior. The most promising terrestrial design, called a tokamak, confines the plasma within a donut-shaped magnetic field. Stoking a tokamak is a bit like shoveling coal into the hottest furnace imaginable—the stoker has to stand back a safe distance, yet throw the fuel hard enough to get it into the fire. Figuring out how to do this is a basic goal of fusion research.

Most designers throw ice cubes into the tokamak to fuel it, but Paul Bellan, professor of applied physics, is blowing bubbles into it instead. "The conventional method is to shoot little pellets of frozen deuterium—heavy hydrogen—into the tokamak with some variation of a catapult or a blowgun. These pellets travel at several kilometers per second (several thousand miles per hour), but it's dicey whether they can reach the tokamak's center, where the burning occurs. [Try throwing an ice cube into the center of the sun sometime.] We're looking at a much more speculative refueling scheme that instead shoots small plasmas, called spheromaks, into the tokamak." A spheromak is a donut-shaped plasma, perhaps as large as a pineapple, that's contained by its own magnetic field the way a bubble is enclosed by a soap film. An electric current coursing around the plasma ring generates the field. The field doubles as a handle by which the spheromak can be flung into the tokamak at incredible speeds. Achieving these high speeds is critical to the method's success—the spheromak has to be hurled with enough force for its charged particles to penetrate the tokamak's magnetic field.

Bellan, senior research fellow Michael Brown, and electronic engineer Frank Cosso are building plasma bubble guns with funding from the Department of Energy. The latest model is a horizontal copper cylinder, some six inches in diameter and two feet long. An iron-alloy rod runs the length of the cylinder's axis, and the cylinder is enclosed in a stainless-steel vacuum casing. Like a wire loop supporting a soap film, a "birdcage" of iron bars outside the casing anchors a sheetlike magnetic field that emanates from the rod and stretches across the cylinder's front end. The rod and the cylinder are actually electrodes wired to a bank of 15,000-volt capacitors via an electronic switch called an ignitron. A puff of gas—hydrogen, deuterium, or helium—enters

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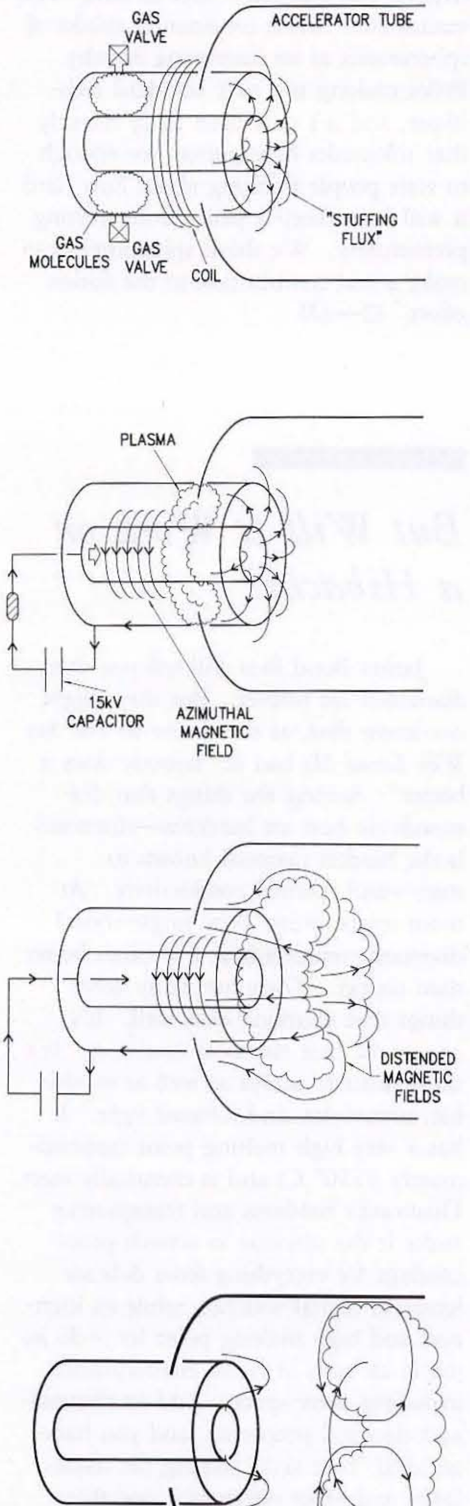
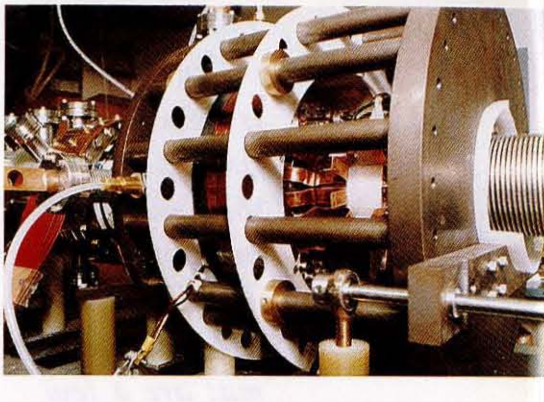




Left: How to blow a plasma bubble. The "stuffing flux" is the sheetlike magnetic field. The "azimuthal magnetic field" blows the plasma bubble. In the bottom figure, the spheromak is cut in half to show its ring current and the reconnected loops of magnetic force.
Right: The "birdcage," with a fuel injector visible at left.

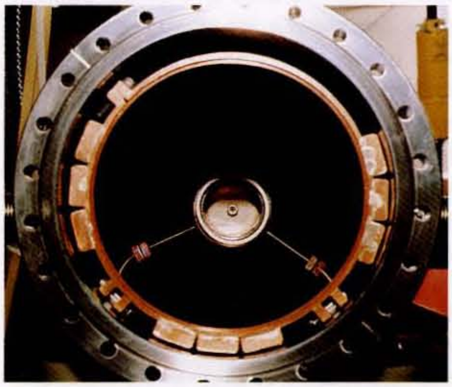
the cylinder just behind the magnetic film. The ignitron fires faster than the gas can disperse. For a few millionths of a second, 200,000 amps arc through the gas, blasting its molecules into plasma. Completing the circuit also sets up a rod-encircling magnetic field in the vacuum between the plasma and the cylinder's rear wall. This second field is the breath that blows the bubble, pushing against the plasma and hurling it into the field stretched across the cylinder. The plasma distends the field until a portion of it breaks free, wrapping itself around the plasma. Says Bellan, "It's like hitting a tennis ball into the net so hard that the ball breaks through, taking a piece of the net with it." The magnetic field, embedded in the plasma like string in a lump of Silly Putty, reconnects its broken lines of force with one another to form the bubble. The field is sustained by a swirl of 200,000-amp current trapped in the bubble.

The spheromak exits the cylinder at a very smart clip. Bellan and Brown's first plasma gun, built in 1987 and operated with help from Summer Undergraduate Research Fellowship (SURF) student David Cutter, now a junior in applied physics, achieved a respectable 65,000 mph. The new gun has an accelerator stage—an additional two meter's worth of rod and cylinder. Here a second capacitor bank creates another magnetic field that boosts the plasma to ramming speed. Bellan is shooting for upwards of 400,000 miles per hour—no mean feat. "While you can accelerate individual particles, like electrons, to this kind of speed fairly easily, a spheromak is a macroscopic object weighing several micrograms. If it were a piece of paper of the same weight, it would be big enough to see with your naked eye."

At more than 400,000 miles per hour, the spheromak will be moving a little too fast for the naked eye. Barry Stipe, a senior in physics, spent last summer designing, building, and testing spheromak detectors as a SURF project. One detector is a set of magnetic induction probes, tiny coils of wire that pick up the spheromak's magnetic field the way that a tape deck picks up music from a cassette. Spaced every five inches along the accelerator stage, each probe registers the magnetic field as the spheromak races by, allowing its speed to be calculated. The other detector, a laser interferometer, measures the plasma's density at a point midway down the accelerator. A high-speed camera, with a shutter speed of 25 billionths of a second, will also be used.

There's another reason for shooting spheromaks besides fuel injection. A tokamak plasma is somewhat like a very large spheromak. The tokamak's ring current needs constant stoking, which is usually provided by induction from transformer coils wrapped around the tokamak. But fueling the tokamak with spheromaks may relieve some of the transformers' burden. According to Bellan, "There's a bizarre theory that says if you shoot a spheromak into the tokamak, the spheromak current merges into the tokamak current. The contribution from any one spheromak is small—it's been compared to throwing flashlight batteries into the tokamak—but the cumulative effect can be substantial. Mike and I were the first ones to see that actually happen, in an experiment last year with our first gun."

The plasma at the sun's core is about twelve times denser than lead, and roughly 15,000,000° C. Tokamak plasmas are a few millionths the density of air, and so have to be hotter in order to burn.



A tokamak's-eye view down the gun barrel, showing both the central rod and the copper cylinder electrodes.

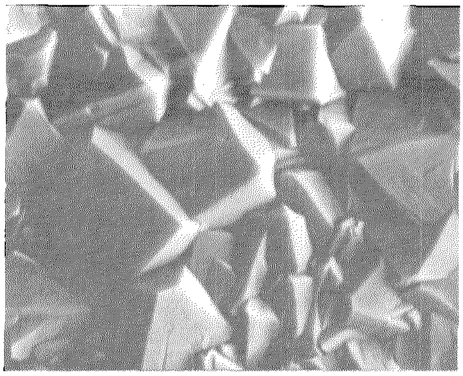
The new gun will be shipped off to the University of Wisconsin-Madison, where it will be mated to a research tokamak there called Phaedrus-T. Phaedrus-T is designed to study how a very hot plasma behaves, and to explore the engineering problems involved in confining it. Each shot from the new gun will replenish about 30 percent of Phaedrus-T's plasma. The gun will only fire about once a minute—insufficient to keep the plasma going, but more than adequate for studying the mechanics of the injection process.

No tokamak has yet achieved fusion. Encore, the Caltech machine where the current-merging was seen, sustains a plasma ring roughly two feet in diameter for a few thousandths of a second. Phaedrus-T maintains its six-foot ring for a tenth of a second. And behemoths at Princeton, New Jersey, and Culham, England, keep 20-foot rings going for about 10 seconds. The next step will be achieve the break-even point, where fusion reactions generate more energy than is consumed in maintaining the plasma. The fusion rate depends on the plasma's density and its temperature. The plasma at the sun's core is about twelve times denser than lead, and roughly 15,000,000° C. Tokamak plasmas are a few millionths the density of air, and so have to be hotter in order to burn—an estimated 100,000,000° C for a sustainable reaction. Spheromaks are a frigid 50,000° C. The new gun's spheromaks are about one-thousandth the density of air, a tenfold density increase compared to the previous gun's output. A power-plant-sized tokamak would probably contain a plasma donut about 40 feet across. Such a plant would need several spheromak guns around its perimeter, each one firing a thousand or more times per second.

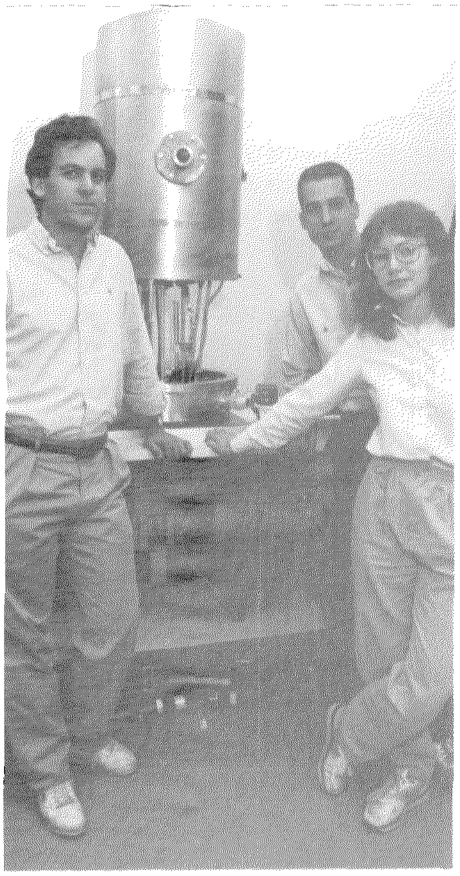
The new spheromak gun's power supply has been successfully test fired, but wiring up the accelerator is proceeding more slowly than anticipated. Says Bellan, "These power supplies aren't off-the-shelf items. They have to be built very carefully so that the power goes where you want it. If you don't keep the system's internal inductance way down, the power is lost before it gets to the gun." The heaviest wiring has to be done with copper sheets—putting that much current through a wire would blow it apart. Once the wiring is done, the gun will be put through its paces before being shipped to Wisconsin, an event Bellan hopes will happen this summer. Says Brown, "The mainstream fusion community thinks of spheromaks as an interesting novelty. Pellet-making is a very standard technique, and it's only been fairly recently that tokamaks have gotten hot enough to start people thinking about how hard it will be to keep a pellet from melting prematurely. We think spheromaks can make a real contribution to the fusion effort." □—*DS*

But Will It Work on a Hibachi?

James Bond fans will tell you that diamonds are forever. But they might not know that, as the theme to *The Spy Who Loved Me* had it, "nobody does it better." Among the things that diamonds do best are hardness—diamond is the hardest material known to man—and thermal conductivity. At room temperature, pure single-crystal diamond conducts heat five times better than copper. There are many other things that diamond does well. It's one of the best electrical insulators. It's transparent to x rays as well as to visible, ultraviolet, and infrared light. It has a very high melting point (approximately 3550° C) and is chemically inert. Diamond's hardness and transparency make it the ultimate in scratch-proof coatings for everything from delicate lenses to digital watches, while its inertness and high melting point let it do its job in all sorts of harsh environments, including outer space. Add its thermal and electrical properties, and you have an ideal "heat sink" coating for diode lasers and other electronics, one that dissipates excess heat while protecting the circuitry.



Left: A typical polycrystalline diamond film, grown by Goodwin's group in a small "hot-filament" reactor that is now being used to grow diamond films in a freshman engineering lab. The individual diamond crystals are three to six millionths of a meter on a side.



Below: (From left) Goodwin, Glumac, and Melnik with their diamond-growing chamber, raised into its "open" position. The apparatus underneath the chamber includes actuators that can move the burner and the substrate independently, to adjust the spacing between them, or as a unit, to bring the instruments to bear on any part of the flame.

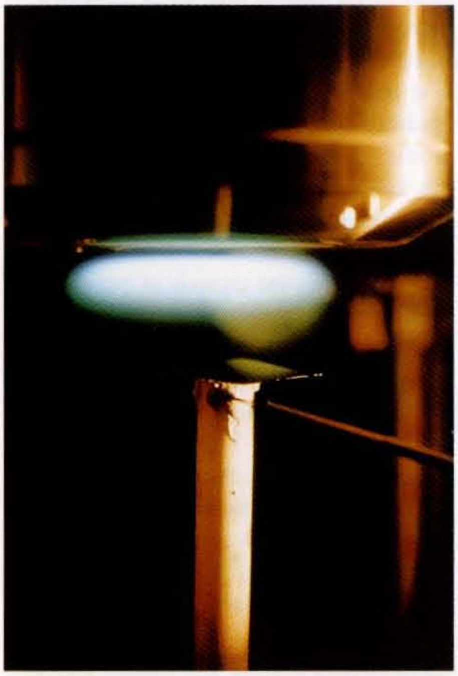
Exploiting all these wonderful attributes in a coating obviously requires that the coating be formed without destroying the object being coated. Since 1958, diamonds have been synthesized industrially—to the tune of $1 billion's worth of abrasive grit and cutting-tool coatings a year—by mimicking the conditions that occur some 75 to 90 miles underground, where diamonds form naturally. However, such high pressures (about 50,000 atmospheres or 1,000,000 pounds per square inch) and temperatures (1500° C) lack the finesse required to make, say, scratch-proof coatings for watches. More recently, methods have been developed that deposit diamond films from a hydrocarbon vapor at low pressures and comparatively mild temperatures, greatly expanding the list of materials that can be coated.

Assistant Professor of Mechanical Engineering David Goodwin's research group is exploring some of those methods. Goodwin, with help from Guillaume Gavillet (MS '89), has developed computer models of the steps leading up to deposition, while Goodwin and grad student Susan Melnik have been collaborating with a group led by William Goddard, Ferkel Professor of Chemistry and Applied Physics, to develop a theoretical description of exactly what happens when an atom hits the growing surface. "There are a lot of different growth-chamber designs," says Goodwin, "and a lot of ways of adding energy to the vapor. They all have different characteristics." Although the details vary, the overall scheme prescribes a gaseous hydrocarbon—methane or ethylene, for example—flowing through some energy source such as an electric arc, a hot tungsten filament, or a flame. The zap (or the heat) dissociates some of the hydrocarbon molecules into fragments (called "radicals") and individual hydrogen atoms. Generous amounts of hydrogen gas are usually present as well, some of which dissociates to create even more loose hydrogen atoms.

Diamonds are crystals of pure carbon, so those hydrogen atoms might seem superfluous, but they're actually the key to the whole operation. The hydrogen atoms "activate" the growing diamond surface. They react with the exposed carbon atoms, forming carbon-hydrogen bonds that stick out perpendicular to the surface. An incoming hydrogen atom can then pick off one of these surface-bound hydrogen atoms, forming a molecule of hydrogen gas and momentarily leaving the carbon end of the former carbon-hydrogen bond dangling from the diamond surface. A hydrocarbon radical—the methyl radical CH_3, for example—can then attach itself to the dangling bond in an atomic version of musical chairs. Atoms don't like to have a bond dangling free, so were it not for the place-holding hydrogen atoms, diamond's three-dimensional crystal—an array of tetrahedrons joined at their points—wouldn't form. Carbon prefers to form flat layers of graphite, a two-dimensional crystal whose molecular structure resembles chicken wire.

Most of the dissociated hydrogen atoms recombine to make hydrogen molecules long before reaching the carbon surface. According to Goodwin, "You really need to do fairly detailed modeling that simultaneously solves for the chemistry happening in the gas, the surface chemistry, the fluid mechanics of

The diamond-growing flame burns upside down over the substrate, which is the small square tile in the middle of the picture. The glass tube supporting the substrate allows gas samples to be drawn from the flame while it's burning. A thermocouple, leading off to the right, measures the flame's temperature.



"Under certain conditions, an oxyacetylene welding torch burning in the open air can deposit a diamond film."

the vapor's bulk motion, the heat transfer, and the diffusion of the atoms, in order to get some idea of how the hydrogen-atom concentration at the surface depends on the chamber's parameters. Our models are the first ones in this particular field that model the whole problem in detail.

"One easy way to make diamond is in a flame. Yoichi Hirose in Japan first demonstrated that under certain conditions, an oxyacetylene welding torch burning in the open air can deposit a diamond film on a substrate. The film grows quite fast—50 microns (millionths of a meter) per hour—and the quality, if you do it right, is no worse than other techniques and in many cases better." The catch is that the area covered by the film is only as big as the flame's diameter, typically about a quarter of an inch—far too small for most applications. And the flame, although an improvement over the high-pressure method, isn't exactly the mildest of environments. Many potential substrates simply can't take the heat. Trying to cool something that you are simultaneously torching seems like an exercise in futility on the face of it, and things are further complicated by the fact that diamond won't form unless the substrate is heated to some minimum temperature. The welder's torch, with its tightly focused nozzle that produces a pencil-thin flame, is obviously the wrong design for this job. Building an infinitely large blowtorch out of an array of small nozzles would cover a larger area, but at the expense of exacerbating the cooling problem and creating the new problem of exorbitant fuel consumption. What's needed is some way of making a much more diffuse flame.

Goodwin and grad student Niko Glumac have borrowed a burner design, frequently used in combustion studies, that makes just such a flame. The burner's business end is a sintered brass plate the size and shape of whatever's to be coated. The gas mixture seeps up through the porous plate, forming a hovering miasma just above it. When ignited, the gas burns evenly across the plate's entire surface, rather like marsh gas on fire in a bog. "We'd like to deposit a diamond film over a four-inch-diameter area. A lot of people have that goal right now—it's the size of a silicon wafer. Ultimately we'd like to do much larger areas, like turbine blades, but for now, four inches is a very large area." The burner operates inside a vacuum chamber, with the oxygen needed for combustion provided as part of the gas mixture. The gas feeds the burner at about five percent of atmospheric pressure, creating a very diffuse vapor over the burner that produces an even, cool-burning flame. Coolness, of course, is relative. A welder's torch burns hotter than 3000° C. The "cool" flame burns at about 1600° C, heating the substrate to an almost-balmy 700° C—still a bit hot for some potential substrates, such as plastics, but well within silicon's comfort zone. Preliminary trials with the new burner have grown what appear to be diamond particles in isolated deposits on the substrate. Tests are still underway to confirm the deposits' identity.

Several people have hit upon the sintered-plate burner idea independently, Goodwin notes, but he adds that the Caltech contingent's comprehensive theoretical models should give them a leg up in learning how the process works. Goodwin and company scrutinize the goings-on inside their diamond-growth chamber with the sort of intense scrutiny normally associated with IRS audits, or a supermarket tabloid's coverage of Elizabeth Taylor. An assortment of instruments identifies and measures the concentrations of the dozen or so important chemical species present in various parts of the flame and on the surface. Says Goodwin, "Our model allows us to predict everything we could want to measure. We can diagnose how the flame is burning, using laser techniques to measure the distribution of important radicals in the flame, and sampling probes to look at stable species. We can predict how changing the fuel mixture should change the flame—and thus the diamond's growth—and then we can measure what actually happens. Our model enables us to understand other peoples' experiments as well as our own, and will help us seek out the optimal conditions for diamond growth." □—*DS*

Random Walk

The third annual Egg Drop Contest, staged by Assistant Professor of Mechanical Engineering Joel Burdick's ME 71 class, drew more than 100 onlookers to the parking lot behind Thomas Lab to watch 66 raw eggs plunge 40 feet to the pavement. Each egg was protected by one of two basic designs: the "standard" package padded the egg with such things as popcorn, toilet paper, and a 19-pound watermelon; while the "bare-egg" package used wings, parachutes, and the like to ease the egg to earth. The final score was eggs 23, parking lot 43.

Honors and Awards

John Allman, the Hixon Professor of Psychobiology and professor of biology, has received the Golden Brain Award from the Minerva Foundation for pioneering research into how the brain processes and interprets visual information.

Don Anderson, the McMillan Professor of Geophysics and director of the Seismo Lab from 1967 to 1989, has been selected the 1991 recipient of the Bowie Medal, the highest honor of the American Geophysical Union, in recognition of his "accomplishments over a distinguished career in geophysics."

Seymour Benzer, the Boswell Professor of Neuroscience, has won the 1991 Wolf Prize, presented by the Israel-based Wolf Foundation. Benzer was selected for the $100,000 prize, one of the most prestigious in international science, for "having generated a new field of molecular neurogenetics by his pioneering research on the dissection of the nervous system and the behavior of gene mutations."

Edward Lewis, the Thomas Hunt Morgan Professor of Biology, Emeritus, and John Roberts, Institute Professor of Chemistry, Emeritus, received the National Medal of Science from President Bush last November. The medals are awarded to honor the impact that an individual's career has had on the present state of scientific knowledge; for outstanding achievements that change the direction of scientific thought; and for distinguished service in the advancement of science. In the same White House ceremony Caltech Trustee Gordon Moore (PhD '54), chairman of Intel Corporation, received the National Medal of Technology. Lewis has also been named corecipient of Brandeis University's 1990 Rosentiel Award for research that has "provided mankind with its first glimpses into the process through which organisms, including humans, assemble and correctly position body parts in the growing embryo."

Masakazu (Mark) Konishi, the Bing Professor of Behavioral Biology, has been awarded Japan's 1990 International Award for Biology, established in memory of the late emperor of Japan because of his special interest in biology.

Shrinivas Kulkarni, associate professor of astronomy, has received the 1991 Helen B. Warner Prize for Astronomy from the American Astronomical Society (AAS) in recognition of his work on millisecond pulsars and on developments in the theory of optical and radio interferometry. The AAS also presented its 1991 Newton Lacy Pierce Prize to Kenneth Libbrecht, associate professor of astrophysics, for his research on helioseismology—"observations of the sun [that] have provided essential new insights into its internal properties." And the AAS gave its 1991 Dannie Heineman Prize for Astrophysics—a certificate and $10,000—to Wallace Sargent, the Bowen Professor of Astronomy, in recognition of his pioneering research into the properties and composition of galaxies and the intergalactic medium.

Rudolph Marcus, the Noyes Professor of Chemistry, has been selected to receive the 1990 William Lloyd Evans Award from Ohio State University.

Andrew Myers, assistant professor of chemistry, has been named one of 12 recipients nationwide of a Camille and Henry Dreyfus Teacher-Scholar Award. The award, whose purpose is to promote the development of exceptionally promising young scholars "who combine interest and demonstrated ability in teaching with performing imaginative research," provides $50,000 in support of those activities.

Allan Sandage, staff astronomer with The Observatories of the Carnegie Institution of Washington and a longtime Caltech collaborator, has been awarded the 1991 Crafoord Prize in Astronomy, presented by the Royal Swedish Academy of Sciences in recognition of his fundamental contributions to "extragalactic astronomy, including observational cosmology." The prize, which carries an award of $260,000, honors outstanding contributions in fields not recognized by the Nobel Prize.

Mel Simon, the Biaggini Professor of Biological Sciences, has received the Selman A. Wakesman Award in Microbiology, administered by the National Academy of Sciences.

Ahmed Zewail, the Linus Pauling Professor of Chemical Physics, has been selected by the Egyptian American Organization as the recipient of their 1990 Outstanding Achievement Award.

Undergrad Confab

More than 860 college students and 500 faculty and administrators from throughout the U.S. will attend the Fifth Annual National Conference on Undergraduate Research, which Caltech is hosting March 21–23 as part of its centennial celebration. This year's conference is called EUREKA—Excellence in Undergraduate Research: Experience, Knowledge, and Achievement—any resemblance to Archimedes's exultation upon discovering the principle of specific gravity is purely intentional.

The opening plenary session will have four speakers: Lee Hood, Bowles Professor of Biology and Director of the Center for Molecular Biotechnology; Evelyn Fox Keller, director of women's studies and professor of rhetoric at UC Berkeley; writer Ray Bradbury; and Louis Sullivan, U.S. Secretary of Health and Human Services. The undergrads will then present their research papers in five sessions. The final plenary session will be a panel discussion on global warming, moderated by Robert Cowen, natural-science editor of *The Christian Science Monitor.*

"It appears that student attendance will be 100 or so more than last year's conference," says Carolyn Merkel, chair of EUREKA's planning committee, "and there's a significant increase in the number of minority students. The National Science Foundation and JPL's Minority Science and Engineering Initiatives Office provided funds to help minority students attend. The response has been overwhelming."

Arthur Amos Noyes first came to Throop College of Technology (later Caltech) part time in 1913 and was finally hired away from MIT permanently in 1919. From 1928 until his death in 1936 he was chairman of the Division of Chemistry and Chemical Engineering. During this time he instituted a weekly research conference in room 27 Gates, which had a small kitchen adjoining it where grad students labored to prepare the seminar refreshments. Noyes left nothing to chance; his laboratory procedure for purchasing, cooking, announcing, and cleaning up (with his own emendations) was posted on the bulletin board. Coauthors appear to have added notes at the bottom. This facsimile was reproduced for the program of the 1968 dedication of Noyes Laboratory.

[illegible]

Bond paste on board yet

Boards over the sinks and tray

3.45 Put coffee urn on board above left-hand sink.
Fill outer jacket of ~~coffee~~ urn with hottest water that can be drawn from tap, and put burner (full heat) under the urn.

3.50 Get quart of whipping cream from organic lab. ice-chest, and whip it in machine.

RESEARCH CONFERENCE REFRESHMENTS.

4.00 Pour 3 quarts of milk into urn, and let it heat 10 min.)
Distribute cakes equally into four platters.

~~12.30~~ ~~Buy 3 qts. of milk at Safeway's on Lake Avenue, and buy enough cakes~~ for

~~Saturday 1.15 P.M.~~ ~~cookies for 36 people, costing not more than [illegible].~~ Return bottles from previous week. ~~If marshmallows are all gone, buy a pound.~~

2.00 Put 6 qts. of water into 8-qt. pot, and heat it to boiling.

2.10 Add 1 1/3 qts. of cocoa beans, cover the pot, and turn down gas so water boils only very gently (~~with cover on~~) for 2 hours.

4.10 Pour off cocoa through strainer into coffee urn. Add ~~second 8-qt. pot, put it on stove,~~ ~~add~~ 2 cups of sugar, and stir thoroly with large spoon. ~~bring it to a boil.~~

4.12 Take 54 cups one by one from racks, fill with cocoa from urn, place in saucers (not cups) on benches beside sinks. ~~Rinse out first pot, pour into it the 3 qts. of milk, and heat it to 60-70°C (not to boiling).~~ Set out ~~cups and~~ saucers ~~on drain board, putting~~

4.25 Put tablespoonful of cream ~~1 marshmallow~~ in each cup. Put spoons and sugar bowls on tables.

4.30 ~~4.20~~ Put 2 platters of cakes ~~or cookies around~~ on each table in seminar room.
Announce cocoa is ready. ~~Take cup of cocoa up to Mrs. Merrill~~

~~4.25 Mix contents of two pots thoroughly by pouring back and forth.~~

4.35. Run out of the urn into milk bottles any remaining cocoa. Fill the urn completely with hot water, add a tablespoonfull of soap powder, stir, and let stand (till 6.00 P.M.)

~~4.30 Fill cups with cocoa.~~
~~Take cup to Mrs. Merrill~~
~~Announce that cocoa is ready for second helpings.~~

~~As soon as pots are empty,~~ Rinse the coffee pot with hot water.

Rinse out the urn, and remove it

6.00 Fill both sinks with ~~hot~~ hot water. To one sink add 1/2 cup of soap powder. ~~Put cups~~ Put cups and ~~[illegible]~~ ~~and~~ saucers into racks. Souse the racks down and up in soapy water a number of times, then rinse in the clear water, and leave dishes in racks on the bench ~~on drain board~~ to drain and dry.

4 qts milk
2 1/2 cups sugar
8 quarts H2O
2 qts cocoa beans
4 1/2 lbs assorted cookies (small)

1 1/4 qt of coffee to 9 gals H2O

Lemonade - 2 qts lemon juice - 5 doz lemons
3 cups sugar
[illegible] to 14 qts

1:15 Start Heating
1:45 Dump in Cocoa
& allow to simmer until 3:00
Put 2 quarts milk in other kettle &
heat to 70°, Put in [illegible]
Pour cocoa in the [illegible]

Engineering & Science

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