California Institute
of Technology

Engineering & Science

Winter 1994

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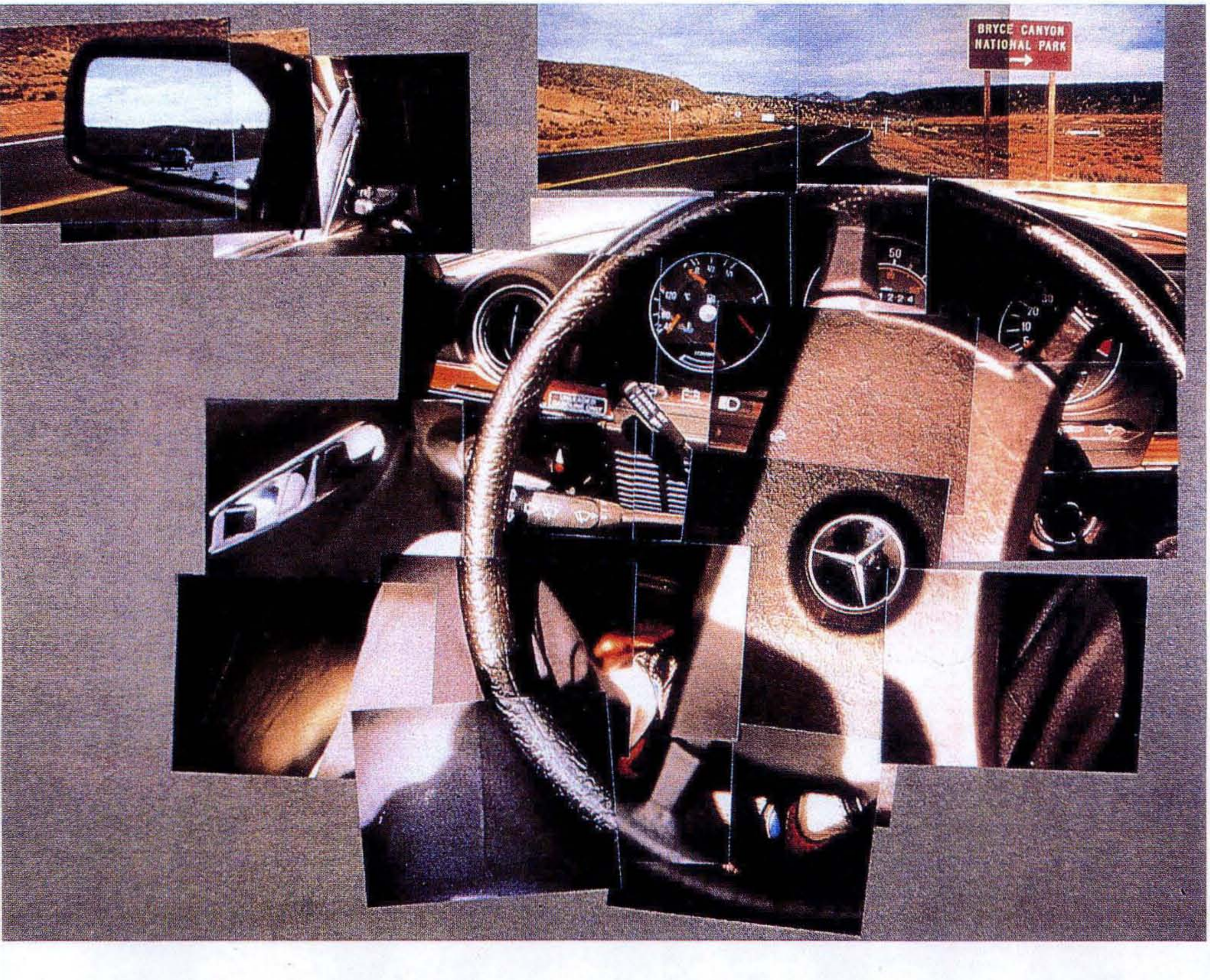
Bonnie Cashin, whose gift established the James Michelin Distinguished Visitor lecture series, talks at a post-lecture dinner with this year's Distinguished Visitor, artist David Hockney. Below: Hockney's photographic collage, *Steering Wheel, Oct. 1982.*



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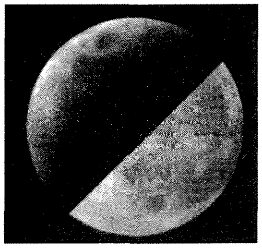
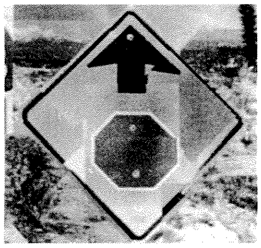
Volume LVII,
Number 2**





On the cover: A gibbous earth shines in the moon's night sky. The lunar disk reflects back some of this "earthshine," providing clues to global change, as Steve Koonin explains in an article beginning on page 2. Although this view from the moon is not the sort of reverse perspective that David Hockney refers to in *his* article (which begins on page 22), it does illustrate one of his points—the vanished veracity of the photograph. This scene was created by computer in Caltech's Media Integration Laboratory.

Departments

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

PICTURE CREDITS: Cover, 3, 4, 6, 10 – Wayne Waller; inside front cover, 22–30 – David Hockney; 15 – Chris Tschoegl; 16 – AP; 17 – Pasadena Star-News; 17, 21, inside back cover – James McClanahan; 18 – Tom Harvey; 19 – Floyd Clark; 20 – TIME, Gordon Glattenberg; 38–40 – Tom Palfrey et al; 39 – Cathy Hill; 43 – Kenton MacDavid; 44 – Doug Smith

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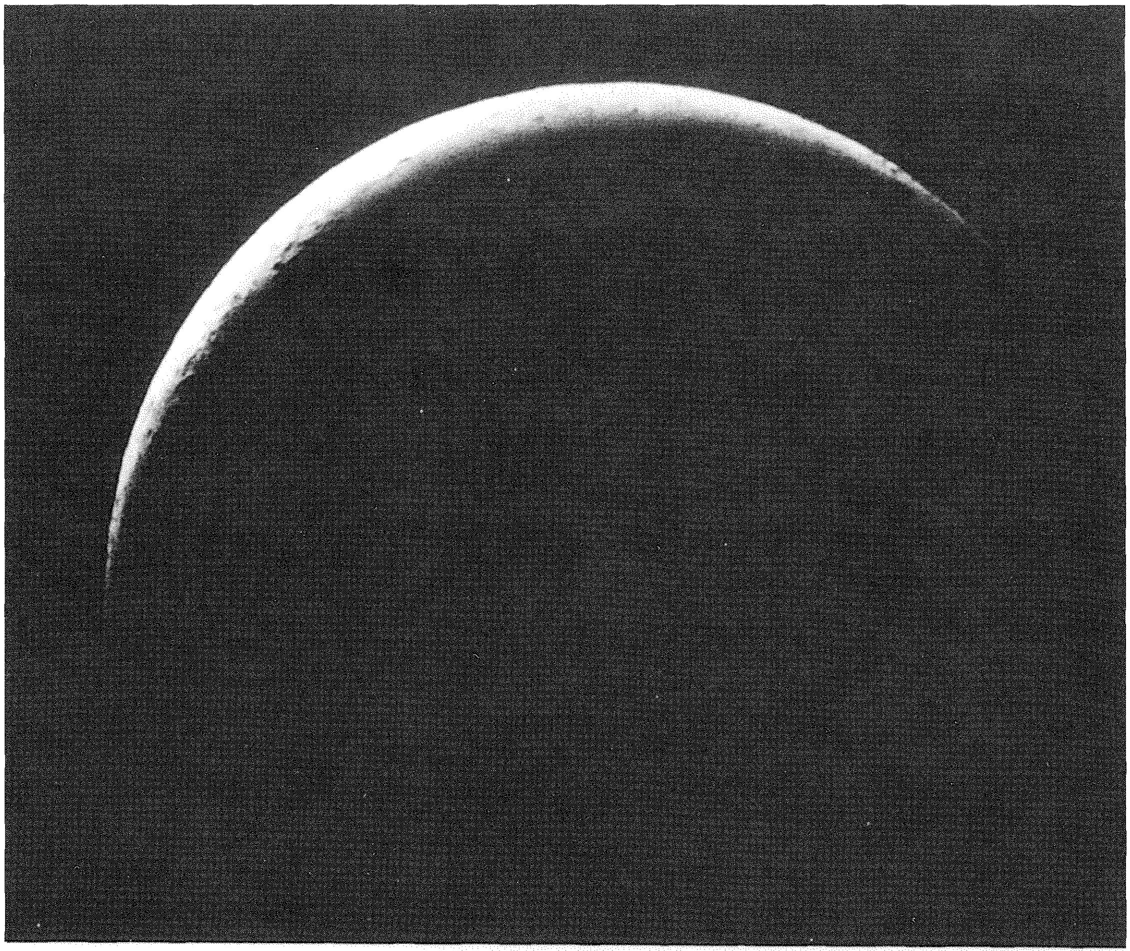
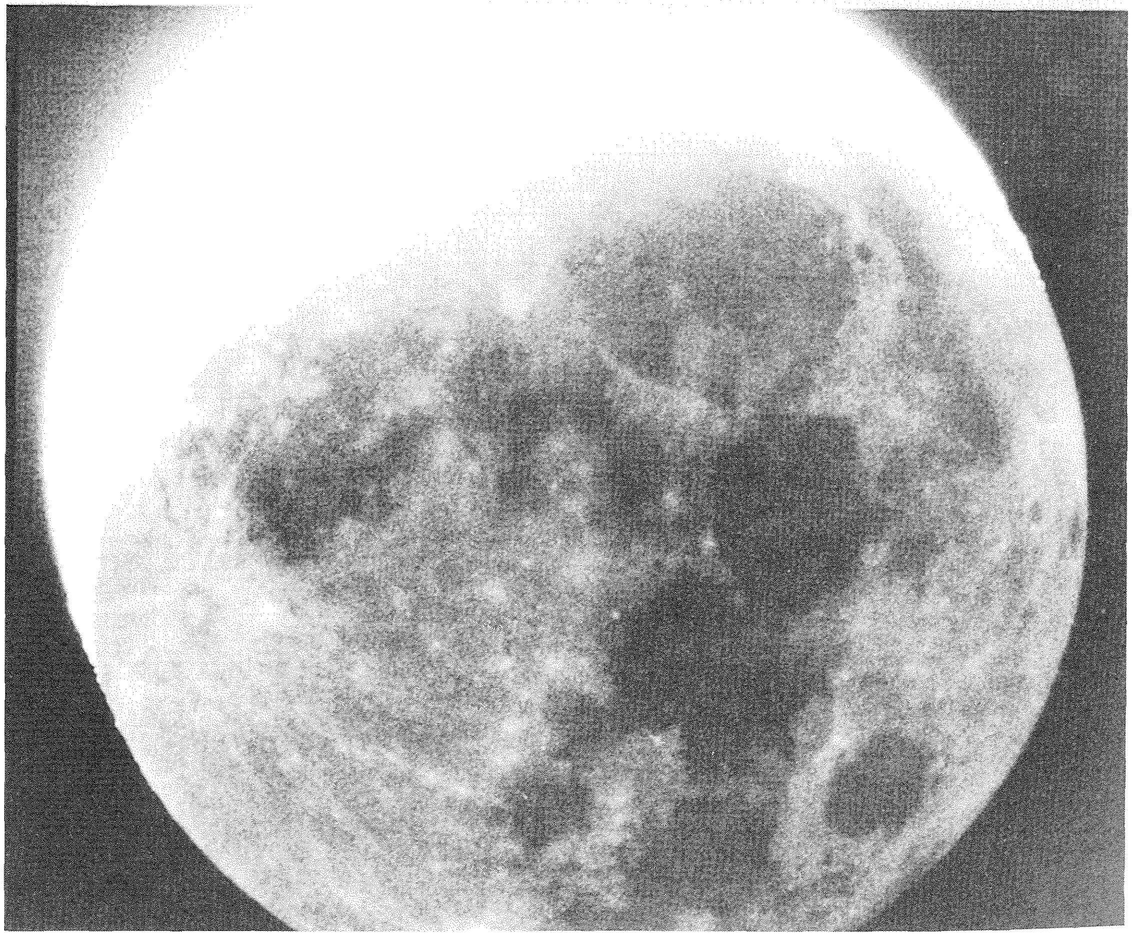




Global Change and the Dark of the Moon

by Steven E. Koonin

Careful study of this "earthshine" can reveal much about the earth.

The lunar crescent, snapped at 1/30 second (top), within a few days of a new moon, is all we usually can see of the moon at this stage. Sometimes, though, the rest of the lunar disk is faintly visible to the naked eye, and with an exposure of 30 seconds, even the features in the dark of the moon can be seen in a photograph.

This story is about watching the moon. Of course, people have been doing that for as long as there have been people, and so you might think that there's nothing new to say about moon watching. But a group of us at Caltech have recently revived and improved a 65-year-old program of precise lunar observations that can tell us something about the earth and offer unique insights into the changing global climate.

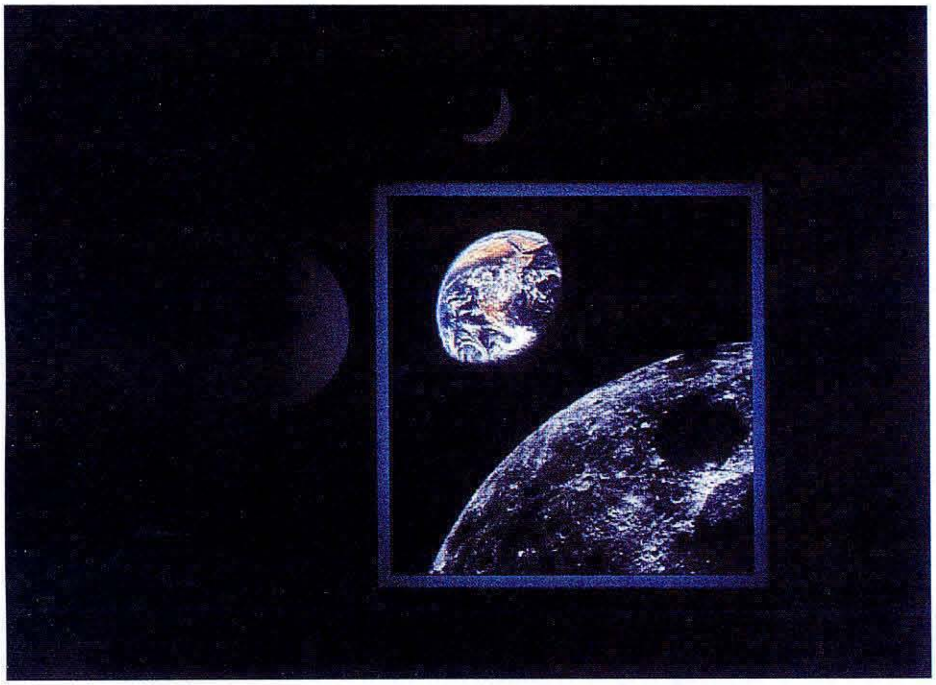
The phenomenon is best understood by considering the lunar image opposite, taken within several days of a new moon. The 1/30-second exposure shows the expected thin crescent. But if the exposure is lengthened to 30 seconds, features on the dark part of the lunar disk become visible. (The crescent, of course, becomes overexposed.) This ghostly glow of the dark of the moon is often visible to the naked eye and was known to the ancients. Various explanations for this light were offered over the centuries, including phosphorescence or translucence of the lunar surface; the great astronomer Tycho Brahe thought that it was light from the planet Venus. However, the correct explanation is generally attributed to Leonardo da Vinci in the 15th century: it is light reflected by the earth.

Careful study of this "earthshine" can reveal much about the earth. But for you to understand how earthshine relates to global change, I'll first need to say something about climate. The first thing to appreciate about the earth's climate system is how complex it is. Of course, there's the atmosphere, whose temperature, humidity, and winds are probably the most familiar manifestation of climate. But the oceans are at least equally important, because they store and transport vast amounts of heat, water, and other chemicals. Snow and ice in the mountains and the polar regions also play a role, as do biological systems both on land and in the ocean. The many interactions among these components result in the delicate balance that determines our climate.

Another important aspect of climate is its great variability. Every few days there are changes due to the weather. Changes over several months are associated with the seasons, and you need only recall that our rainy winter last year ended almost a decade of drought to realize that there is considerable variability from one year to the next. Most, but not all, scientists believe that the rising levels of carbon dioxide in the atmosphere will cause a greenhouse warming over the next several decades, and there is ample proof that the climate has varied substantially on even longer time scales. To go along with this variability in time, there's also great variability with location. Composite satellite images of the cloud cover over the whole globe show that two places with similar climates might have very different weathers on any given day.

So, the answer to the question of whether the climate is changing is "Of course!" But the real questions are: "How is it changing? How fast is it changing? And why is it changing?"

Well, if the climate is changing, what, if anything, should we do about it? The question is sharpened by the realization that human activities can affect the climate system. Examples include the burning of fossil fuels, the destruction of



From the moon, the earth appears to go through phases, just as the moon does to an observer standing on the earth. But these phases are opposite one another. When we see a crescent moon (background), an inhabitant of the moon gazing up at the sky would see a gibbous earth (inset).

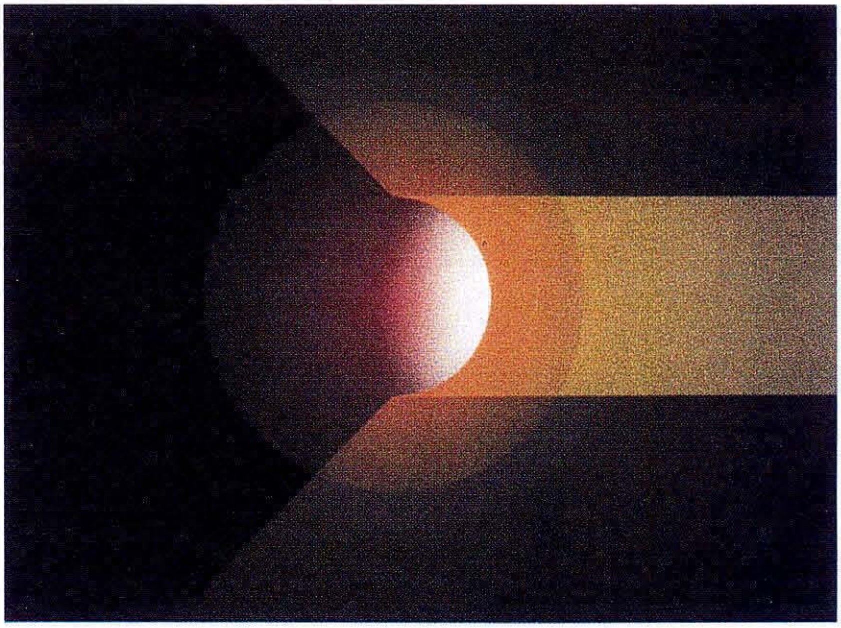
forests, and the use of chlorinated fluorocarbons (CFCs). Many, including the present administration in Washington, advocate far-reaching changes in society, aimed at reducing these activities or diminishing their impact, or, at the very least, adapting to climate change. Such measures, however, must rely on a firm scientific understanding of the climate system, including a clear separation of those changes that are natural from those that are caused by humans, and an assessment of how the system responds when we change something. This scientific understanding rests, in turn, on three kinds of activities.

The first are local observations of the various processes that connect the components of the climate system: for example, the way that plants remove carbon dioxide from the atmosphere, or the way that winds generate ocean currents. The second are computer models that use basic physical laws to describe and predict how the climate system behaves. This task, which is carried out in part using the Intel Touchstone Delta parallel supercomputer here at Caltech, is among the most challenging problems in computational science.

Finally, there are observations to find out what the climate is actually doing. To average out the great variability that I mentioned earlier, these must cover large areas of the globe and extend over many years. Further, they must be very precise, because any significant changes will likely be signaled by subtle shifts in the climate variables. Only when scientific understanding is firmly in hand can we confidently make changes in society and endure the great disruption they will undoubtedly entail. In my opinion, we have reached that point in some cases (for example, in the destruction of stratospheric ozone by CFCs), but are far from that point on many other issues. There is an urgent need for further understanding. The global observations I'm discussing here are one example of the attempt to meet this need.

Watching the planet as a whole is more easily imagined than accomplished, because the earth is large and we are simply too close. To appreciate the problem and its solution, consider how I might watch changes in my own face. The high-tech way would be to set up a video camera, step back, and then watch myself in a monitor. This is analogous to how we've been using artificial satellites for the past 30 years to watch the changing earth. A much easier way, however, is to just look in a mirror. And that brings me back to the moon.

We all learn in school that the moon goes around the earth once every 28 days. The familiar phases of the moon, from new to first quarter to full to last quarter, are caused by the varying visibility of the sunlit side of the moon. Sunlight, however, also illuminates the earth. Most of that sunlight is absorbed by the earth and drives its climate system, but some of it is reflected back into space. In particular, some of the reflected light reaches the moon. If you were standing on the moon and watching the earth, you would see it going through phases opposite to those of the moon: When the moon (as seen from earth) was new, the earth would be full (and you could see half the globe at once); when the



When light from the sun (way out of the picture to the right) reaches the earth, about 70 percent of it is absorbed, warming the planet. Eventually this heat returns to space as infrared radiation from all parts of the globe (the orange glow in this schematic drawing). The other approximately 30 percent (the albedo) is reflected from the sunlit half of the globe. Variations in the albedo change the portion of the sunlight available to warm the earth, and so can provide insights into global change.

It's been a long time since anyone has been on the moon to watch the earthshine, but we can use the moon as an imperfect mirror and see the earthshine reflected back to us on earth as the faint glow of the dark part of the lunar disk.

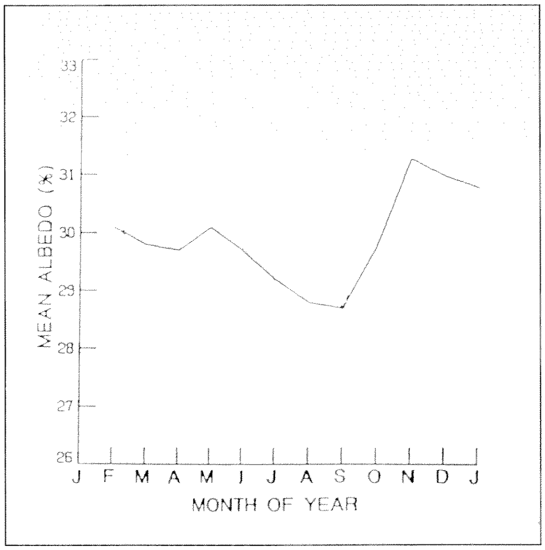
moon was full, the earth would appear as a thin crescent. It's been a long time since anyone has been on the moon to watch the earthshine, but we can use the moon as an imperfect mirror and see the earthshine reflected back to us on earth as the faint glow of the dark part of the lunar disk. The earthshine will be bright and easy to observe near new moon, while it will be dim near the full moon.

To understand how earthshine can be used to monitor the earth's climate, we need to look at what happens when the sun shines on the earth. About 70 percent of the light is absorbed and converted to heat; it is this energy that warms the planet and drives the winds and ocean currents. The heat eventually returns to space as infrared radiation emitted from all parts of the earth. But the 30 percent of the sunlight reflected back into space from the sunlit half of the globe is not available to warm the planet. So the fraction of the sunlight reflected (called the albedo) determines the temperature of the earth. Other things being equal, if the albedo were 29 percent, the earth would be about 2 degrees Fahrenheit warmer, and if it were 31 percent, the earth would be 2 degrees cooler. Since the total greenhouse warming due to doubling the carbon dioxide in the atmosphere is predicted to be between 2.5 and 7 degrees, it's clear that knowing the albedo to a small fraction of a percent would be important.

Of course, it's not that simple. For example, if the albedo were smaller, the earth would be warmer and there might be more water vapor in the atmosphere, leading to more clouds, which would tend to make the albedo larger. Such "feedbacks," where a change in one element causes a second to change, which in turn affects the first element, are a central feature of the climate system and greatly complicate our attempts to understand it. In any event, the albedo is a key parameter of the climate, and the global temperature is very sensitive to it.

The albedo of the entire globe (the fraction of sunlight reflected) depends upon the reflectivity of each part of the earth's surface. The ocean is generally darker than the land, the desert is more reflective than the average land, and snow and ice are the most reflective of all. The presence of clouds can enhance the albedo greatly, and if there were no clouds on the earth, its albedo would be only 17 percent, much less than its true value of 30 percent. The global albedo is therefore highly variable, changing with the weather and the seasons. Since the oceans and continents are not uniformly spread over the earth, it even depends upon which half of the earth is in the sunshine. Volcanic eruptions, such as the recent one from Mt. Pinatubo, also increase the albedo by as much as 0.5 percent, since the dust clouds they loft into the stratosphere linger for several years.

Observations from satellites in orbits several hundred miles high have told us most of what we know about the global albedo. They do this by observing the sunlight reflected from five-mile patches of the earth, one patch at a time. After enough orbits, and with certain assumptions about how the sunlight scatters from the earth, they can deduce the total amount of sunlight



Satellite data produced this monthly plot of the global albedo in 1985, which averages about 30 percent. The variation of about 1 percent is due to the distribution of continents and the changing snow and cloud cover.
Below: French astronomer André Danjon, who performed the first, pre-satellite, quantitative measurements of the earthshine.

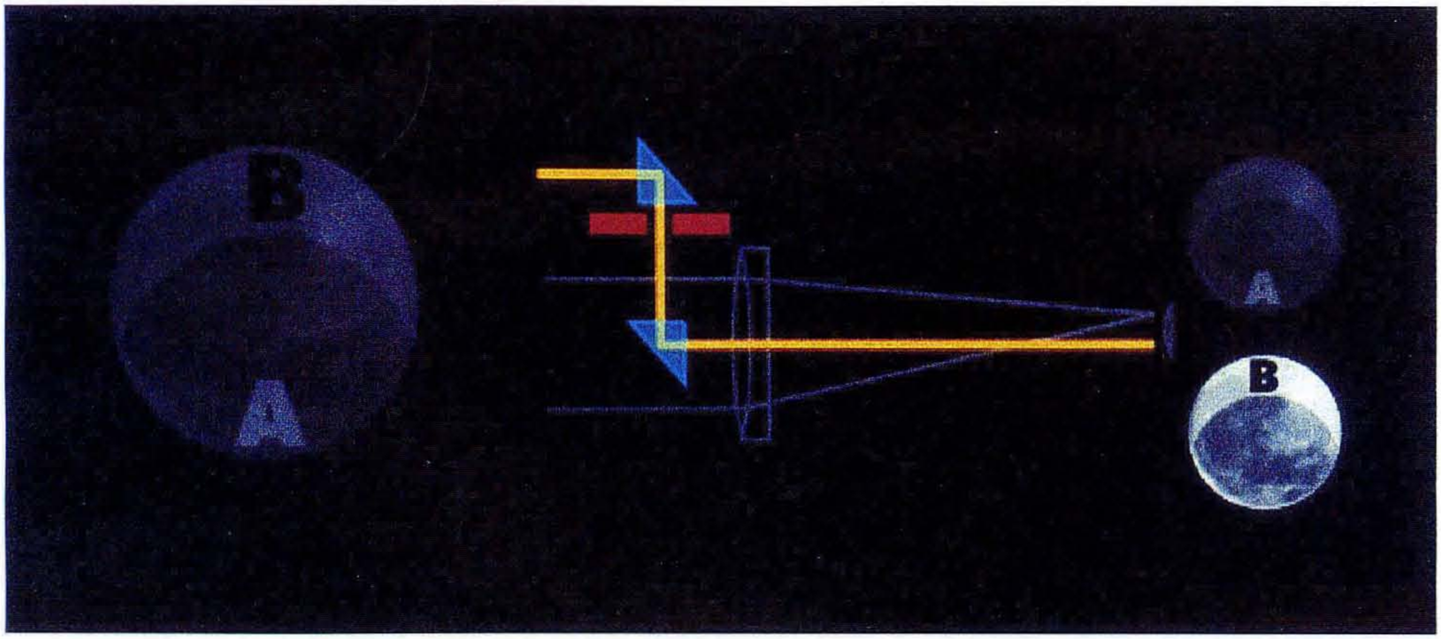
Because earthshine observations are ground-based, they are relatively inexpensive, and, compared to a satellite, the equipment is easy to maintain and upgrade.

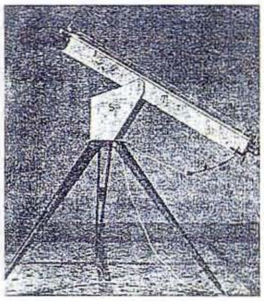
reflected. Above is a plot of the global albedo for each month of 1985 as determined by satellite. You'll notice that it averages about 30 percent, but varies up and down by about 1 percent during the year, with a minimum in August/September and a maximum in November. This variation is due to the way in which the continents are distributed on the earth (there is much more land in the northern hemisphere) and to the way in which the snow cover changes during the year. Of course, if we were to look at the results for 1986, the details would be different, but the overall features that I just mentioned would be similar.

Satellites are marvelous devices, and many people work very hard to produce and analyze the wealth of data they provide. But satellites are not perfect. They cannot cover large areas of the globe simultaneously (just patch by patch), nor can they do it continuously; any one satellite sees a given point on the earth only infrequently. Further, there are uncertainties in how to relate the light that the satellite detects to what is actually scattered from a given patch of the earth. It's also not so easy to keep a precision instrument calibrated in space. As a result, two different satellite systems will typically differ by 0.7 percent in the monthly average albedo. That may not sound like much, but it's worth about 1.5 degrees in the global temperature, a non-negligible fraction of the expected greenhouse warming. Satellites are also expensive—typically costing hundreds of millions of dollars—and they can break; at this moment there is no satellite doing precision monitoring of the earth's albedo. And finally, we have good satellite data for only two or three decades at best.

Earthshine observations can complement the satellite observations in interesting ways. By observing close to the new moon, you can cover almost half the globe at once, and with sites spaced around the globe, this could be done during more than half of each lunar month. Earthshine observations are self-calibrating, as I'll explain shortly, and should be able to detect changes in the albedo as small as 0.2 percent. Because earthshine observations are ground-based, they are relatively inexpensive, and, compared to a satellite, the equipment is easy to maintain and upgrade.

Another advantage is the existence of a historical record of observations extending back over 65 years, which should give us a long interval over which to assess climate change. André Danjon, a French astronomer, devised the method that allows a quantitative measurement of the earthshine. Danjon was a major figure in French astronomy, finishing his career as director of the Paris Observatory before his death in 1967. He was noted for inventing several types of astronomical instruments and for his precise photometry (studies of variations in brightness) of the planets and stars. Danjon's method for measuring the earthshine is quite elegant. He selected two spots nearly diametrically opposite on the lunar disk. They were chosen to have similar optical properties (both are bright highland regions) and are quite clearly described in his papers. We can call one of these spots region A and the other region B. At some time early in





Danjon's photometer (left) employed an elegant system for determining the brightness of the dark of the moon relative to its sunlit crescent. Two spots, A and B, were selected on opposite sides of the lunar disk; at any point in the lunar month, one spot would be bright in sunlight and the other dimly illuminated by earthshine. The photometer's eyepiece showed two adjacent lunar images, one of which had passed through a simple telescopic lens, and the other through a series of prisms (blue) and a diaphragm (red). This diaphragm, shaped like a cat's eye, could be adjusted to reduce the brightness until both spots looked the same. The amount of adjustment provided an accurate measure of the relative brightness of the two spots.

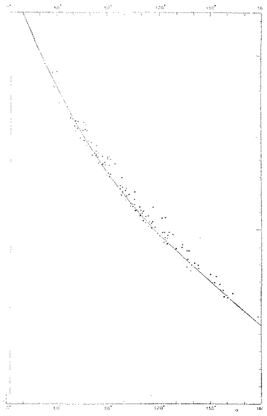
the lunar month, one of these two spots (say, A) will be in the earthshine, and the other, B, will be in the sunshine. Of course, later in the month the role of the two spots will be reversed.

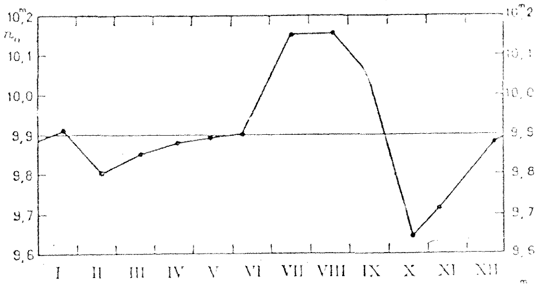
Danjon developed a "cat's-eye" photometer to make his measurements. The device presented two adjacent images of the moon in the eyepiece. One of these passed through an ordinary telescope arrangement. The other image was produced similarly, but first passed through some prisms and a diaphragm shaped like a cat's eye, which could be adjusted to reduce the brightness of the image. In the eyepiece spot A of the first image would appear next to spot B of the second image. If Danjon then adjusted the diaphragm until the two spots appeared to be equally bright, he could determine the brightness of the earthshine relative to the light from the bright crescent. This gives the technique the advantage of self-calibration; that is, the earthshine is compared to a "standard candle"—the sunlit part of the moon.

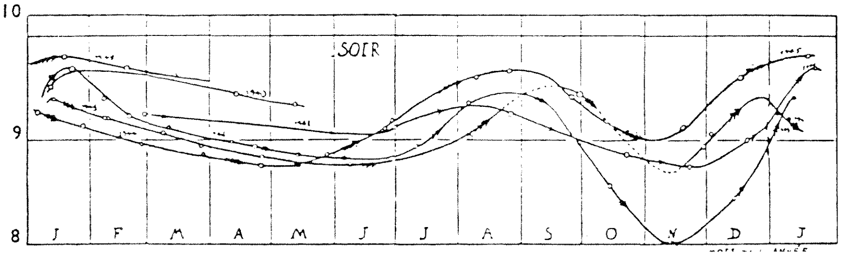
It also neatly solves another problem. Precision astronomical observations from the ground generally suffer from the fact that the light has to pass through the atmosphere where it can be absorbed or distorted, as in the familiar twinkling of the stars. But, since the light from both spots takes almost the same path through the atmosphere, both images suffer the same distortion, which then doesn't matter when they're compared. Danjon estimated that, with his trained eye, he could determine the ratio of the light from the two spots to an accuracy of 5 percent.

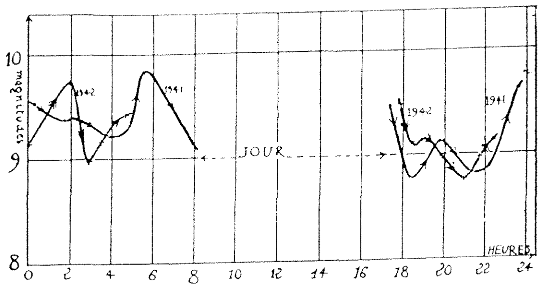
Danjon's photometer was packaged in a portable device, easily set up and operated on a

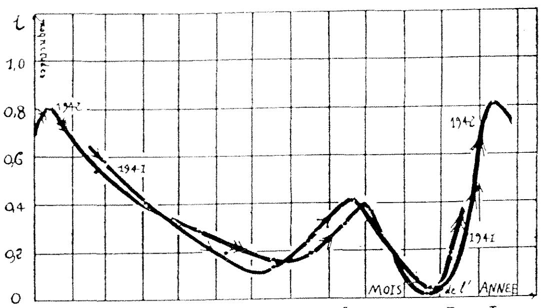
Dubois and Danjon observed and plotted earthshine variations, beginning in 1927 and continuing until 1960, showing (from top) how it changed with lunar phase (brightest near new moon), with season (in 1929; Roman numerals represent months; numbers along vertical axis refer to astronomical magnitudes), with season again in the 1940s, with time of day (hours along bottom), and with color by month (the vertical axis is the color index in astronomical magnitudes).









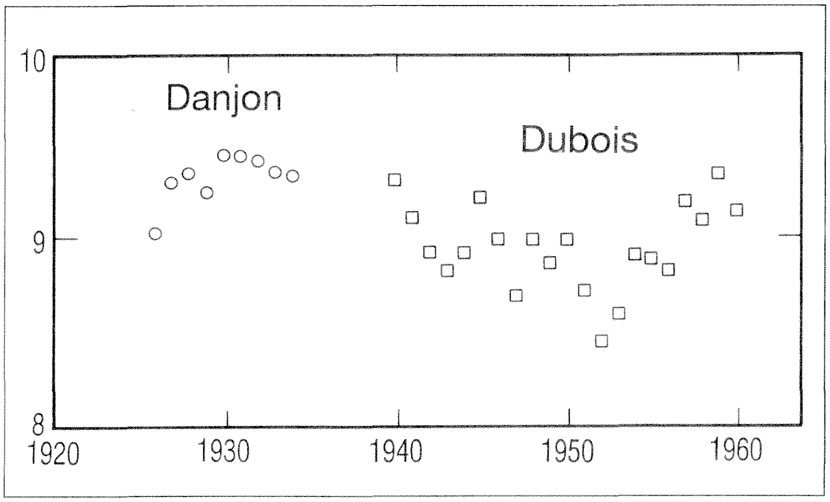


The old French observations qualitatively established many important features of the earthshine.

tripod. He used it to make some 200 earthshine observations in southern France from 1927 to 1931. His program was pursued and extended by J. E. Dubois, who made many observations from 1940 to 1960. One might imagine that Dubois took a considerable risk running around the countryside of occupied Bordeaux pointing this funny-looking contraption at the night sky. But, in fact, he claimed that the wartime blackouts actually made his work easier by darkening the sky.

The old French observations qualitatively established many important features of the earthshine. They mapped out how the phenomenon varies with the lunar phase (as I mentioned earlier, it's brightest near new moon and dimmest near full moon), and showed that it depended upon the weather. For example, one of the papers remarks that the earthshine was particularly bright one night due to the clouds of a large storm in the North Atlantic. They were also able to map out how the earthshine varied with season, which is quite similar to the variation in the albedo seen in satellite measurements. (Remember, this was done about 25 years before the first satellite was launched!) They also showed that the earthshine varies from one year to the next and even showed that it varies with time of day, as different parts of the earth reflect sunlight up to the moon. Finally, they studied the color of the earthshine and found that it's relatively blue, as might be expected of the light from our "blue planet." They also found that the color varies with the season and with the time of day.

Many of the details of the old French measure-



Don Huffman's analysis of the Danjon/Dubois data shows large fluctuations, particularly over the latter two decades. This phenomenon is not yet understood.

One might imagine that Dubois took a considerable risk running around the countryside of occupied Bordeaux pointing this funny-looking contraption at the night sky.

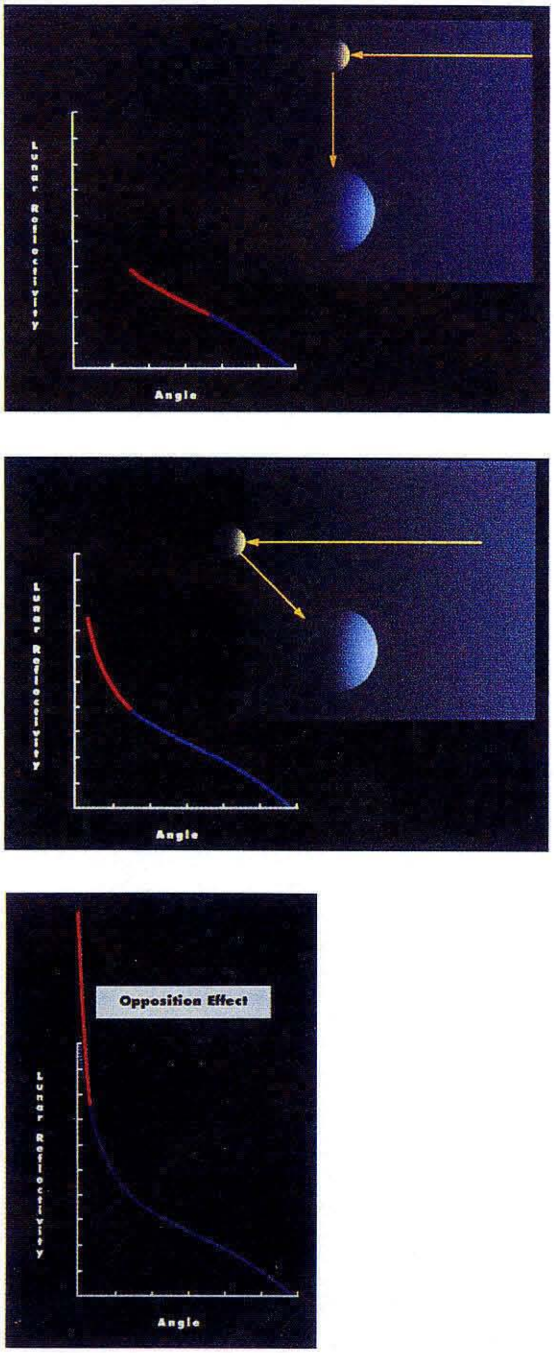
ments have been recovered by Don Huffman, professor of physics at the University of Arizona. Huffman has achieved great notoriety in recent years in figuring out how to make large quantities of "Buckyballs," a new family of large, all-carbon molecules. But earthshine is sort of a hobby for him. He has been to France several times since 1989 to interview Dubois's widow, examine the old notebooks, and inspect the instrument that Dubois used. Huffman's analysis of the Danjon/Dubois record shows large fluctuations over two decades, a phenomenon not yet understood. Don has also reproduced the old French instrument and trained himself in the observing techniques; he's making regular earthshine measurements from the Arizona desert to extend and understand this 65-year time series. This still leaves a gap of some 30 years; we would love to find someone who was making earthshine observations during that period.

The French observations stopped in 1960 for several reasons. Professional astronomers were getting bored with the moon and moved on to more glamorous objects such as quasars. In fact, Danjon closes one of his papers with a charming apology for spending his time on earthshine, saying that, while it might not be the most fashionable astronomy, it probably has something to do with the earth's climate. Moreover, Danjon determined the average albedo to be 36 percent. Because this is much greater than the 30 percent later measured by satellites, his work has been dismissed with some vague mention of problems with the observations or method.

That discrepancy persisted until 1991, when my part of the story starts. During that summer I was a member of a government committee looking into what use a fleet of small satellites might have in studying global change. One of the suggestions was to study the albedo. At some point in the discussion, someone noted that the way the earth's albedo was first determined was through observations of earthshine. A fellow committee member (Gordon MacDonald of UC San Diego) and I thought that sounded pretty interesting. So we set off searching libraries all across the United States and Europe, using phones, fax, and the Internet. We soon dug up the old French references and learned the historical details I've just recounted. But we also realized that the old analyses had not accounted for a peculiar property of the lunar reflectivity called the opposition effect.

The point is that the sunlight is reflected from the bright lunar crescent by an angle that changes with the lunar phase. The earthshine, however, is always reflected straight back from the moon in the direction of the earth. So, to compare the intensity of the earthshine to the sunshine, you need to know how the quality of the lunar mirror (its reflectivity) changes with the angle. Danjon measured this by studying how the brightness of his two spots varied during the lunar month, when they were in the sunshine. His data trace out a smooth variation with angle, as shown by the blue curve in the middle figure on the following page. Unfortunately, Danjon measured the lunar reflectivity only to within 11 degrees of the full moon, in part because the 6-degree tilt of the moon's orbit prevented him

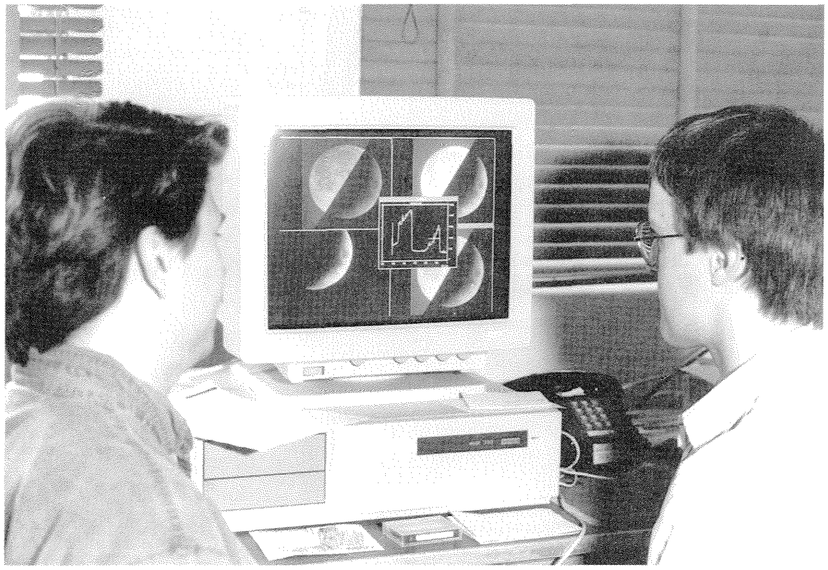
Since I'm a theorist, my first urge was to rush to a computer and start modeling. And, being a professor, I couldn't resist the opportunity to educate someone else.



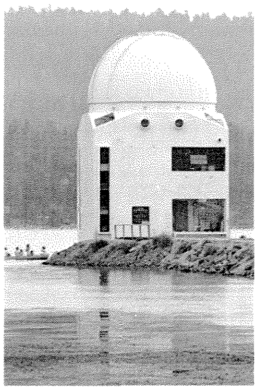
A phenomenon called the opposition effect complicates the comparison of the intensity of sunshine with earthshine. It refers to the fact that, while the sunlight reflects through an angle that varies with the lunar phase, the earthshine is always reflected straight back to the earth. The reflectivity is least just after new moon (top). Danjon expected a smooth curve upward as the angle of reflected sunlight decreased (center), but failed to account for the opposition peak near full moon, which causes the curve to rise dramatically near the axis (bottom). Modern corrections of Danjon's underestimate of the reflectivity of the moon bring his figures for average albedo into line with current observations.

from seeing an exactly full moon. But he made what seems like a quite reasonable estimate of the reflectivity for earthshine. More modern measurements of the variation of the moon's brightness near lunar eclipses (when the sunlight is reflected almost exactly straight back) show that the lunar reflectivity rises dramatically (the opposition peak) within a few degrees of back-reflection, as shown by the red curve in the bottom figure. As a result, the moon is a much better mirror for earthshine than Danjon had estimated. (It is this same opposition effect that makes the full moon much brighter than one would have guessed.) When the correct lunar reflectivity is used, Danjon's average albedo of 36 percent becomes just about 30 percent, in accord with the satellite values.

The realization that there probably wasn't anything wrong with the old observations and that earthshine could be a good quantitative measure of the earth's albedo motivates our modern observational program. Since I'm a theorist, my first urge was to rush to a computer and start modeling. And, being a professor, I couldn't resist the opportunity to educate someone else. So I enlisted two research fellows with a background not in planetary sciences or something germane, but, of course, in theoretical nuclear physics: Edwin Kolbe, who's visiting Caltech from Germany, and Ming Chu, BS '83, PhD '87. We three amateurs in the climate business soon had the first modern model of the earthshine working. This involves the well-known motions of the earth around the sun and the moon around the earth, but also the much more uncertain way



Research Fellow Edwin Kolbe (right) and grad student Jason Maron analyze the corrected lunar images taken at Caltech's Big Bear Solar Observatory (below).



in which the sunlight reflects from each patch of the earth. To know this, we needed to know the instantaneous cloud and ice cover over the whole globe for at least a year. Since that's pretty hard to come by, even with satellites, we used the results from one of the most advanced climate models developed by the European Center for Medium Range Weather Forecasting.

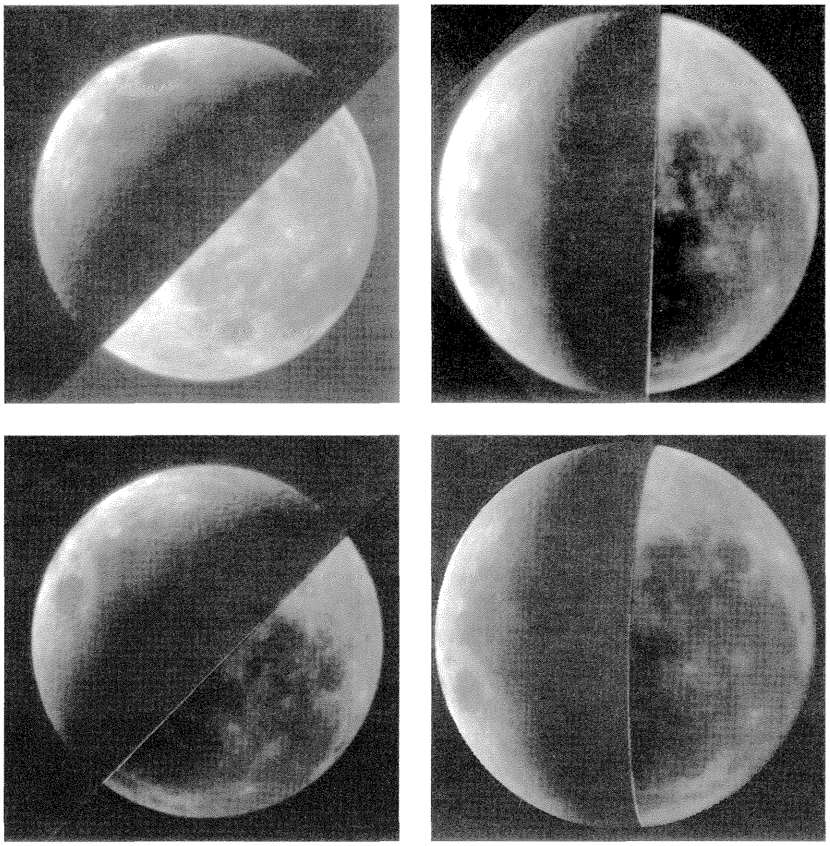
Our modeling confirmed the suspicion that earthshine was an excellent indicator of the earth's albedo. It also showed that we could determine the albedo with an accuracy of 0.2 percent (somewhat better than the satellite measurements) if we could measure the earthshine intensity to 1 percent. Danjon and Dubois had achieved a precision of 5 percent by eye, and, of course, technology has improved in the intervening decades.

At that point it was time to get professional, and so I interested my colleague, Professor of Astrophysics Hal Zirin, in the project. With funding from the Department of Energy's Western Center for Global and Environmental Change, we started taking regular pictures of the moon last May at Caltech's Big Bear Solar Observatory, of which Hal is the director. Rich Goeden, member of the professional staff, constructed the hardware, including a 6-inch telescope mounted on the solar drive, which uses a magical chip called a charge-coupled device (CCD) to produce digital images. Glenn Eychaner, BS '90, a Big Bear observer, and Research Fellow Jo Bruls have rearranged their lives according to the lunar cycle to take the pictures.

We use a modern version of the Danjon technique, in which a neutral-density filter dims the sunlit crescent, as seen on the monitor above. This allows both the earthshine and the sunshine to be recorded simultaneously. The images come out of the CCD camera as square arrays, 512 pixels on a side. Each of these pixels contains a number giving the brightness of that spot of the image. You might note in the raw picture on page 2 the variation of the sunlight across the crescent in contrast with the uniform brightness of the earthshine; this is a consequence of the opposition effect I described earlier.

The analysis of the images is being carried out by Edwin Kolbe and Jason Maron, a first-year graduate student, also in physics. To reach our goal of 1-percent precision on the earthshine measurement, the raw images have to be corrected in several ways. One of the most important corrections arises from the moonshine being about 10,000 times brighter than the earthshine, so that even a tiny bit of moonlight scattering in the atmosphere or the telescope can overwhelm what we're looking for. So we subtract this background.

A second problem is defining the comparison spots on the moon. Danjon's spots are not well-enough described for our precision work, and, because we have the whole lunar disk in our images, we're free to pick our own and as many pairs as we like. To find the spots with precision, we first scale the lunar image to a standard size, since the apparent size of the moon varies by 20 percent during the lunar month. Then we rotate it to a standard orientation. We can then let the computer find precisely the areas that we want.



This CCD image taken at Big Bear has been corrected in several ways. At top left a neutral-density filter dims the sunlit crescent; at lower left the background of scattered moonlight has been subtracted and the image inverted; at top right, the moon has been scaled to a standard size (it varies up to 20 percent over a month) and rotated to a standard orientation, in order to precisely locate the pairs of comparison spots; and at lower right, the image has been "delibrated" to correct for the moon's slightly changing face due to its elliptical orbit.

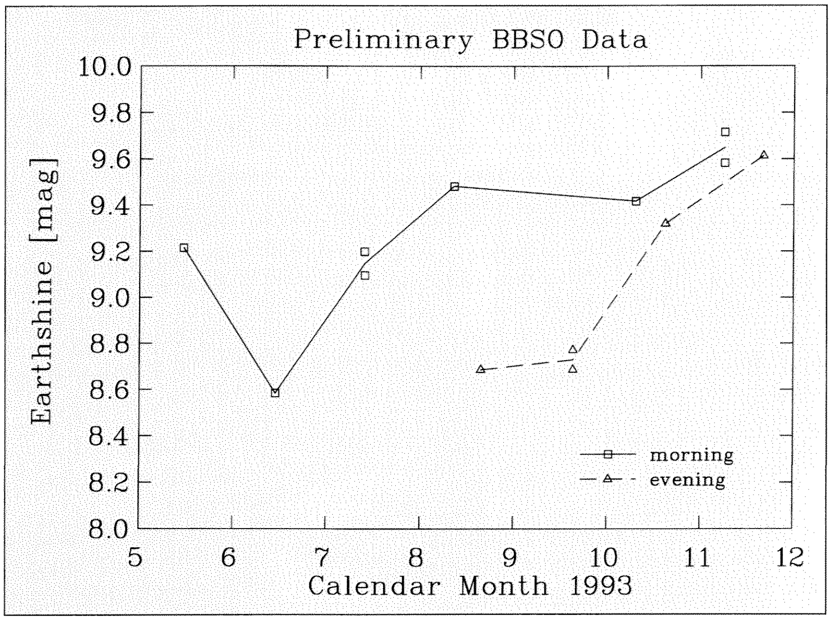
Another important correction is the lunar libration. Because the moon's orbit around the earth is elliptical rather than circular, it does not always present exactly the same face to the earth. Rather, it appears to swing back and forth, or librate, during the lunar month. Of course, the libration affects our spot finding, so we must "delibrate" the images using the known orientation of the moon. The libration also affects the brightness of our sunlit spot by changing the angle at which it's illuminated.

Finally, there is the issue of the lunar reflectivity. We need to know how the lunar reflectivity varies with angle for all of our spots. Since we've been unable to locate modern digital data, we're doing it ourselves by taking precision images at all lunar phases. Fortunately, Big Bear was cloud-free during the lunar eclipse last November 28, which allowed Glenn and Jo to take many pictures to catch the opposition peak I described earlier.

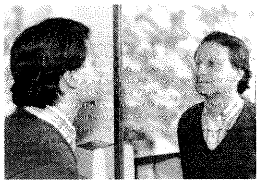
After applying these corrections and several others, we've been able to determine the relative variation in the earthshine (and hence in the albedo) since last May. The plot on the opposite page shows the brightness of the earthshine in astronomical magnitudes, measured each month when the moon's phase was plus or minus 120 degrees (that is, about a third of the way before or after new moon). While we would still term these data preliminary, several features are already evident. First, on several dates you see two points plotted. These were taken within two hours of each other, and so give some measure of the hourly variation in the earthshine. Second, you can see significant variations from month to month of a size comparable to that expected from the French work. Finally, there are clear differences between morning and evening observations. These are to be expected because observations in the evening (just after a new moon) sample the hemisphere to the west of us (the Pacific Ocean), while those in the morning (in the waning days of an old moon) sample the Americas and the Atlantic.

Although these data give you a feel for what we'll be able to do, they need further study before we can have complete confidence in them. We're really just six months into this program, and we're funded to continue for another 18 months. There will be more intensive earthshine observations at other lunar phases, as well as campaigns to map out the hourly and color variation.

The color of the earthshine, or more technically its spectrum, is also the subject of a more speculative activity that we're engaged in, still using the moon as mirror in which to view the



Corrected data from Big Bear Solar Observatory show the brightness of earthshine in astronomical magnitudes measured about a third of the way before or after new moon. Hourly variations are indicated by two boxes in one place; expected differences between morning and evening observations are due to sampling from different hemispheres. Strong monthly variations are similar to the French data on page 8.
Below: Steve Koonin monitors facial change in his own mirror.



earth. Everyone has probably seen that familiar demonstration in which light from the sun is broken into its component colors when passed through a prism. It's interesting to ask what happens to those colors when the light is reflected by the earth. The sunlight passes through the atmosphere and is either absorbed or reflected by the earth or from the clouds. During the course of that passage, light at particular wavelengths, or colors, is removed, the precise colors depending upon the various chemicals in the atmosphere. Carbon dioxide, for example, will have one particular pattern, water vapor another, and so on. These patterns will also depend upon the temperatures of the absorbing molecules. So, by measuring the spectrum of the earthshine, we might be able to monitor, on a global scale, the concentration and temperatures of various chemicals in the atmosphere.

Frank Very, an American astronomer, was, to our knowledge, the last to attempt to take a spectrum of the earthshine. He published a paper on it in 1914 with inconclusive results. Surprisingly, there have been few, if any, high-resolution spectral measurements of the whole earth from satellites, although there are plans to orbit at least one device capable of such measurements in a few years—at considerable expense, of course.

We had the feeling that it shouldn't be too difficult to take a spectrum of the earthshine since, after all, the light is bright enough to see and astronomical instrumentation has improved markedly since Very's time. So we enlisted Jim McCarthy, assistant professor of astronomy, and made two attempts last summer to take a spectrum of the earthshine using Jim's echelle spectrograph installed on the 60-inch telescope on Palomar Mountain. Unfortunately, we were hampered by scattered light, but we've got time this spring to try again more systematically under better conditions. Should we be successful in getting a spectrum and demonstrating that it varies from day to day or with the seasons, it could provide a different and very interesting way of assessing the changing global climate.

So, in the end, why are we doing all of this watching of the moon? I think there are at least three reasons. First, we hope to demonstrate that earthshine observations can be carried out with sufficient precision that they can usefully complement satellite observations. Certainly our modeling indicates that this is possible, but there's nothing like actually doing it to convince the skeptics. Second, we hope to establish firm benchmarks against which future observations can be compared to assess global change. Thus, if someone measures the earthshine 10 or 20 years from now, they'll have our precise and well-documented observations against which to judge changes. We might also try to calibrate Huffman's observations to tie back to the Danjon/Dubois work in order to obtain a 65-year record of global change.

And finally, a very important reason is that it's just plain fun to be working on a small, interdisciplinary project that might contribute a bit to one of the major issues facing society. I hope this presentation will let you share a bit of the amazing feeling I get when I look up at the crescent moon and realize that I'm watching the earth.

Steve Koonin's "real" research is centered on the structure and interaction of atomic nuclei. He spent his undergraduate years at Caltech (BS '72), earned his PhD at MIT in 1975, and then promptly returned to Caltech as assistant professor. He was appointed associate professor in 1978 and professor of theoretical physics in 1981. When he's not moongazing, Koonin has also developed an innovative course in computational physics, led the Caltech denunciation of cold-fusion claims in 1989, and was chairman of the faculty in 1989–91. This article was adapted from Koonin's January Watson Lecture. The graphics were originally developed for the lecture by Wayne Waller in Caltech's new Media Integration Laboratory, part of the Campus Computing Organization.

Lee A. DuBridge
1901–1994

Above: Lee DuBridge in 1981 at his 80th birthday party.
Left: DuBridge and his predecessor, Robert A. Millikan (left), in front of Throop Hall in 1951. Millikan died in 1953.

Lee A. Dubridge, president of the California Institute of Technology from 1946 to 1969, died January 23, 1994. At the memorial service on February 15 in Beckman Auditorium, five men who had known him well during the various stages of his long and distinguished career recalled with warmth and affection his impact on their own lives.

Joseph B. Platt
Retired President, Harvey Mudd College

Lee DuBridge was a very important person in my life. I first heard of Lee DuBridge in the spring of 1934, when I was a freshman at the University of Rochester, in New York State. I wasn't sure what I wanted to do when I grew up; I was interested in all sorts of things. Physics looked like it might be an exciting field. Both the neutron and the positron had been discovered in my last year of high school, and who knew what might come next? Then I learned that a 33-year-old physicist from Washington University in St. Louis, Lee DuBridge, was coming to Rochester in the fall as full professor. He had quite a research record in photoelectricity. I decided I would major in physics.

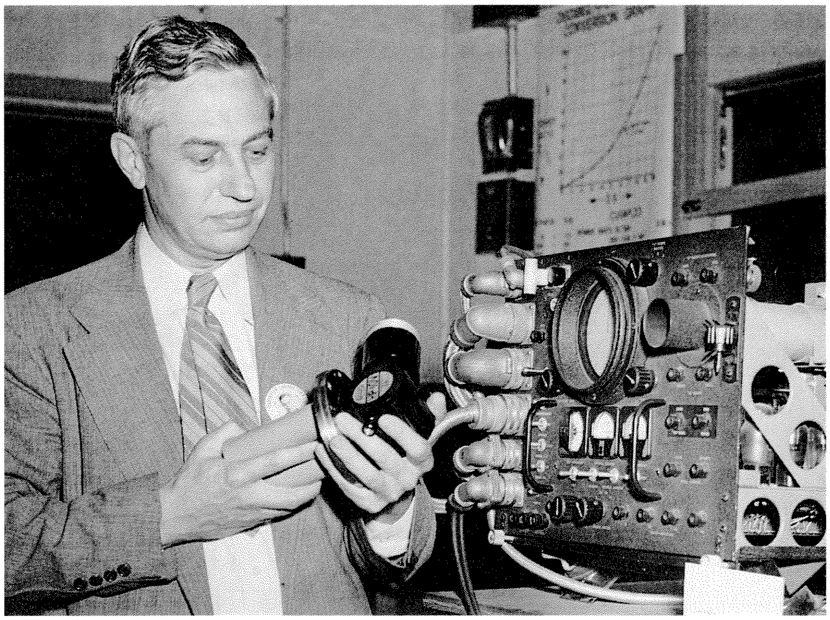
Indeed Rochester turned out to be a great place to study physics. In my sophomore year Lee had the department building a cyclotron, the third one in the United States. By the middle of my junior year our cyclotron was accelerating protons to an unheard of seven million electron volts. There was a big table of isotopes on the wall of the control room, with cup hooks in every slot where an unstable isotope could be expected. As a new radioisotope was discovered, a tag went on the appropriate cup hook: blue for Berkeley, orange for Princeton, and yellow for Rochester. In my senior year a good portion of the wall was in Rochester's dandelion yellow. That was heady stuff.

My senior thesis involved making an electrometer to measure short half life radioactivities, and then actually measuring one. I did these things. On my senior oral it turned out I didn't understand what I had measured; I had found a beta activity of a few minutes, but what was it? I could balance the equation, but I couldn't believe I had been measuring positrons—after all, not long ago they had been discovered by Carl Anderson in the cosmic radiation, and I didn't know we had them on the East Coast. Lee DuBridge pointed out that they were as "common as snakes in the grass."

By the time I graduated, I knew that Lee was a warm and generous person, an excellent teacher and scientist, and a man who got things done. These are precisely the things for which he became internationally famous.

I did my graduate work at Cornell. When I had nearly completed my doctoral work, Lee offered me an appointment at Rochester as instructor in physics, and I accepted. But that was September 1941. Lee was already gone much of the time, setting up the MIT Radiation Laboratory. Over the next 18 months my other senior colleagues disappeared to Los Alamos, Berkeley, the Naval Research Lab, and the like. By the summer of 1943 I was an instructor in physics and the acting department chairman, teaching introductory physics to Navy and Marine officer candidates with the help of what high school teachers I had been able to assemble. The I, too, left for a war-lab job, working for Lee at the MIT Rad Lab.

The MIT Rad Lab was a busy and exciting place. The laboratory developed the techniques for producing, detecting, and manipulating microwaves. The magnetron, a British invention, was developed there from a bench demonstration to a production item, and the wavelengths available were extended from ten to three centimeters, and then to one. All the detecting and modulating equipment, plumbing, and the like had also to be invented or developed. From this came a plethora of radars: there were radars for detecting aircraft or submarines or ships at a distance, radars for controlling air traffic or air interceptions, radars for making air interceptions at night, radars for gunlaying, radars for blind bombing, and much else. Most of the develop-



At the MIT Radiation Laboratory in August 1945, DuBridge, holding a magnetron, explains radar to the press during the first public release of information about the lab (Associated Press photo).

ments of the Rad Lab were transferred to commercial corporations for manufacture, and along with the hardware there were also training programs to run, training manuals to write, and much else to do—including introducing these new gadgets into combat. Some 4,000 of us were employed by the Rad Lab, of whom I estimate at least 400 (including some of our best) were unaccustomed to taking orders from anyone. In his soft-spoken and persuasive way, Lee had us all working as a team.

In 1991, at the 50th anniversary of the Rad Lab's founding, Norman Ramsey traced the postwar applications of the wartime Rad Lab radar work. These include, of course, air traffic control, microwave communications, and microwave ovens. Norman himself used his newly learned microwave techniques to invert the electron populations of molecules, which led to the maser, which led to the laser, which now seems to be an essential link in all kinds of optical communications. The medical people are finding nuclear magnetic resonance to be a useful diagnostic tool. Meanwhile, the radio astronomers used some of the same technology to measure the temperature of outer space. All sorts of timing circuits developed for radar were at hand when they were needed, first for television, and then for computers. Lee and his team left quite a legacy.

At the end of the war Lee returned to Rochester and invited me along. I was overjoyed. There had been many rumors of corporate or university presidencies that might have lured him elsewhere. Lee promptly got us all involved in building a synchrocyclotron. But Lee and Doris left in June 1946; Caltech was too much to resist!

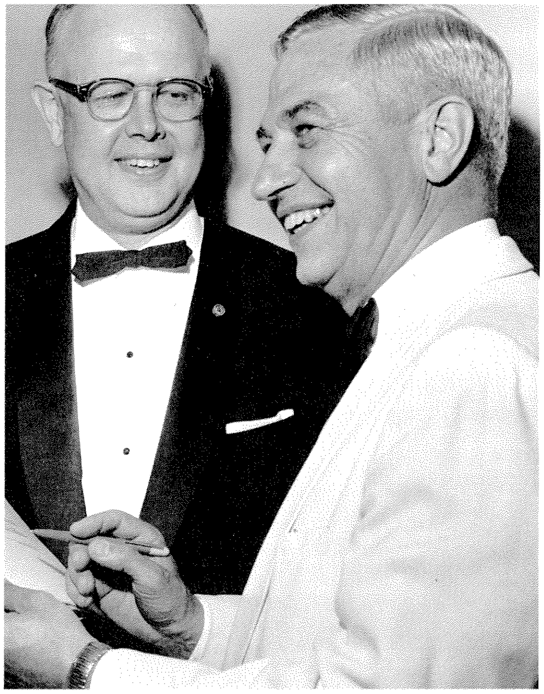
Ten years later, in 1956, I was approached about becoming president of a nonexistent college in Claremont. My wife and I knew very little about Southern California, but we had two families of former mentors at Caltech: the DuBridges, and Jean and Bob Bacher. Both couples encouraged us to give the move very serious thought, and both had volunteered any personal help they might be able to give. As most of you know, we did come. There is no official connection whatsoever between Harvey Mudd College and Caltech, but there are many interpersonal ones. We opened our doors in September 1957. Our first commencement came in June 1959, when we graduated two students who had been upper-class transfers—both mathematicians, since we did not yet have any upperclass laboratories. Our commencement speaker was Lee DuBridge, president of the California Institute of Technology. We had a burst of applications the next month from prospective freshmen who had seen our commencement on television.

Lee was a very good scientist, a great administrator, and a warm friend to everyone. Our lives, and those of thousands of others, are the richer for him. I thank you for this opportunity to tell a little of the part of his life I had the good fortune to share.

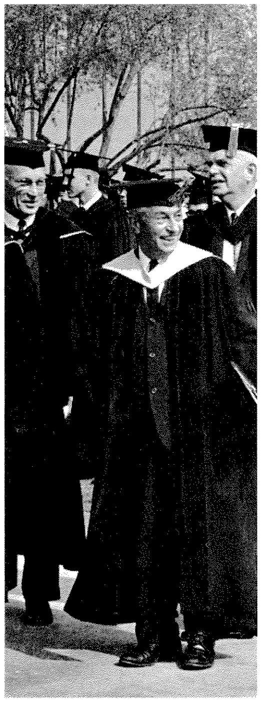
William A. Fowler
Institute Professor of Physics, Emeritus

Lee Alvin DuBridge made a creative change in the administration of the California Institute of Technology in 1946. It is true that Robert Andrews Millikan had transformed the Throop Institute of Technology into Caltech in 1921. But Millikan never became president of Caltech. From his knowledge of the experiences of the president of the University of Chicago with the board of trustees at that university, he preferred the position of chairman of the executive council of the board of trustees at Caltech. Thus it came about that Lee DuBridge was the first president of Caltech.

I first came to know Lee DuBridge in 1946. Charles Christian Lauritsen, my PhD professor, and I attended a meeting that Lee chaired of the American Physical Society at the University of Rochester. DuBridge was a professor of physics at Rochester from 1934 to 1946, where he supervised the construction of a cyclotron that produced the highest energy proton beam at that time. On leave of absence from Rochester he



Right: Willy Fowler and Lee DuBridge in 1958.
Below: Commencement 1960.



headed the Radiation Laboratory at the Massachusetts Institute of Technology, Caltech's East Coast branch, and led the development of radar for the military. Without radar we probably would not have emerged victorious from World War II.

Lee spotted Charlie and me in the audience, and at the end of the meeting he came off the platform and extended his arms to us. His spirit, his charm, made me realize that Caltech would never be the same. What Caltech is today, is in large measure the result of his spirit and charm as well as his administrative ability and devotion to making Caltech the great institution it was when he retired in 1969 and as it has continued to this day under his successors as president. Like many of you, I generally do not like administrators, but I loved Lee Alvin DuBridge.

DuBridge's book, *Photoelectric Phenomena*, coauthored with Arthur Llewelyn Hughes at Washington University and published in 1932, was the first book I ever purchased in the form of a brand new copy. All of my previous textbooks I had borrowed or purchased secondhand at the bookstores in Columbus, Ohio, or in Pasadena. A library copy of the book fascinated me, and I decided I wanted a brand-new copy of my own. Permit me to read to you from the preface:

"The output of theoretical and experimental results in physics grows at an ever increasing rate. The task of keeping abreast of recent developments becomes correspondingly more and more difficult. There is therefore ample justification for the publication of any book whose purpose is to give a concise yet comprehensive survey of one of the many fields of physics. For those merely wishing to obtain a general idea of the recent progress in a particular field, a book of this kind should save many a tiresome search through a voluminous literature. For those actually working in the field such a book justifies itself if it provides a fairly complete summary of the experimental methods and results, and of the prevailing theories, as well as a time-saving sign post to the original papers in the subject. For those interested in the technical branches of the subject, the book should serve as a guide to the fundamental physical principles which underlie the engineering and commercial applications. It has been our aim to prepare such a book covering the field of photoelectricity."

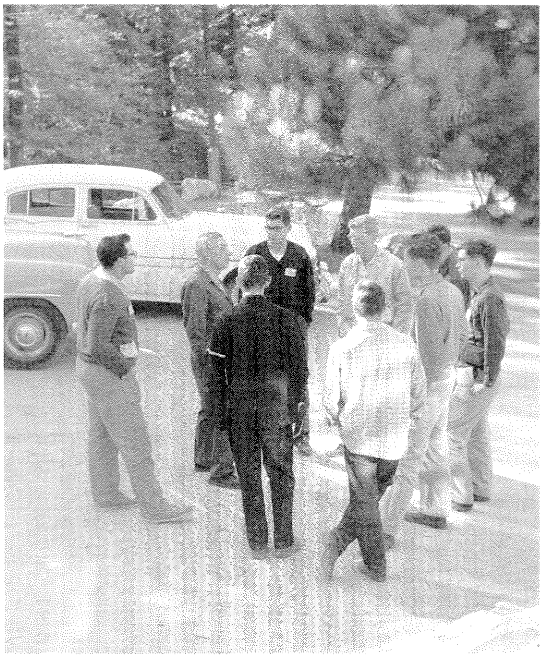
Finally, permit me to mention Lee DuBridge's impact on my own life and career. At the beginning of his presidency our teaching load was reduced to one three-hour teaching course per quarter. There never had been teaching during the summer quarter, and in fact many of us taught only two quarters. I taught only during the fall and winter quarters so that I could go in the spring and summer to Cambridge University in England to collaborate in research with Fred Hoyle. The research with Hoyle and that which I did on my own brought me many rewards, including the Nobel Prize in physics in 1983, which I shared with Subrahmanyan Chandrasekhar.

So, I do indeed owe very much to Lee DuBridge, and I told that to Lee many times. He always replied: "Keep it up, Willy, I'm on your side." He still is on my side and on the side of everyone at Caltech. His memory stimulates and renews us, and we shall never, never forget him.

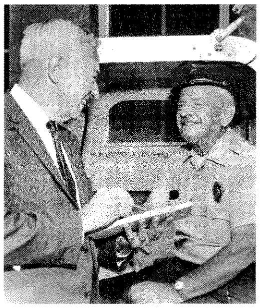
Harry B. Gray
Arnold O. Beckman Professor of Chemistry
Director of the Beckman Institute

It was the spring of 1965, and Shirley and I and our children had come to Pasadena to look at Caltech. We'd heard a lot about Caltech. We had heard that the students were smarter than other students, and the faculty smarter than faculty at other places. Full of enthusiasm, I started interacting with the chemists and I discovered immediately that the people at Caltech weren't smarter than other people. The students weren't any smarter than the students I'd encountered at other places and the faculty didn't seem any smarter. Perhaps that's because I hadn't met any *physicists* yet; I'd met only chemists.

But we discovered that there was something else about this place—there was a wonderful spirit here. There was a tremendous friendliness about the place; it was really quite un-university-like, the spirit and the friendliness. And we naturally wondered what in the world this could be due to. It was so friendly that Jack Roberts offered me a job—that's what I call friendly—and said that we might want to stay here in Pasadena and at Caltech. When I went in to talk to him about it, he suggested that I'd want to talk to Lee DuBridge about this prospect. I looked at Jack and I said, "Jack, isn't Lee DuBridge the president of Caltech?" And he said, "Yes, yes," in his usual way. I didn't say anything to him at the time, but I was thinking, "Jack, you don't *talk* to presidents, at least where I've been. Presidents are people you read about in the paper, hobnobbing with the trustees. Faculty members don't talk to the president." But he kept talking, and then I realized as he was talking that apparently at Caltech you *could* talk to the president. You could talk to the president regularly. And I walked out of Jack's office thinking, "Well, maybe I will call Dr. DuBridge and talk this over with him."



Right: The president talks with students at Freshman Camp in 1956.
Below: DuBridge congratulates A. G. "Fig" Newton on his retirement in 1968 after 20 years as campus security officer.



I never did call Dr. DuBridge, because he called me first. He wanted to talk to me. He said, "I've heard something very exciting—that you might move and do your chemistry at Caltech." So I made a beeline over to Throop Hall to see him. When I walked into his office I realized immediately why the chemistry department was so friendly. *Everybody in the president's office was friendly.* I wasn't used to this.

I'll never forget my first meeting with Lee DuBridge. His eyes were sparkling and his smile was infectious. There was that spirit that Willy was talking about. He made me feel at home immediately. He was interested in what *I* was doing. I learned he was a physicist, which was very peculiar—for a physicist to be interested in what I was doing—because where I had come from, Columbia University, the physicists were always putting the chemists down. They were always telling us how inferior we were. And I was always having to fight back at Columbia. I think during that conversation Lee DuBridge also told me that he thought physics was better than chemistry, but he did it in such a nice way that I felt good about it. I felt so good that I thought I could tell him one of my Columbia physics stories.

I don't think I'd ever spoken to the president of Columbia, Grayson Kirk, but I had sent him a memo once when I'd learned that he was planning to build a physics building on the lovely Barnard tennis courts that I played on. I said, "Dear President Kirk: In my opinion a good set of tennis is worth at least six physicists." And Lee DuBridge laughed at that with that wonderful laugh of his—you all remember that wonderful laugh of his—and he said, "Harry, if you'll come to Caltech and do your chemistry here, I'll promise you right here and now that I'll never build a physics building on the Athenaeum tennis courts." And I walked out of that meeting with him feeling great. I felt so much better about my science and about Caltech than I did before I talked to him.

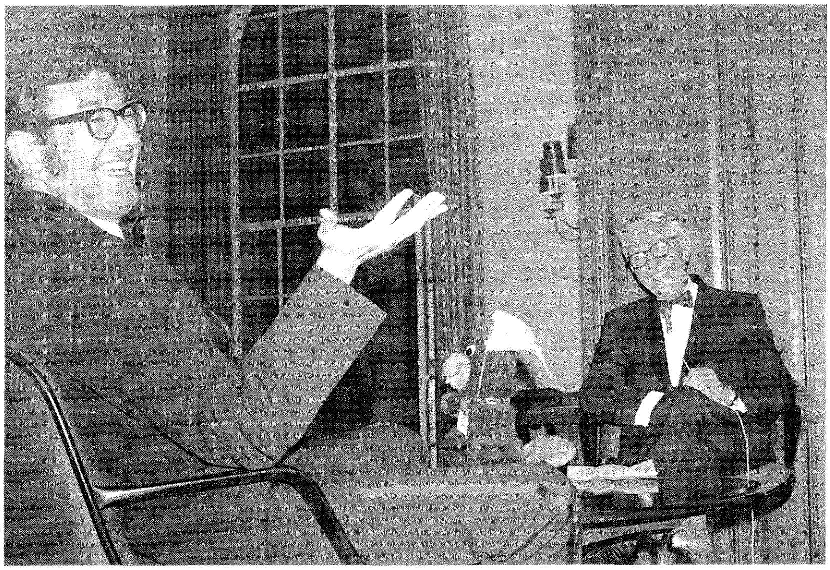
Over the next few weeks while we were visiting here, I had many occasions to talk to Lee and get to know him. And I observed how he dealt with people on this campus. This was his campus; these were his people. He cared about everybody. He cared about staff, students, faculty; he wanted to know what *they* were doing, what *they* were interested in. And he talked to them. I observed the phenomenon that I had experienced myself, that when Lee talked to people he pumped them up. You always felt better about science, about Caltech, about whatever, after you talked to Lee. He lifted our spirits. And he showed us how to act by his example.

He was enthusiastic about science; he loved science; he loved to talk about science. He loved to communicate science, not only to scientists on campus but to nonscientists. He taught us that that was a good thing to do. It was OK to be enthusiastic about your science, to talk to your colleagues; it was even better than OK to be enthusiastic about your science in communicating it to nonscientists. I think that Lee felt that one little bit of good science was much better than six good memos. He had his priorities straight and he taught us how to get our priorities straight. We were enormously proud that he was our president.

He was a lucky man, a very lucky man. He was able to share his life with two wonderful women, Doris and Arrola. How lucky can you be? After a life with one beautiful lady, he was lucky enough to marry a lady like Arrola. Her eyes sparkled more than his—particularly after she came out a winner in their afternoon Scrabble game. And her smile always has been infectious. Lee loved Arrola very much.

I last saw Lee just a few weeks ago at the Athenaeum. I never gave up trying to sell him the greatness of chemistry and trying to get him to admit that chemistry was almost as good as physics. I tried it one more time. We made small talk and then I said, "Lee, you know, I think we're doing pretty well here in chemistry. There are a lot of exciting things going on; don't you think so?" And he said, "Harry, you're doing OK, but you still have a ways to go to catch up with physics." He was a lot slower than he'd been 30 years before, when I'd first met him. He was slower, but his eyes were still sparkling and his smile was still the same. And when I walked out of the Athenaeum I felt good, as I'd always felt after I'd talked to Lee. I felt better about Caltech, and I felt better about science.

Lee DuBridge lifted our spirits. He showed us how to act. We shall miss him very much.



At a 1976 dinner celebrating the 50th anniversary of the founding of the Associates, Harry Gray and Lee DuBridge reminisce about Gray's early days at Caltech. In the *E&S* account of the occasion, Gray said he told the Columbia president that a good set of tennis was worth "at least *12* physicists." Apparently Gray's memory has accounted for some inflation in the value of physicists since then.

Ruben F. Mettler
Chairman Emeritus, Board of Trustees

Lee DuBridge was a great American, and he leaves a great legacy. His personal research, his successful institutional leadership and management, and his warm personality had a national impact that was profound in many dimensions—science and university research; engineering and technology; national security; university administration and entrepreneurship; national science and technology policy; spokesman for the role and significance of science and technology in society; and more.

In this context, I wish to speak from personal experience as one who first met Lee almost 50 years ago, and then had the good fortune of subsequent association in a variety of circumstances during those years. I want in this way to highlight some of the exceptional abilities of this remarkable man.

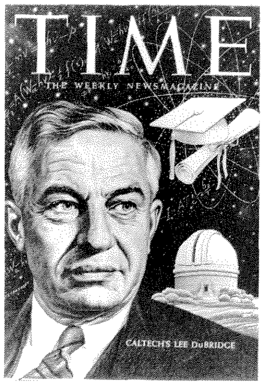
Lee was persuaded, in 1940 when he was a professor of physics and the chairman of the physics department at the University of Rochester, to become the founding director of the MIT Radiation Laboratory. The Laboratory's mission was to invent, develop, and put into production, airborne, shipborne, and land-based radar. The Battle of Britain had begun, and the United States was moving toward war.

With brilliant leadership, he recruited leading scientists and engineers on a crash basis as the start-up staff of the Radiation Laboratory, which then grew to become the nerve center of an immense national enterprise involving many other organizations. Radar systems were successfully developed, put into industrial production, and installed in combat aircraft, ships, and ground equipment. Military personnel were trained for radar operations and maintenance, and logistic support systems were established. Throughout the war, the Radiation Lab provided direct on-the-spot operational advice and support to the forces in both the European and Pacific Theaters.

I was one of a group of Navy officers who were sent to the Radiation Laboratory for a crash training program in the operation and maintenance of shipborne radar systems, before shipping out to the Pacific. Despite all his other duties, Lee visited and talked to us several times. His ability to describe our task in a national context, his warm personality, and his straight-arrow responses to sensitive questions made an indelible impression on me.

When I came to Caltech as a graduate student in 1946, Lee was the new president. His dynamic and entrepreneurial qualities were highly

Right: Richard Nixon, then vice president, visits the Caltech campus in 1958. A decade later DuBridge became special assistant to the president for science and technology during Nixon's administration. Below: In its cover article in 1955 *Time* called DuBridge the "Senior Statesman of Science." Opposite page: DuBridge takes a curtain call after the faculty production of "Lee and Sympathy," a surprise to honor his 20th anniversary as Caltech president.



visible in the transition that Caltech was making from wartime to peacetime conditions and in his efforts to position Caltech for leadership in newly emerging opportunities in research and teaching. Lee met with graduate students individually and was an inspirational source of advice and encouragement for me.

The 1940s and 1950s were a critical turning point for federal support of science and technology. The dramatic contribution of science and technology to the war effort, and the famous paper by Vannevar Bush, led, after some fierce political battles, to the establishment of the National Science Foundation. Science and technology began to assume a new and permanent place in national policy with respect to national security, foreign affairs, and the domestic economy.

Appointed by President Truman to his newly formed Science Advisory Committee, and continuing as chairman of the committee in the Eisenhower administration, Lee became an active participant in helping to shape national science and technology policy, an interest he continued for two decades.

In 1954 I spent about a year in Washington working in the Pentagon and traveling often to the White House to meetings concerning various projects of interest to the Science Advisory Committee. When Lee was in a meeting, his analytic and persuasive manner in discussing complex and difficult policy issues gave him a highly productive leadership role.

In early 1969, Lee was asked by President Nixon to be his special assistant for science and technology and then a member of the President's Science Advisory Committee. One day he called me and asked if I would respond favorably if the president asked me to chair a task force on national science policy with a focus on issues of significance in that time period. I was pleased to accept that task with Lee's support and guidance. Once again I could observe Lee's wisdom and experience, and his deep understanding of how science and technology affect policy design and policy outcomes.

Finally, I'd like to say a few words about Lee from the perspective of one who has been a Caltech trustee for about 25 years and chairman of the trustees in recent years. When Lee returned to Pasadena after serving in the Nixon administration, he was honored with the title of president emeritus and served as a lifetime trustee of Caltech. He continued as an active participant in Caltech affairs generally, and in trustee meetings in particular, often with penetrating observations and questions. He was an active participant in a meeting of the trustees just two weeks before his death. Presidents Brown, Goldberger, and Everhart all benefited from his experience and wisdom and dedication to Caltech, as did the trustees, the faculty and students, and all parts of the greater Caltech community.

Lee DuBridge was a towering figure in Caltech's history and in the world of science and engineering. He was also a kind and compassionate man, with a strong love of family and friends. All of us are fortunate to have known such a man. His devotion to Caltech was complete. He often said he thought Caltech was the most wonderful place in the world. All of our lives have been enriched by Lee DuBridge, and we will all miss him very much.

Thomas E. Everhart
President

When Lee Alvin DuBridge was born in 1901, most physicists then living thought that almost everything had been discovered about their field. They were wrong. Lee DuBridge participated in and lived through one of the most exciting periods in physics, his chosen field. Louis deBroglie showed the relationship between particles and waves when Lee was a graduate student, and Lee worked on the photoelectric effect, in which the energy of a photon of light is transferred to an electron, allowing it to escape from a solid. He came to Caltech as a National Research Council Fellow in 1926 to work with

All of our lives have been enriched by Lee DuBridge, and we will miss him very much.

Robert A. Millikan, who had made a major contribution by studying this effect.

He learned more than physics from Millikan. In some "Memories" he recorded for the Caltech Archives, he mentions conversations with Millikan: "These were entrancing introductions to the larger world of science and scientists." In addition to Lee's own research, this period of time was exciting for him "because of all I learned in lectures, seminars and personal associations with the many fine scientists who were at Caltech at that time." He specifically mentions Richard Tolman, Paul Epstein, Robert and Clark Millikan, and Clinton Judy (who was in English literature). He liked the breadth of these faculty, and enjoyed luncheon with them and others at the "round table at the old faculty club." He commented that to hear these and others "freely and learnedly discussing topics in literature, art, music, and other fields was an education in itself."

Following Caltech, Lee started up the academic ladder at Washington University in St. Louis, and then was called to be professor of physics and chairman of the physics department at Rochester in 1934. His talents were soon noted there, when he was appointed dean of the faculty of arts and sciences in 1938. In 1940, he was called to be head of the Radiation Laboratory at MIT, and as you have heard, recruited and led a large number of scientists in an important effort that undoubtably helped to save many lives and win World War II. Lee showed a remarkable ability to keep a diverse and independent group of academic scientists focused and working together.

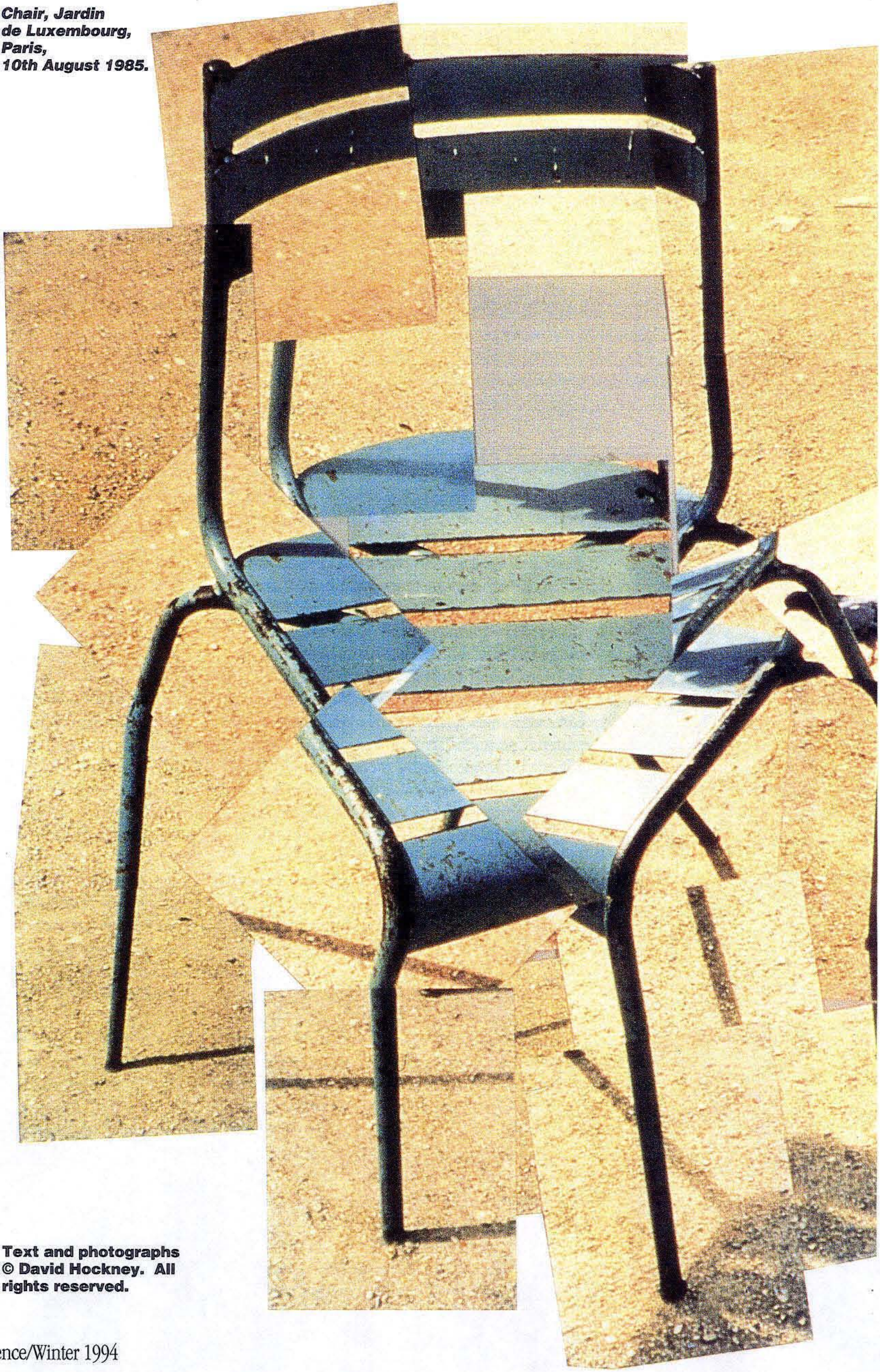
Following the war he returned to Rochester, where he was active in helping to organize new laboratories for high-energy physics. When Lee stepped down as head of the Rad Lab, he said he did not want another administrative job. Yet, on returning to Rochester, he found himself behind in his own field. While he had led the Rad Lab, focused on radar and associated electronics, great progress had been made in nuclear physics at Los Alamos. In his own words, "Catching up would be tough. I slowly realized that, after all, I had enjoyed administrative work in a scientific atmosphere. Hence, though I at first declined to accept Max Mason's urgent plea that I come to Caltech as president, I finally came to the realization that it was the right thing to do. I never regretted the decision."

Because of his background, he was excellent at explaining the exciting developments in physics to the public. He became a spokesman for science on the national scene, and helped the public appreciate what science could do for the nation. But especially here at Caltech, he was appreciated as a warm human being who helped others mature and become more than they might have otherwise. It is for these qualities, as well as for his courage, his decisiveness, and his leadership that we remember him today.

Recently, an asteroid was named in Lee's honor. I understand from Eleanor Helin that No. 5678 DuBridge is some 15 kilometers in diameter, and inclined some 34° to the ecliptic. Ted Combs fortunately let Lee know this shortly before he died. Lee was touched. Earlier he had had a mountain named for him in Antartica, and was pleased to have another mountain flying around in space carrying his name. This seems to me a fitting memorial to the man who had overseen the Jet Propulsion Laboratory as it entered the space age.

Lee was an extremely supportive person, especially when it came to Caltech. After I was named president, his was one of the first letters of welcome and congratulations that came, and Doris received a wonderful letter of welcome from Arrola. I learned from him here, too. He could give an extemporaneous talk that made everyone present feel special—a significant talent for a college president. When I would visit him sometimes, to bring him news of the Institute, he was always interested, always constructive, always supportive. He was truly a great president of Caltech, and after he retired, a great supporter, trustee, and friend. We will all miss him. Our world is a better place because of Lee DuBridge, and we are better people for having known him. ☐

Chair, Jardin de Luxembourg, Paris, 10th August 1985.



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Which Way Is Reverse?

> *We are still looking at pictures of the world and what we think the world looks like.*

by David Hockney

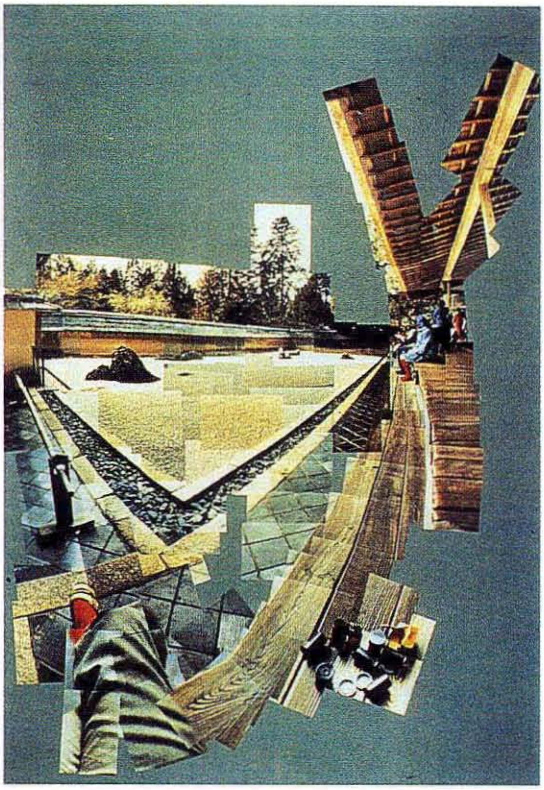
The second annual James Michelin Distinguished Visitor Lecture brought artist David Hockney to a packed Beckman Auditorium last November 16. Established by a gift from New York fashion designer Bonnie Cashin in memory of her uncle, the lecture series is intended to foster a creative interaction between the arts and sciences. James Michelin was "a splendidly talented petroleum geologist," according to Vice Provost and Professor of Physics and Applied Physics David Goodstein, who introduced the lecture. Although Michelin was educated at UC Berkeley, he held a longtime dream to return to study at Caltech. "It may be precisely because he never did so that the family has such warm feelings toward us," Goodstein suggested.

Goodstein sought to introduce Hockney, one of the world's best-known and most influential artists, "in a way that will be more meaningful to us scientists than a list of his one-man shows." He described how someone, several years ago, had given Hockney a Polaroid camera, which in Goodstein's own hands might have produced a few fuzzy snapshots. "But with a camera in David Hockney's hands, what we got back was nothing less than a whole new way of seeing the world." Paying Hockney "the highest compliment I'm capable of paying," Goodstein compared him to Richard Feynman, who also "saw the world with fresh new eyes."

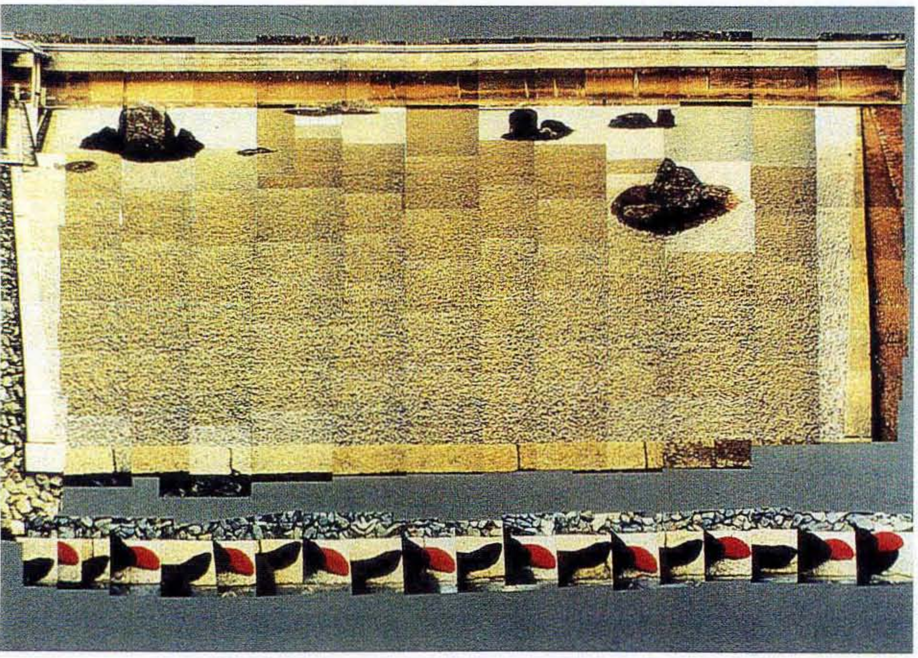
I will tell you straightaway I'm not a professional lecturer of any kind. I don't teach either, so I'm not that used to it, although I have given some lectures. And when I was asked to give a talk here, I did think about it and thought, well, I suppose there are things that could interest scientists as well. And I thought perspective could be interesting, so I agreed to come and talk about it. The main thing that I'm going to talk about is the depiction of space on a flat surface—what it does to us and so on, and my own inquiries into this. I've been deeply interested in photography, because of perspective in photography.

Picasso was one of the first artists to make an attack on perspective. These female nudes are seen from many different angles. [These were unavailable for reproduction, but can be viewed in the Zervos Catalog.] The journey he made over the 40 years between these two works (the first one was painted in 1908) is fascinating, but not much explored today. Nevertheless, we are still looking at pictures of the world and what we think the world looks like. I mention Picasso here because, fortunately or unfortunately, at the same time another kind of picture came along that people thought was much more realistic—the moving picture. Everybody thought it was much more vividly lifelike than cubism.

A lot of people in Hollywood are interested in putting reality onto a flat surface. They're always trying to make more vivid movies, and one of their attempts was what they called 3-D movies. These always seemed to fail, never seemed to get anywhere. I thought it was for a quite simple and obvious reason, a question of simple arithmetic: They'd actually got it wrong and what they were really trying to do was make 4-D movies, because any movie is three-dimensional in the sense that it's got two dimensions of space, and linear time makes it three dimensions. And all attempts to make four-dimensional movies will fail because that would be like real experience, and you'd be so confused you wouldn't



Right: *Sitting in the Zen Garden at the Ryoanji Temple, Kyoto, Feb. 19, 1983.* Below: *Walking in the Zen Garden at the Ryoanji Temple, Kyoto, Feb. 21, 1983.*

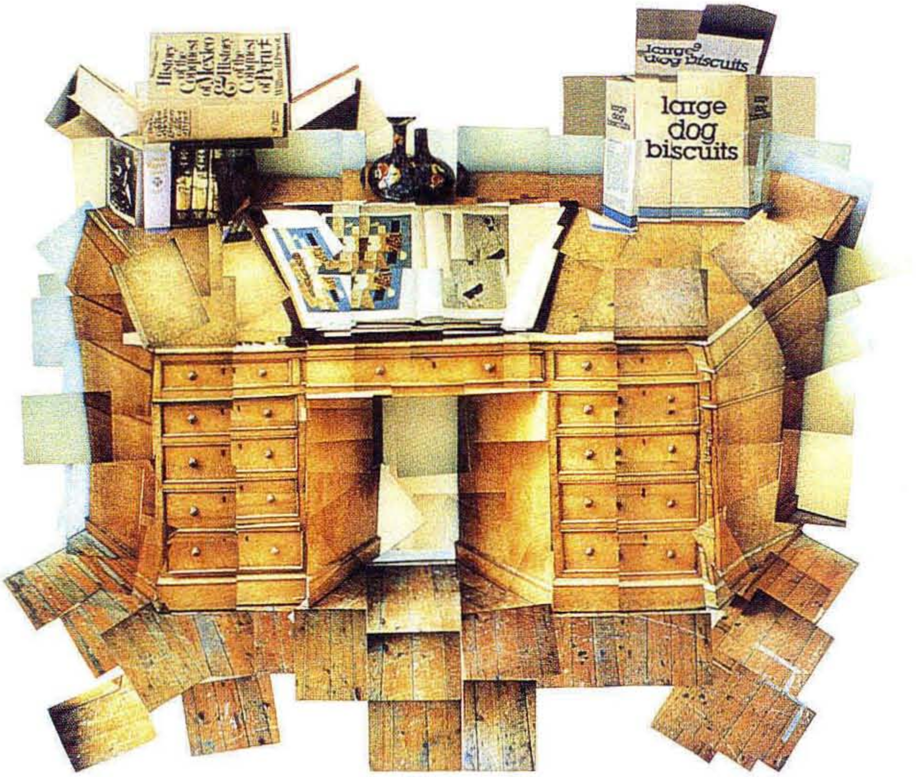


If you suggest that there's movement in the viewer, the shape of things alters, and I was fascinated by this. . . . It means maybe we're not sure about the shapes of things anyway.

know you were at a movie. In movies it's very difficult to show grandeur in space. Fifty years ago the novelty of movies was so great that we accepted pictures of grandeur—*Ben Hur* and things like that; we accepted the space. Today we see so many moving pictures that I don't think we can accept it in the same way.

There *are* different ways to make a more vivid picture. One of my interests in perspective in photography and the reason why I felt like giving a talk here on what I call reverse perspective is that I think we accept too easily particular ways of looking at the world. We accept the "realism" of photographs, and I think this will begin to give us problems. These problems would be noticed first, I think, by people who actually make, or construct, pictures. Of the two photographic collages I made of a Zen garden in Kyoto, Japan, the first one [left, above] was made with me just sitting there—you can see my legs —and I'm just moving my head and my eyes around with the camera. And that's the kind of shape you get; the garden itself was a rectangle.

Then I thought about this garden quite a lot, and I wondered how it would be possible to make it a rectangle in a picture, even in a photograph. Of course, the most obvious way would be to rent a helicopter and go above it and point your camera down, and the garden would be a rectangle as it is in nature. And you could do that. But then, while I was walking around Kyoto, it occurred to me that to make it a rectangle you have to see it as a rectangle, which means you have to move. The next photograph is the same garden. I calculated how many photographs I



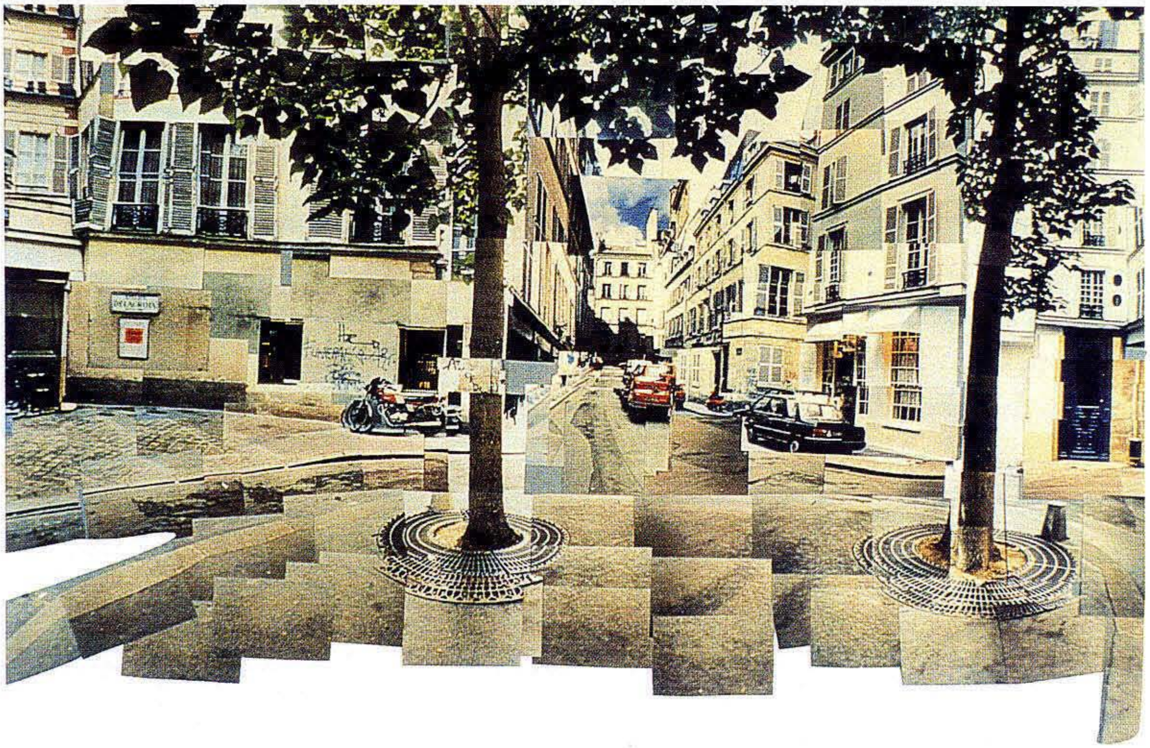
The Desk, July 1st, 1984.

needed to take (I should take more photographs at the top than at the bottom because of what happens), and then I stood along the bottom—you can see where my stocking feet make a pattern. I used four rolls of film, each with 24 exposures—that's the lot; I used them all—and a half-frame camera, a tiny Pentax that I carried around in my pocket. All the pebbles actually are in the right place; there's no repetition (you'd recognize the repetition of the pattern if you just took one picture and filled it in), which meant that I had to look at all the pebbles and connect each photograph. I had to fix points—fix little patterns that I could then link with the next photograph and so on. And I was counting all the time. Other photographers there were probably thinking I was a little mad really, with this stupid little camera that normally any photographer would think was a very unserious camera. But I realized if you make a collage using plenty of negatives, you're actually making a picture with quite a big negative, really.

So, if you suggest that there's movement in the viewer, the shape of things alters, and I was fascinated by this; I'm still fascinated by it. It means maybe we're not sure about the shapes of things anyway. Here's a desk that I photographed. Again, to get reverse perspective it means you have to move. In ordinary perspective the infinity is a long way from the viewer. In reverse perspective the infinity is actually in you, the viewer. I've made the suggestion that if the infinite is God, then in pictures with ordinary perspective you could never connect. But in reverse perspective you can connect because it's within you as well. This gives a theological explanation for reverse perspective that seemed to make more sense to me.

The vanishing point was an invention of 15th–16th-century Italy. It is only European. The moment people realized what it was, military technology was able to develop, using triangulation and so on. But there are other connections. The vanishing point means that the viewer is very still. On a Chinese scroll it's not possible ever to have a vanishing point because it would mean you'd stopped moving. In 15th-century Italy most of the pictures being painted were commissioned by the church, mainly the depiction of the crucifixion. This is speculation on my part, but if you look at the first pictures where they used one-point perspective, there's a great advantage and a disadvantage in it. The one great advantage is that the volume of a body looks weightier; in fact, it could show suffering better. So this would make it attractive to the church. In Eastern religions nobody developed the vanishing point. I made a movie about two Chinese scrolls—one where the vanishing point was never used and another where it was. I suggested that the latter one showed an artistic decline. And China did decline from a country that was obviously very advanced in the 16th century. When I asked why, I was told that they'd lost their intellectual curiosity—and military technology was better elsewhere. Military technology was clearly connected with the vanishing point.

I think it's quite fascinating to be able to make new kinds of space in pictures. The chair



Right: ***Place Fürstenberg, Paris, August 7, 8, 9, 1985.***
Below: ***Paint Trolley, L.A., 1985.***

in the Luxembourg Gardens in Paris [page 22] was made from one role of film; the whole picture is 24 negatives on a little role of 110 film. For one of the first very complex pictures I made, of the Place Fürstenberg in Paris, I was moving about constantly. I also had to construct it in the Place Fürstenberg. I would shoot one day, then have the photographs printed, glue them down on a board, and then take the board with me to tell me what to do next. For instance, say, to take the photograph of the Atelier Delacroix, I was standing over to the left in front of it, not in the center. The viewpoints are actually many, creating the effect of a different kind of space. It seems to me that way.

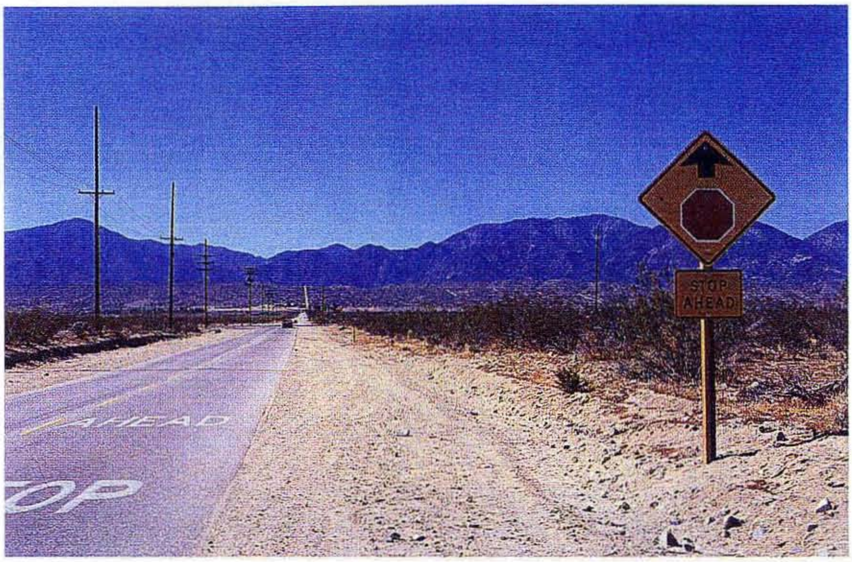
Another aspect of altering perspective this way I noticed was that you could get closer to things by being actually involved in them. It seemed to remove a distance. The photographs of the collage [on the inside front cover] were taken sitting behind the wheel of a car. When I was sitting in the car, I realized I could see all the wheel in front of me and it seemed closer to me. In a single photograph of it, there's something stopping you connecting with the wheel; this is an impossible photograph to take with a single shot, really. But in the collage it's a very close view of something right in front of you. I felt it's a kind of closeness; you seem to be closer to things.

And here's a single photograph of a trolley that I kept brushes and paints on. But I wanted to show more of it in the collage photograph by moving around it. I made these originally for French *Vogue.* French *Vogue* had asked me if I would do 40 pages for them, and I told them I wasn't that interested in fashion, really; I couldn't think of 40 pages. But they said I could do anything, and so I actually did a whole 40 pages of photography and perspective for them. These were part of it.

The last photograph I made before I stopped exploring photography was made out on Pearblossom Highway. First look at the single photograph taken by the side of the road. My version of the same scene is seen in a very different way. Although it looks like one particular view, it's actually about 800 views. And again, I'm walking about continuously. To make this photograph, I went out every morning to Pearblossom Highway (out in Antelope Valley, about an hour's drive), and I had to take a quite big ladder, because, if you think about it and look at the picture carefully, you'll notice that the lettering on the ground, for instance, is actually photographed from above. And for the stop sign—you can see it's actually just one photograph—I was up the ladder right in front of it. Otherwise, from the ground it would appear at an angle. You get a very different way of looking—compare the "Stop Ahead" sign photographed from the road. Actually, when I was doing this, a police helicopter came and circled above, obviously thinking this is very strange—somebody up a ladder next to a stop sign! What is he doing?

I constructed it out there. I'd stick the first pictures down on a board, and then I would look at it and take more photographs. It took about nine days of taking photographs. Then I made the small version, and then I made a second bigger version, which was shown in the Los

Although it looks like one particular view, it's actually about 800 views. And again, I'm walking about continuously.



Pearblossom Hwy., 11-18th April 1986, #2.

Left: *Room 229 Palmdale, Calif. 11th April 1986.* Right: *Room 209 Palmdale, Calif. 16th April 1986.*

Angeles County Art Museum when I had a retrospective. I was told that people looked at that picture longer than at any other picture in the show. But I assume that was because most people know about photography and in some sense understand it. Originally I did this for a story my friend Gregor von Rezzori had written about Humbert Humbert chasing Lolita all over the Southwest, but it was never used. He had described a landscape, and he also described motel rooms being the same and so on. So I also did these two motel rooms, again from a different viewpoint—constructing it and making perspective different.

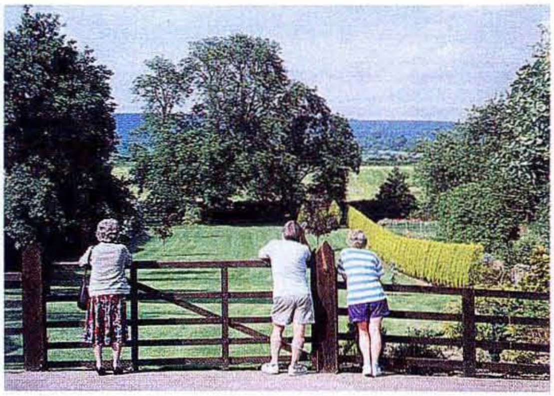
I then gave up photography and spent a lot of time once again painting and in the theater. Theater too, Italian theater and opera, involves making space and illusions of space behind a flat plane. (The English did it another way.) In the *Turandot* we did recently in San Francisco, we made an illusion of a very grand space in quite a small space using perspectives that were not real ones in any sense.

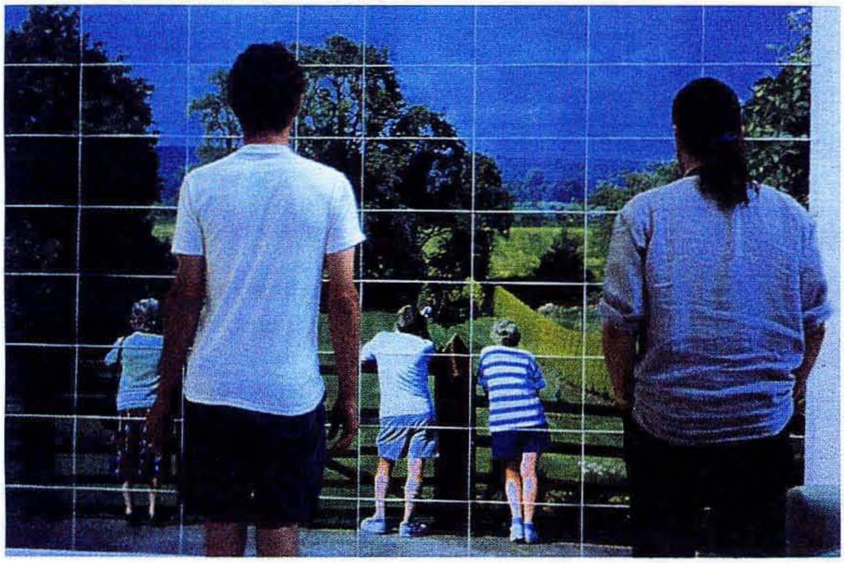
But I do still take pictures sometimes. I took this little snap in Yorkshire last summer, in a town called Coxwold, where Laurence Sterne wrote Tristram Shandy. I was just walking past this lane, and I saw these three people stopped there, so I took the picture because I realized that what you're doing looking at the picture is exactly what they're doing. They seem to be looking at a picture as well. What were they looking at? Well, it was actually a day when all the gardens were open in the village, and so they were simply looking at gardens. I then played with the idea a bit, and I made a great big version; we blew one up on a laser printer at different levels. I stuck them around the studio—put people in front of them as well. It was all quite interesting.

A couple of months ago I was asked to take a photograph for a London newspaper. I put two paintings together in a corner. Then we put a chair there that was done in reverse perspective; and I painted the floor because I thought it then made it look as though we were sitting in the whole picture [page 30]. After that, though, I put a painting in this space, and it takes you a while, I think, to realize what the space is. I made one that I thought was like a family of paintings—mother painting, father painting, little baby painting—but I was fascinated with what was happening. It seemed to me that even in the photograph you are forced to see some other dimension. And I realized that this was, of course, because I was photographing flat surfaces in space, but the thing about them is that they're flat surfaces with something on them, so there's a kind of illusion on that space. And then there's an illusion on the very space you're looking at. When we printed them on our laser printer, we got such terrific vivid color that at first people didn't see them as photographs at all. I think in some pictures you can see how it's set up. As I say, it was all accidental; each painting was begun individually.

Some people keep telling me that I'm wasting my time, really, because the perspective we like is one that makes us more comfortable. Well, that's OK, but I don't think it's always going to

Right: People looking, Coxwold, Yorkshire, July 1993.
Below: In the studio, people looking at people looking.





I was just walking past this lane, and I saw these three people stopped there, so I took the picture because I realized that what you're doing looking at the picture is exactly what they're doing.

be like that. I've also made the point, as well, that there are things going on that are themselves actually quite fascinating and revealing about pictures. Everyone who lives in Los Angeles knows the power of images; we've even seen social disturbance from images. It's coming out of images, the way images are made, and I watch things like that quite carefully myself, because I think what could be happening is that the photograph itself might be losing its veracity. When I say veracity I mean that the photograph has had a unique position in pictures for 150 years in the sense that whatever you see in a photograph or think you recognize, you do tend to think that at some point in time, in space, this object existed. And that's now not necessarily the case. The computer can now recreate things, can draw as well as a photograph. If the photograph loses its veracity, what will that do to us? A very profound change would happen, and I can see a very disturbing side to it; it's like pulling the ground from under us in some ways.

On the other hand, it can also open up enormously our vision of the world. I think somehow or other we need to see more; we need to see bigger spaces. I do think wider perspectives are needed. Anybody who's used a video camera knows that it's a very small section of what you can see that you can see with a video camera. Recently I was in Monument Valley at dawn with a video camera. As the sun was rising in the east, over in the west was a storm coming towards us. And as the light came, you could see the clouds moving, and a great big rainbow appeared, with lightning happening in the middle. Being there was one of the most exciting and thrilling experiences I'd had. When the sun came up and hit the tops of the monuments, I thought it looked as if Moses was going to speak at any moment. But the widest angle of the camera could only see a tiny section of the scene; it wasn't possible to see it all in one.

I think we can look at things in new and fresh ways even with the old camera, even with a video camera. I've been asked if I would design a movie, and I said, well, as far as I can see, the cameraman designs the movie. It's the way it's seen that does it. I must admit I've resisted going into it simply because I know perfectly well there's too many people involved. The theater is enough for me—to have to compromise. Collaboration means compromise; I accept that. I accept it in the theater. But in the theater it's not many people; in movies it's a lot of people, I'm told. And frankly, I can see with new technology, the new little video cameras, you could do an awful lot at home. I'm assuming

Three studio installation shots, November 1993.

I was photographing flat surfaces in space, but the thing about them is that they're flat surfaces with something on them, so there's a kind of illusion on that space.

kids will figure it out. There are new ways you can make movies very cheaply really, because anybody can make a picture as good as you see on television. Television is a bore because the picture itself is very boring. High-definition television has been available for five or six years. The difference is amazing. Why isn't it here? That picture hasn't changed for 30 years. The illusion of sound has changed enormously, but not the picture. People think the picture's fine. I don't. I think it's terrible.

I'm still excited by the possibilities of combining movement and vision to produce new, exciting pictures of the world. But there might be something in just standing in one place and looking from a fixed viewpoint as well. When I draw my dogs I have to set up a piece of paper and just wait until they're still. Right now I'm exploring painting, but I did take some photographs today. So I keep going back to photography, but I tend to think we put too much on it really. I got a marvelous catalog in the mail about two months ago from the British Royal Academy, called "A Golden Age of English Watercolors." The book was of landscapes, mostly of Italy and England. And I loved looking through it and I thought, if these were photographs they would be very boring, actually. Because each of these paintings is a different way of looking, describing, and so on, and each one tells you a great deal. And they make the world more exciting to me, even though these are mostly from the 18th and early 19th centuries. Beautiful things. I don't think photography can do that really. There's a lot it can't do. ☐

The Ultimate Sharper Image Catalog

by Jay Aller

The finished catalog will list an estimated 50 million galaxies and 2 billion stars, or several hundred times more information than is contained in the largest existing data sets.

Remember when you first learned to count, and were so proud that you would count from 1 to 100 for anyone who would listen? But soon you found that it took such a loooong time to reach 100 that the fun went out of the counting. Now imagine tallying more than a billion stars, and not just counting them, but also noting their location, brightness, and other vital statistics— it would take an eternity, or at least the entire life spans of many people. Up until now, "approaching the sky-object classification task manually has been forbidding," to say the least, explained Associate Professor of Astronomy S. George Djorgovski.

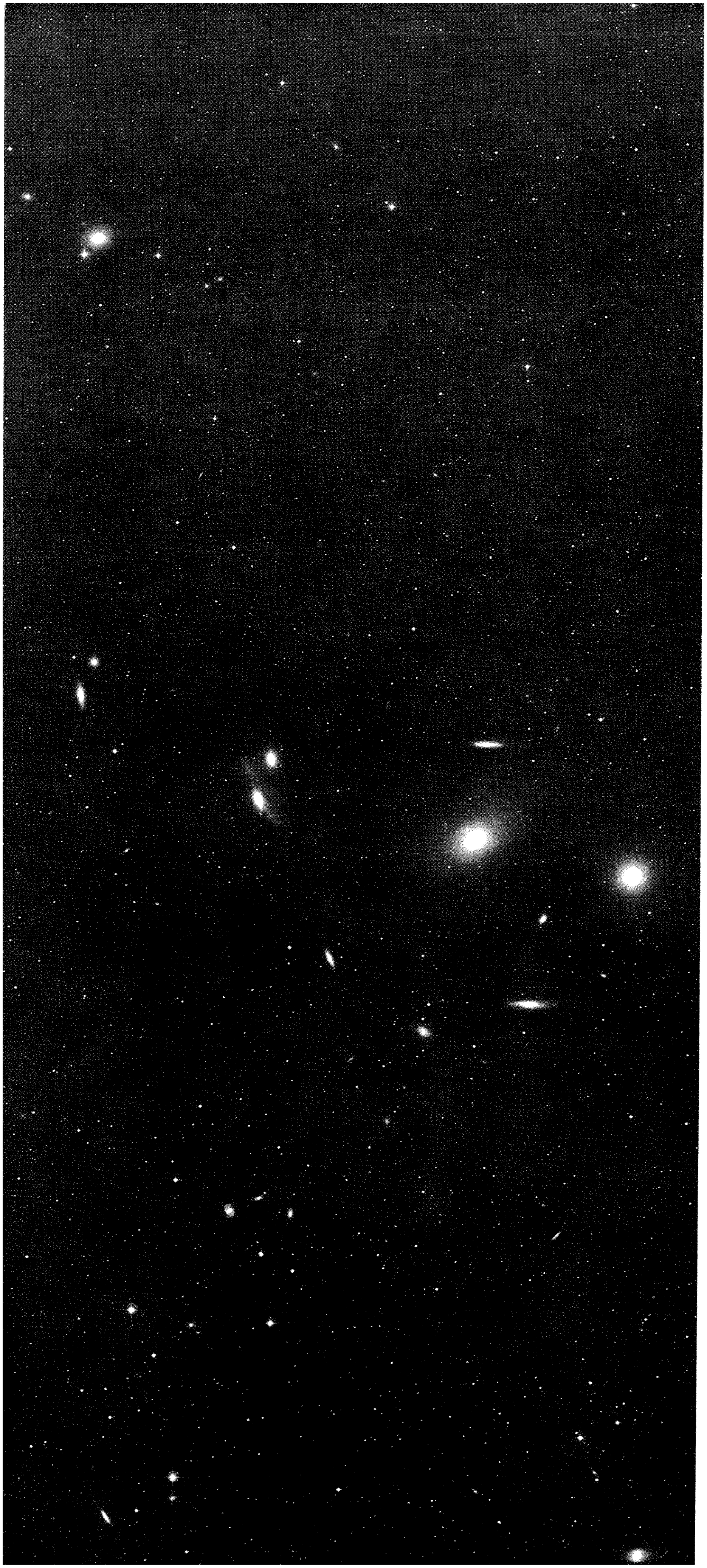
But a new computer software system developed jointly by Djorgovski and Nick Weir (PhD '94), and Usama Fayyad and his colleagues in JPL's Artificial Intelligence group, promises to change all that. Called the Sky Image Cataloging and Analysis Tool, or SKICAT (pronounced "sky-cat") for short, the system is like one of those miracle devices advertised on late-night local television; it's many tools in one: a classifier, a catalog, a database. It stops short of making julienne fries.

SKICAT's powerful new programs enable it to analyze previously unscalable mountains of data, relieving astronomers from the tedious and visually demanding task of classifying objects by poring over a photograph with a magnifying glass, and freeing them to pursue more challenging problems.

In addition to classifying billions of objects much faster than humans could, SKICAT also classifies objects better than humans can, in several ways. For one, it bases classifications on objective criteria, eliminating the biases that creep in when astronomers make judgment calls. It also has a very high correct identification rate of 94 percent. This exceeds the 90 percent necessary for scientific analysis of the data to yield useful results. And, most amazing of all, it is able to detect and categorize objects that appear too faint in the photographs to be recognizable by the human eye.

As SKICAT quickly and accurately classifies the millions of sky objects, it will store them in a new type of astronomical catalog that is revolutionary in both its size and form. The finished catalog will list an estimated 50 million galaxies and 2 billion stars, or several hundred times more information than is contained in the largest existing astronomical data sets. And, unlike other catalogs, which are printed and updated only every few years, the catalog created by SKICAT will always be changing and growing as it is updated with new information. Users will never print the catalog in its entirety, for it would fill roughly 50,000 large volumes, or roughly one floor of Caltech's Millikan Library. Instead they will be able to browse through the billions of entries and sort them by location, magnitude, color, or other properties, all by computer.

The inspiration for the development of SKICAT comes from the Second Palomar Observatory Sky Survey (POSS-II), an effort currently under way to photograph the entire Northern Hemisphere sky. Astronomers are using the Oschin Telescope on Palomar Mountain, a 48-



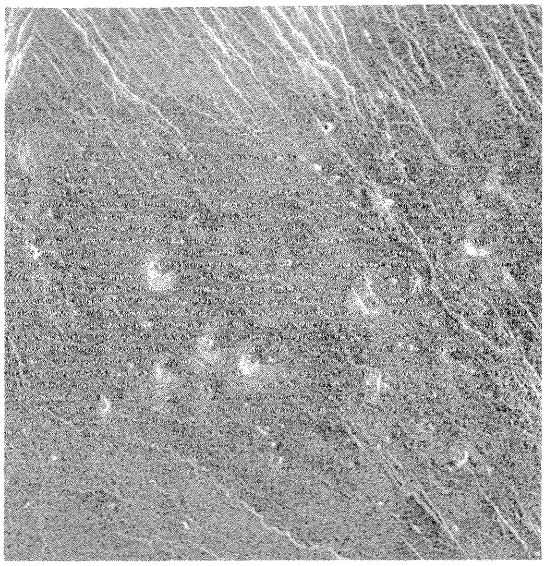
Multiply those 2,700 photographic plates by up to 10 million objects per plate, and it's . . . well, an immense number.

This corner of a POSS-II plate shows a section of the Virgo cluster of galaxies.

inch instrument also used for the original Palomar Observatory Sky Survey (POSS-I) back in the mid-1950s.

POSS-II will pose a pleasant problem for Caltech astronomers—more data than they can deal with. To enable SKICAT to digest the billions of objects, the survey photographs will be converted into a digital form, which will provide a rich vein for the mining of new information. Using the catalog based on POSS-II, astronomers will be able to map the large-scale structure of the universe and the finer structure of our own Milky Way galaxy, study the evolution of galaxies over billions of years, and pick out large numbers of rare or exciting objects, such as high-redshift quasars. But before scientists can even start any of these interesting projects, the raw data of POSS-II must be transformed into a properly classified catalog. Hence, SKICAT.

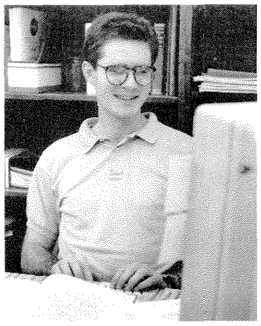
The scientists' present goal is not only to recreate the 1950s sky survey, but also to make a better survey, better both in sensitivity and accessibility. The new survey is able to detect objects approximately 1 to 1.5 magnitudes fainter than the original, due mainly to the new fine-grain emulsion film and the better image quality of the improved telescope optics. These advances also make classification of faint objects as either stars or galaxies possible to at least 1 to 2 magnitudes fainter. The dimmest detectable objects are near 22nd magnitude, or a few million times fainter than can be seen under optimal conditions by the naked eye. The magnitude gain would be larger, except that the sky above Palomar is now much brighter than it was 40



This digital image of Venus from the Magellan spacecraft shows numerous small volcanoes, some of the perhaps million volcanoes on the planet's surface. How many can you spot? See page 35.

years ago, due to the encroaching lights of San Diego County. And since SKICAT can classify sky objects that are too faint for humans to recognize, the resulting catalog will contain a wealth of new information.

Astronomers also want to make data from POSS-II more accessible for themselves and their colleagues. The 1950s sky survey remains a remarkable accomplishment, beautifully preserved in atlases, but much of the data is in practice inaccessible, simply because there is so much of it. Poring over the photographic plates to pick out certain types of objects would require absurd lengths of time. POSS-II will present the same problem. In fact, due to the greater sensitivity of the present survey, there will be even more objects to sort through. POSS-II is expected to photograph four times more objects than were seen in the 1950s. So Djorgovski and Weir turned to Richard Doyle and his colleagues in the Artificial Intelligence Group at JPL for assistance in creating a catalog from the POSS-II data. Working with Usama Fayyad, Weir developed features in SKICAT that enable it not only to process immense data sets, but that also make the resulting catalog easy to use.

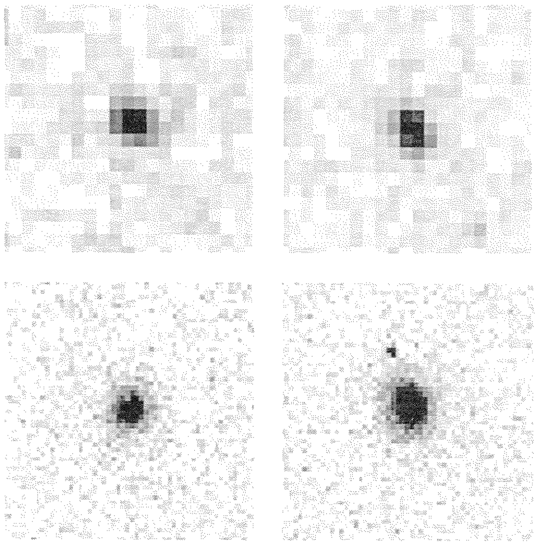


Nick Weir counts galaxies to see how they evolve.

The 48-inch Oschin Telescope can photograph the entire Northern Hemisphere sky with about 900 exposures. Each exposure is made on a square photographic plate, 14 inches on a side, and each plate contains up to 10 million objects. Because the scientists want to see as many faint objects as possible, they expose each plate for up to an hour or two. On a good night, astronomers can expose three plates. To gauge the stars' colors, which are all-important in determining their temperature, each square of sky is photographed not once but three times, in the colors of blue-green, red, and near infrared—wavelengths of 480, 650, and 850 nanometers—so the entire survey will produce some 2,700 photographic plates.

Multiply those 2,700 photographic plates by up to 10 million objects per plate, and it's . . . well, an immense number. Before SKICAT can classify these myriad points of light, the information must be converted into digital form. So Palomar sends the 14-inch square plates to the Space Telescope Science Institute (STScI) in Baltimore, where each plate is converted into an electronic image containing more than 500 million picture elements, or pixels, in a 23,040 by 23,040 grid. To give an idea how sharp this resolution is, a television or computer screen contains only about 250,000 pixels, on a 512 by 512 grid. With each plate digitized into almost 100 billion bits of information, the total amount of data quickly becomes, (dare I say it?), astronomical. To give some idea of how much total data will be collected, the estimated three terabytes (24,000,000,000,000 bits) of information is several hundred times more than that gathered by the Infrared Astronomical Satellite (IRAS), one of the largest collections of data ever.

After STScI records the digitized information on tape, it sends the tapes back to Caltech, where the data are fed into SKICAT, which will automatically process the roughly 24 trillion electronic bits of image data to produce a comprehensive catalog in the form of a computer database con-



The top two images are digitized objects that look virtually identical, from one of the POSS-II plates. Below them are the same two objects, but from higher-resolution CCD frames. SKICAT correctly identified the one on the upper left as a star and the one on the upper right as a galaxy.

Some classification tasks performed in the past over a period of years could now be finished in a few hours with SKICAT.

taining an estimated two billion entries. This analysis is hundreds of times faster than present methods. "Some classification tasks performed in the past over a period of years could now be finished in a few hours with SKICAT," Nick Weir said.

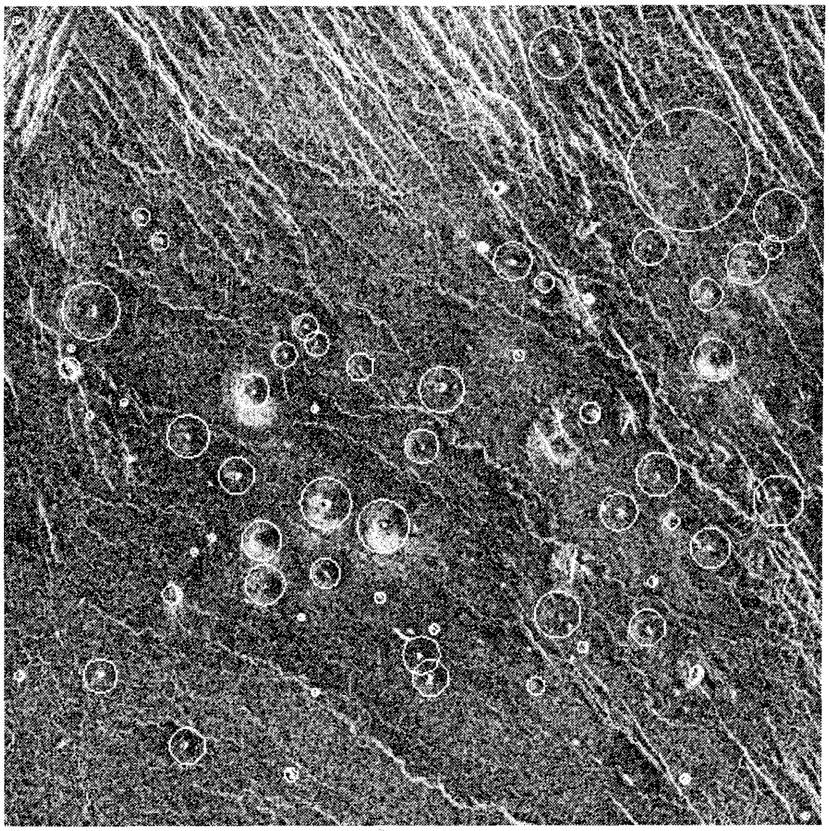
Astronomers estimate that the survey, begun in late 1986, will be 90 to 95 percent complete by late 1996. (All-sky surveys take a long time; the original one, with two-thirds the number of plates of this time around, took eight years—from 1949 to 1957.) Digitizing the data didn't start until 1992, but is proceeding more quickly than the survey itself, so the two tasks should be finished within a year or two of each other. Estimated completion time of the digitizing is roughly 1995 to 1997. The resulting Palomar Northern Sky Catalog (PNSC), a continuously updated database accessible by computer network links, will be an entirely new type of astronomical catalog. A partial release of the PNSC may start in 1995, with a nearly complete release planned for 1998. The resulting data set will not be surpassed in scope for the next decade.

SKICAT's machine intelligence comes into play after the digitized information returns from Baltimore and is fed into the system. SKICAT scans the tapes and uses its built-in artificial intelligence to decide how to classify all the millions of objects. First it must learn how to classify by practicing on a "training set" of objects. This is a set of images taken using a charge-coupled device (CCD) instead of a photographic plate. CCDs are much more sensitive than photographic film to faint light, and give higher-resolution images. The high-resolution objects in the training set can be classified fairly easily, one by one, by a program (which is then checked by an astronomer) into one of four categories: star (s), star with fuzz (sf), galaxy (g), or artifact (long), a sort of catch-all class for objects that don't fit neatly into any of the other three categories. These training data, along with the classes, are fed to SKICAT, which in its computer manner notes the category and examines the properties of each object, for example its sharpness, color, shape, brightness, etc. It then automatically makes up a "decision tree."

At each fork in a branch of the decision tree is a question about some distinguishing feature of an object, such as its diameter, or its core magnitude. For example, SKICAT might "say" to a group of objects, if your diameter is bigger than two arc seconds, branch left, otherwise, branch right. And then one subgroup might reach a fork in the next higher branch, where the system would say, if your core is brighter than 19th magnitude, branch left again, if not, branch right this time. The other subgroup might get the same treatment, or might be sorted according to how elliptical or circular they are. At the end of each branch of the decision tree are clusters of objects correctly grouped into the same category: galaxies, or stars with fuzz, etc.

One of the most powerful features of SKICAT is its ability to automatically create a decision tree, which it does in a matter of seconds, that correctly classifies all of the objects in a data set. The core of the new system includes two machine learning algorithms, called GID3* and O-Btree, which automatically create decision trees based on the training data. SKICAT tests the tree on another set of objects, which have also been classified by hand, but which the computer system hasn't seen before. It categorizes objects correctly about 94 percent of the time. By contrast, the best performance of a commercially available learning algorithm is about 75 percent. The main goal is to automate the process of transferring data from photographic plates to a catalog. But an added benefit is that by training the learning algorithms to predict classes for faint objects, the algorithms can learn to classify objects that are too faint for humans to identify.

SKICAT is not limited to analyzing data about sky objects. It has also been used by scientists at JPL to pick out small volcanoes on the surface of Venus in images sent back by the Magellan spacecraft. The problem, as with the Palomar Sky Survey, was an overwhelming amount of data. Magellan's map of Venus is contained in some 30,000 images stored on more than 100 CD-ROM disks, and planetary scientists estimate that as many as a million small volcanoes less than 15 kilometers in diameter may be scattered over the planet's surface. The Magellan team took an approach much like the one used in the sky-object classification problem, utilizing SKICAT's artificial intelligence, but teaching it with a different training set. Unlike astronomers, who can measure magnitude, area, and other properties of a star or galaxy, planetary geologists do not have a good set of features to measure for the volcanoes. So, much of their work deals with automatically extracting features from pixels in order to rapidly identify all the tiny volcanoes.

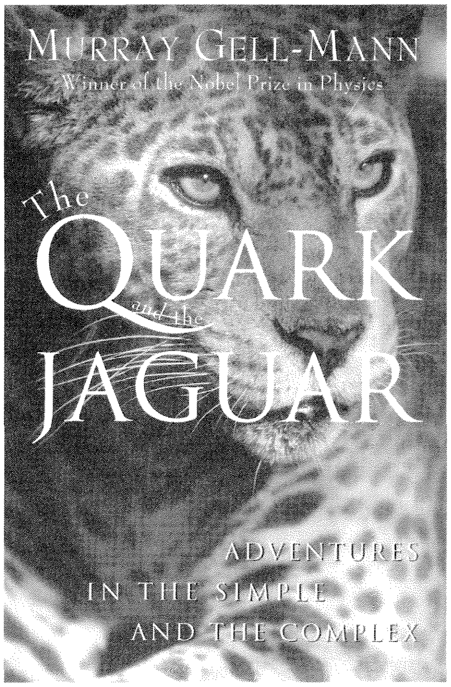


SKICAT was able to pick out 68 small volcanoes on this slice of the Venusian surface.

The Palomar Observatory Sky Survey and the Magellan satellite both present a problem confronted by many scientists as research becomes computerized: a computer's ability to store data has far outpaced our ability to analyze it. SKICAT may prove useful not only for separating stars from galaxies and picking out Venusian volcanoes, but may be applicable to a wide variety of chores. "We view SKICAT as a step toward the development of the next generation of tools for the astronomy of the turn of the century and beyond," Djorgovski said.

Jay Aller has been the science writer in Caltech's Office of Media Relations since 1992. He holds a BS in astronomy from Whitman College and completed the graduate science-writing program at UC Santa Cruz.

Books



W. H. Freeman and Company
$23.95
375 pages

(*The Quark and the Jaguar* will be available in bookstores in April.)

by John Sutherland

This is a difficult book to categorize. The mythical librarian who put *Cancer Ward* in the medical section will probably tear *The Quark and the Jaguar* in two and put one half in "Zoology" and (after toying with "Irish Literature: Joyce") deposit the other in "Physics." The resistance to categorization is, in large part, a willed thing. Murray Gell-Mann despises the "departmentalization of knowledge" that goes on in institutions of higher education and research—even, lamentably, at Caltech. He devotes a longish digression to the incorrigible "reductionism" (and by implication philistinism) of the Caltech intellectual regime:

> Why does so little research in psychology go on at Caltech today? Granted, the school is small and can't do everything. But why so little evolutionary biology? (I sometimes say in jest that a creationist institution could scarcely do less.) Why so little ecology, linguistics, or archaeology? One is led to suspect that these subjects have something in common that puts off most of our faculty.

It is that "something in common" of the subjects disdained by Caltech that nowadays fascinates Gell-Mann. On one level *The Quark and the Jaguar* is a long advertisement for the Institute at Santa Fe, which he founded in 1984 and where he and a band of carefully selected MacArthurite geniuses cogitate on "complex adaptive systems, functioning in such diverse processes as the origin of life on Earth, biological evolution, the behavior of organisms in ecological systems, the operation of the mammalian immune system, learning and thinking in animals (including human beings), the evolution of human societies, the behavior of investors in financial markets, and the use of computer software and/or hardware designed to evolve strategies or to make predictions based on past observations."

It is clear that Gell-Mann regards the interdisciplinary seminars at Santa Fe as a kind of ideal superstructure to Caltech's foundation. A lot of prophets have gone into the desert and come up with a plan for the salvation of mankind (this is not hyperbolic or intended as sarcasm: *The Quark and the Jaguar* concludes with a program for the human race in the face of its imminent self-destruction). But this description does not fit Gell-Mann. Nor is he what he superficially resembles—a 19th-century totalizing sage crudifying science into popular ideology (Ernst Haeckel, for instance, with his genealogical "tree of life forms" with its monera at the roots and man in the top branches; or Herbert Spencer with his pan-Darwinism). Even when discussing subjects as remote from theoretical physics as the territorial behavior of big cats in the rain forest, Gell-Mann is, first and foremost, a fastidiously strenuous scientist. In his 40 years at Caltech as one of the world's most distinguished exponents of quantum mechanics, he has, as he demonstrates, articulated a definition of "complexity" (and its correlative "plectic," simplicity), which he now feels he can apply to other fields—particularly that of complex adaptive systems. Wall Street is a CAS, Madonna is a CAS, the TB bacterium is a CAS. It is magnificent, although many of his colleagues may think it is not science.

The Quark and the Jaguar is at least three kinds of book. It opens with a short introspective prelude made up of a number of autobiographical snapshots. In terms of reader enjoyment this is the most attractive section. Gell-Mann reminisces about not having seen a jaguar in the wild, but having had a dramatic encounter with a jaguarundi (the precise distinction of species is very characteristic) in a northeastern corner of Guatemala. He reminisces about himself as "a curious child" and recalls early intellectual adventures with his brother in prewar New York. He confides some touching recollections of his first-generation American father and hints at youthful rebellions that still trouble him. There is a vivid vignette of himself, newly married in 1956, driving with his wife through the Tejon Pass in their Hillman Minx and coming upon 11 California condors feasting on a dead calf.

These early pages make one wish that the author had been prevailed on to write an autobiography proper. But the central section of *The Quark and the Jaguar*, rather surprisingly, comprises a series of briskly technical expositions on plectics, coarse-graining, Algorithmic Information Content (a central element in Gell-Mann's definition of complexity), randomness, chaos theory, Zipf's law, Grand Unified Theory, superstring theory, and "Quantum Mechanics and the Classical Approximation." It is aimed at the intelligent layperson, and the overall effect is that of an extraordinarily bracing series of Watson Lectures (one recalls, incidentally, that Gell-Mann is one of that select band of Watson lecturers for whom Beckman Auditorium is too small). Speaking as a layperson who was baffled to the point of fury by *A Brief History of Time*, I found this pedagogic section of the book lucid yet never condescending. Richard Feynman, one is told, habitually brushed off requests to explain the mysteries of his subject to nonspecialists with the good-natured jest: "If you could understand it, you'd win the Nobel Prize as well." Gell-Mann, by contrast, gives the nonspecialist at least a glimpse of the inner workings of theoretical physics. One comes away feeling a cleverer person.

It is aimed at the intelligent layperson, and the overall effect is that of an extraordinarily bracing series of Watson Lectures.

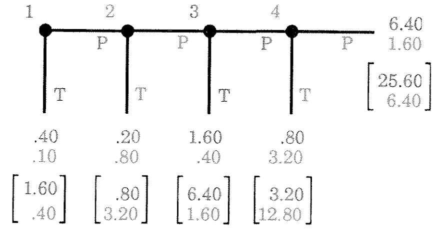
The final section of *The Quark and the Jaguar* picks up themes publicized in Caltech's centenary "Sustainable World" conference. Gell-Mann makes a passionate and rational plea for conservation, particularly in the tropics, where the bulk of the planet's surviving biological complex adaptive systems are stored. A professional theorist, he believes that the kind of interdisciplinary theoretic interventions secreted at places like Santa Fe may offer "some kind of headlight, even a flickering one, to help avoid some of the worst disasters." Since the disasters he foresees will involve, among other things, the extinction of the human species, one hopes he is right.

One reads *The Quark and the Jaguar* with the sense that only Caltech could have produced Murray Gell-Mann, and not even Caltech can hold him. No institution organized into "divisions" can satisfy his Baconian ambition to make "all knowledge my province." This is an outstandingly brilliant man's book and will doubtless have a huge impact. It must be said, however, that for all its author's brilliance, the editorial hand could have been applied to better purpose. The overall shape of the discourse seems in places to have been improvised. Stretches of the text read as if they had been dictated and never polished. The author's love of name dropping ("Tom" Kuhn, "Steve" Hawking) and his habit of attaching a kind of *Who's Who* list of honors to even cursory mentions of his distinguished friends should have been curtailed. The final sections of the book—which, as Gell-Mann's Cassandra-call to humanity, require a ringing eloquence—seem to have been phoned in from Rio. It is a pity that with a book of this importance the publisher could not have contrived to produce something worthy of the author's mind.

John Sutherland is the Lord Northcliffe Professor of English at University College London. From 1984 to 1992 he was professor of literature at Caltech and returned last fall term as a Fairchild Distinguished Scholar.

Lab Notes

Table stakes: Rita's and Bruce's possible payoffs during the four-move centipede game. Each turn is symbolized by a numbered dot, and the lines labeled P lead to the next turn. The lines labeled T give the payoffs at any turn if a player takes the money. The top row of numbers (in red) is Rita's pot, and the bottom row (in blue) is Bruce's. (The numbers in brackets are for the high-stakes version.) If both players pass for the entire game, they get the payoffs shown in the rightmost column.

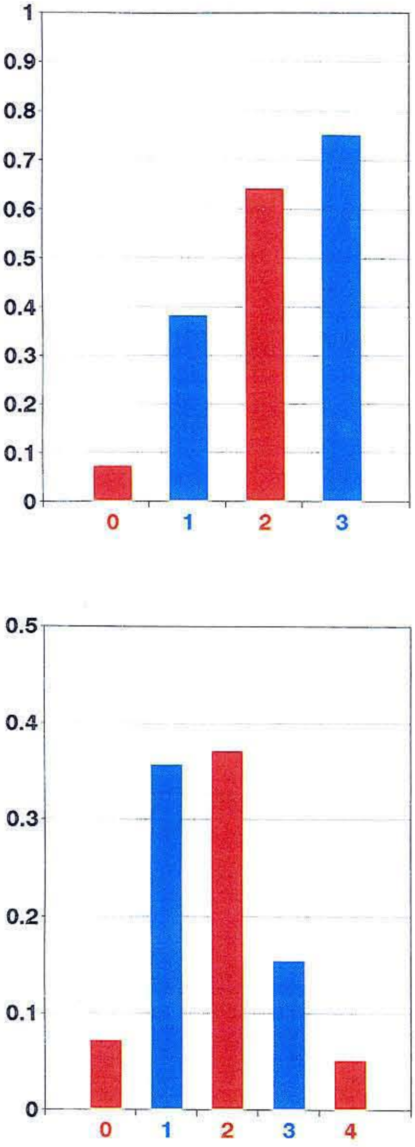


Do Unto Others Before They Do Unto You

"The best judge in a beauty contest is the one who always picks the average score," according to Thomas Palfrey (PhD '81), professor of economics and political science. In other words, if a particular contestant winds up with a score of 8.7 after all the judges' votes are tallied, then the judge who actually wrote that score on his or her own scorecard had the best feel for how the contestants would rank. A game called the centipede is another exercise in second-guessing, and it works like this: Rita and Bruce start with two pots of money, one of which is four times bigger than the other. Rita moves first. If she takes the money, the game's over—she gets the big pot and Bruce gets the small one. If she passes, each pot doubles. Now Bruce gets the opportunity to take the larger one, and so on. If the game survives two such innings—four passes in all—it ends anyway and Rita gets the big bucks. (The original version ran for 100 passes, hence the name.)

Rita and Bruce both know that the game will end after four passes, and both know how big each pot will be at every step. Game theorists call this a game of "perfect information," since both players can see all the way to the end and plan accordingly. The winning strategy, says game theory, is simplicity itself—take the money at your very first opportunity. Assuming that the small pot began with a dime and the large one with forty cents, as shown in the chart at left, the logic runs as follows: If Bruce passes on turn four, he knows that Rita will get $6.40 and he'll get $1.60; if he takes the money, he'll get $3.20 and leave Rita 80 cents. Thus Bruce should take the money. But Rita knows this, too, so therefore she should freeze Bruce out and take the money on turn three, awarding herself $1.60 and Bruce a lousy 40 cents. And Bruce knows that Rita knows, so he should preemptively grab the dough on turn two, winning 80 cents and sticking Rita with 20 cents. And finally, Rita can see that Bruce will stiff her if she passes, so she should take the 40 cents offered her on the very first turn.

But that's a pretty low-reward strategy, and it's not what I'd do. It apparently isn't what almost anybody else would do, either. Professor of Political Science Richard McKelvey, Palfrey, and graduate student Mark Fey (BS '90) have



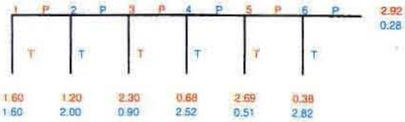
As the game progresses, players are more likely to run with the money. The top graph shows the probability (vertical axis, with "1" being certainty) of a player taking the pot after the number of passes shown on the horizontal axis. (In other words, zero passes is Red's first turn.) The bottom graph shows the relative frequency (vertical axis) with which games ended after the number of passes shown on the horizontal axis. Thus, most games ended after one or two passes.

studied the centipede, and only one of their 138 experimental subjects ran with the loot at every opportunity. In fact, most people passed on their first move. From then on, the probability of a player taking increased with every turn. Even so, a significant number of games went the full four moves, and nine subjects even passed on turn four!

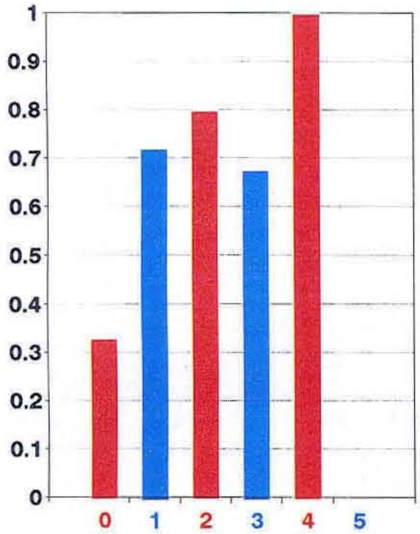
The games were played in Caltech's Laboratory for Experimental Economics and Political Science, by Caltech and Pasadena City College undergrads. The players communicated through a network of personal computers that also recorded their moves and calculated their winnings. At evening's end, the participants got paid—in real cash. Starting with 40 cents in the pot doesn't imply trivial stakes—one player walked away with $75.00 for less than an hour's work. (If this person had known game theory, he or she would only have netted $7.00—40 cents times 10 games, plus $3.00 for showing up.) The players were designated as either Red (the first mover) or Blue at the beginning of the session, and kept their color for the duration. In order to prevent anybody from capitalizing on what they learned about an opponent, each Red played exactly one match with every Blue, and vice versa, and no player participated in more than one of the seven sessions. Some sessions played a six-move centipede, or had a more generous scale of payoffs—variations designed to encourage greed—but 10 to 20 percent of these games still went the distance.

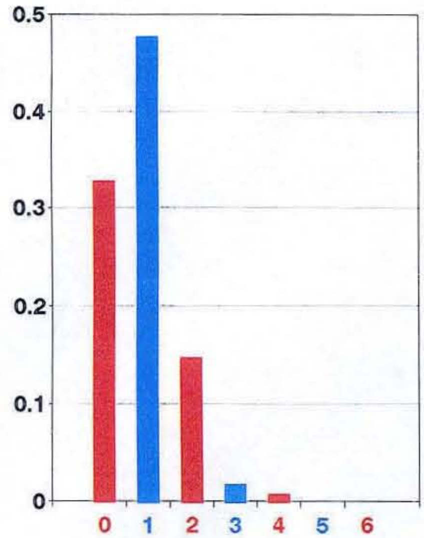
The remaining 128 subjects showed a spread of behavior between the one grabby guy and the nine passive people. Most participants appeared to be learning on the job. The later games in a session tended to be over more quickly, as people who'd been burned before pounced sooner. But some individuals appeared to be playing haphazardly, with no clear pattern emerging from their behavior over the course of a session.

Since game theory's prediction was a colossal failure, some other factor was obviously at work. The theory assumes that humanity has the predatory instincts of a leveraged-buyout artist—one maximizes one's own rewards and the heck with the other guy. This is called the "rational" strategy. But someone whose moral development has progressed beyond that of the shark (or who "just wants to bankrupt the Social Sciences department," as Palfrey remarks) would realize that by passing, both players are better off because both pots get bigger. And if these people exist (even as an endangered species) an intelligent opportunist would realize that the way to beat the game is to make like an altruist and pass in the first inning, then



Above: The six-move constant-sum centipede game's payoff structure. Below: Conditional probabilities (top) and frequencies (bottom) for the six-move, constant-sum game ending, again plotted versus the number of passes. This data is from PCC students—Techers "took" so quickly that there are very few late-round data points for them.





move in for the kill and claim a twice-doubled pot in the second. Thus, if you believe your opponent may be altruistic, it's in your selfish best interest to pretend that you are too, at least for a little while. This will even work if your opponent is selfish—if, by passing, you can make the other person believe that *you* are the altruist, that person may decide to play you for a sucker and pass the pot back once, in order to burn you on the following turn.

McKelvey and Palfrey developed a mathematical model for the game in which they assumed that a small percentage of the players were altruists. The rest were not, but knew that there was a smattering of angels in their number and played accordingly. The model also included a random-error function to mimic the small probability that a player might hit the wrong key, forget what turn it was, or otherwise mess up. Computer simulations based on this model, and run on the Caltech/JPL Cray X-MP supercomputer, plotted the probability of the game ending in a "take" at any given turn. The predictions agreed with the actual games very nicely.

Unfortunately, this explanation didn't survive a second set of experiments designed to test it. This time, the two pots started out equal, and if Red passed, one-fourth of the money in one pot was moved to the other. After each succeeding pass, the larger pot absorbed one-fourth of the smaller one, as in the chart at left. Here, the logic of the game dictates that saints as well as swine should take the money on the first move. The combined pots don't grow, and the initial 50-50 distribution is certainly the most equitable outcome. But again, over half of the games played went beyond the first move. The rate at which people chose to take thereafter, however, increased very rapidly, and all the games ended early. Nine sessions of this game (including three at the University of Iowa) were played, using six- and ten-move centipedes in order to let the smaller pot *really* dwindle.

With altruism joining capitalism in the dustbin of history, how can one construct a model in which people often pass, even when it doesn't seem to be in their selfish best interest to do so? Fey, McKelvey, and Palfrey have come up with one, dubbed "quantal response." (The term "quantal" comes to the social sciences by way of biology, where it describes a yes-or-no, all-or-nothing response—a skin test for allergies, for instance. Such models are widely applied to discrete-choice situations—what kind of car to buy, for example—but had not been combined with experimental game theory before.) Folks make mistakes, says quantal-response equilibrium, but folks know that everyone else makes mistakes too. Everyone intends to take the money, but every so often, someone does something dumb, like the "Wheel of Fortune" contestants who pick letters that have already been used. (Of course, the more costly a mistake—passing in a late inning, in this case—the less likely someone is to make it.) And since some people are more error-prone than others, a player may elect to pass at first, in hopes of having drawn an inept opponent. But as the session progresses, the odds of passing drop—the klutzes begin to wise up, so there are fewer of them to prey on.

When this model was run, it agreed with both sets of experiments. "You don't have to hypothesize altruists or other extraneous factors," says Palfrey. "Different levels of skill and a bit of noise will give the same results." These models are of more than academic interest—speculative bubbles, such as occurred in real estate in the 1980s, are real-world examples of the centipede game. If all investors behaved with the perfect rationality of game theory, they'd anticipate that the bubble would eventually burst. The bubble would never grow in the first place—nobody would buy in, for fear of being left holding the bag. In real life, of course, the winners are the ones who guess best what the average person is guessing, and bail out just before everyone else does. "A lot of game theory has been built on introspection by some very smart people, but introspection only gets you so far. These sorts of adjustments to make the model more realistic wouldn't happen if there weren't experiments. Now we know that a little bit of error goes a long way." ☐ —*DS*

Letters

I just finished re-reading Jay Labinger's review of Collins and Pinch's *The Golem* in the Fall *E&S*. While Labinger agrees with the authors that better sensitivity to how social factors affect scientific practice would be a good thing, in my opinion his review subtly distorts the book so as to make it seem quite a bit more extreme and polemic than it is. Whether or not "sociology of scientific knowledge" in general claims that knowledge is created by social factors, Collins and Pinch do not. Purposely or not, Labinger presents *The Golem* almost as part of the movement that claims that there is no truth, merely claims fought over by more or less powerful groups, a bit of blatant nonsense obviously antithetical to science. For example, he states that "in their view . . . choosing to favor Pons and Fleischmann's . . . results . . . can only be based, ultimately, on whether we believe in cold fusion. A dispassionate assessment of the experiments cannot be reached." This is almost a caricature of their detailed description of how the various scientific communities and subcommunities handled Pons and Fleischmann's reported results.

In a similar vein, Labinger appears to be saying that Collins and Pinch claim that it is never possible to assess the validity of an experiment without a priori acceptance of a theory. But they do not. Rather, they simply point out that when the appropriate range of outcomes of an experiment is not known in advance, some other criteria must, logically, be used to decide the validity of the experiment. These may be technical or nontechnical, and Collins and Pinch document several nontechnical reasons actually given by scientists for believing or disbelieving the results of various gravity-wave experiments.

The Golem seems to me to be presenting a much more reasonable picture of the actual doing of science, and as a practicing scientist I believe that scientists and the institution of science would be better off if more scientists read and understood it. Labinger's review would not lead many to read it, so I would like to present a different point of view.

If we simply examine the facts of what scientists do, not the theoretical or philosopical redescription of those facts, perhaps the most fundamental and obvious fact is that science is practiced by scientists. Scientists do experiments; scientists interpret experiments; scientists negotiate about how experiments ought to be interpreted; scientists agree that an experiment proves or disproves a theory. Saying "Experiment E proves theory T" is a shorthand description, albeit a useful one, one that leaves out the scientists, or the particular scientific community, that agrees that E proves T. This does *not* mean that theories cannot be verified, or disproved, or that all theories are equally valid or proper, etc. It is simply a reminder of the fact (and it is a fact, not a theory or an opinion) that the members of a particular scientific community agree, or disagree. Agreeing and disagreeing are done by persons, not theories.

Doing science, rather than something else, means committing one's self to negotiating about theories, experiments, and interpretations based on precision, rigor, and systematic investigation, rather than other things. It does not, and cannot, eliminate the necessity for judgment and skill. The exercise of that judgment and skill in no way invalidates the science. When Eddington chose to not use the Sobral results in evaluating the photos attempting to confirm general relativity (Collins and Pinch, p. 51), his behavior was not arbitrary, highhanded, or capricious; he was exercising his professional judgment that a "systematic error" had occurred. This kind of judgment is exercised in just about any experiment. No real data falls perfectly on a mathematical curve. "Experimental error" is a universally used concept, and a key part of a scientist's training is learning where, when, and how to use it.

When someone presents a result that, if accepted, would imply a tremendous fundamental change in basic theories, scientists quite naturally and appropriately seek to explain the results in another way, not for any of the illegitimate reasons often ascribed to them but simply because they are doing what scientists do: seeking the most parsimonious account of all the facts. Questioning whether the procedure reported was actually what was done, and whether the experimenter has the necessary kind and degree of skills, is an appropriate search for an explanation of the facts. Sometimes, perhaps due to lack of detailed information (as in the case of Pons and Fleischmann) it is impossible to say what went wrong, but in the judgment of respected members of that scientific community "something must have," and in this case the standing and credibility of those presenting the unusual results become important. None of this is illegitimate, inappropriate, or unscientific. It is simply how groups of human beings negotiate differences. It only seems to conflict with "the scientific method" because we are so used to language such as "this experiment proves conclusively that X" that we have taken it literally, and confused this partial description of the facts with the facts themselves.

The Golem is basically a depiction of this phenomenon, and a detailed reminder of the situation described above. I would recommend it to anyone, scientist or not, who wants or needs a better understanding of what science is, and is not.

H. Joel Jeffrey, BS '69
Professor of Computer Science
Northern Illinois University

Jay Labinger responds:

I agree with almost everything Jeffrey says: 1) *The Golem* effectively demonstrates that doing science is intimately bound up with social activities; 2) it would be very valuable for scientists to read it (I thought I said so in the review!); and 3) my review might well leave the impression that Collins and Pinch take an "extreme and polemic" position. I would only disagree that the last is in any way a distortion, and that they do indeed claim that knowledge is created by social factors. Page 138: "Science works the way it does, not because of any absolute constraint from Nature, but because we make our science the way that we do." Still stronger versions appear in Collins's earlier work: "explanations should be developed within the assumption that the real world does not affect what the scientist believes about it . . ." and "The natural world in no way constrains what is believed to be."

Jeffrey feels that scientists should pay attention to the issues that Collins and Pinch and other science observers address, as that would benefit the practice of science. I would go further and argue that there is a huge agenda that begs for collaboration between scientists and science observers. Why isn't that happening? Pinch wonders elsewhere (in connection with cold fusion), "Despite all our work and understanding of controversies, what has our input been? Zilch. Our message is clearly not getting through, and that is the most depressing thing of all." I suggest that their emphasis on how much social factors determine knowledge would strike most scientists as a severely distorted picture of how science really works. The barrier that keeps their message from getting through is one they have done much to help build. It is ironic that Collins and Pinch and their colleagues place so much weight on the roles of negotiation and consensus-building within science, yet seem to have little interest in moderating their own positions in order to enlist scientists in a true dialogue. If my review accentuated the negative a bit too much, chalk it up to the hope of encouraging *both* sides to move.

Obituaries

Robert P. Dilworth

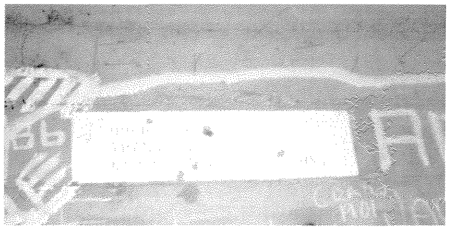
Robert Dilworth, professor of mathematics, emeritus, a member of the faculty since 1944, died October 29, 1993. Born in Southern California in 1914, Dilworth never left the area for very long. He earned his BS in mathematics at Caltech in 1936 and his PhD in 1939. After a few years as Sterling Research Fellow and instructor at Yale, he returned to Caltech as assistant professor in 1943, becoming associate professor in 1945, and full professor in 1951. He retired in 1982. Dilworth was known for his work in the fields of lattice theory and universal algebra.

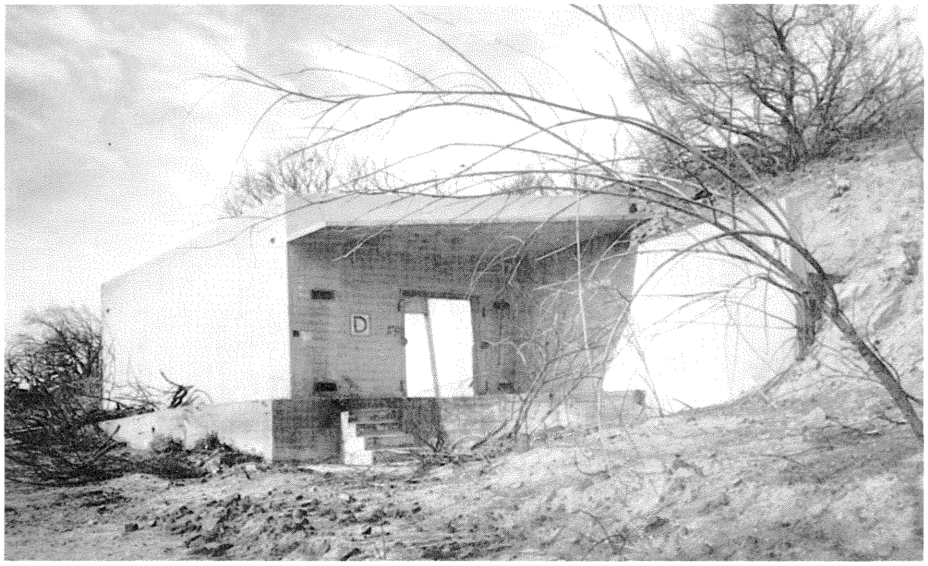
Charles Newton

Chuck Newton, who came to Caltech in 1948 as special assistant to Lee DuBridge, died March 2, 1994. Born in Kentucky in 1907, Newton earned his PhB from the University of Chicago in 1933, then worked for a few years as a newspaper feature writer in Chicago and as radio director at the University of Chicago. From 1941 to 1946 he served as head of special publications and photography at the MIT Radiation Laboratory, where he met DuBridge, whom he was shortly to follow to the West Coast. At Caltech Newton wore many hats, being in charge, at various times from 1948 to 1966, of public relations, high school relations, publications, and development. He founded the Industrial Associates in 1951, and he also taught, as a lecturer in English in 1955, 1960–62, and 1966–75.

Jan L. A. van de Snepscheut

Jan L. A. van de Snepscheut, associate professor of computer science and executive officer for computer science, died February 23, 1994. Born in the Netherlands in 1953, van de Snepscheut earned his MSc in electrical engineering from the Eindhoven University of Technology in 1977 and his PhD in computing science in 1983. He first taught at Caltech as a visiting assistant professor in 1983–84, then returned to the Netherlands, where he was professor in the Department of Mathematics and Computing Science at Groningen University from 1984 to 1989. Van de Snepscheut returned to Caltech as associate professor in the fall of 1989, and became executive officer in 1992.





Below: Magazine 78, which held high explosives for the Eaton Canyon Project during World War II, stands revealed after the fires.
Right: The site wasn't completely forgotten, as a graffiti-scarred stencil on Magazine 88 attests; the "transient" referred to was not one of the recent visitors but a floating crew member, usually a munitions handler.

Watson Lectures

The Earnest C. Watson Lecture Series for the balance of the academic year includes: *April 6:* From Biological to Machine Vision—Pietro Perona, assistant professor of electrical engineeering; *April 20:* Galileo: Enroute to Jupiter—Torrence V. Johnson, project scientist, Project Galileo, JPL; *May 11:* Early Results from the Keck Telescope—B. Thomas Soifer, professor of physics; *May 25:* Farewell to the Party of Lincoln: African-American Politics in Depression-Era Los Angeles—Douglas Flamming, assistant professor of history. All lectures are at 8:00 p.m. in the Beckman Auditorium, and admission is free.

The Reemergence of Things Past

The fires that ravaged Altadena and the San Gabriel Mountains last October exposed some pieces of Caltech history that had been overgrown with brush and hidden from view for decades. In January Kenton MacDavid discovered five concrete storage magazines for high explosives still standing in the hills above Eaton Canyon, where they had remained for more than 50 years, although the other buildings of the Eaton Canyon Project had long ago been bulldozed for residential development.

The project, directed by Charles Lauritsen, was part of Caltech's contribution to the war effort—making rockets (*E&S*, Spring 1991), which involved a large fraction of Caltech's scientists. Also known as Physics 3, the Eaton Canyon Project handled solid-fuel rocket design and production. MacDavid, who worked as a technician and crew chief on the project in 1943–45, and then at JPL for more than 40 years, is researching the project's history and welcomes stories and information from other participants. He can be reached at 818-794-2919.

MacDavid is puzzled about one thing: although he has found five magazines, maps of the project obtained from the Caltech Archives show only four. Does anyone remember?

Honors and Awards

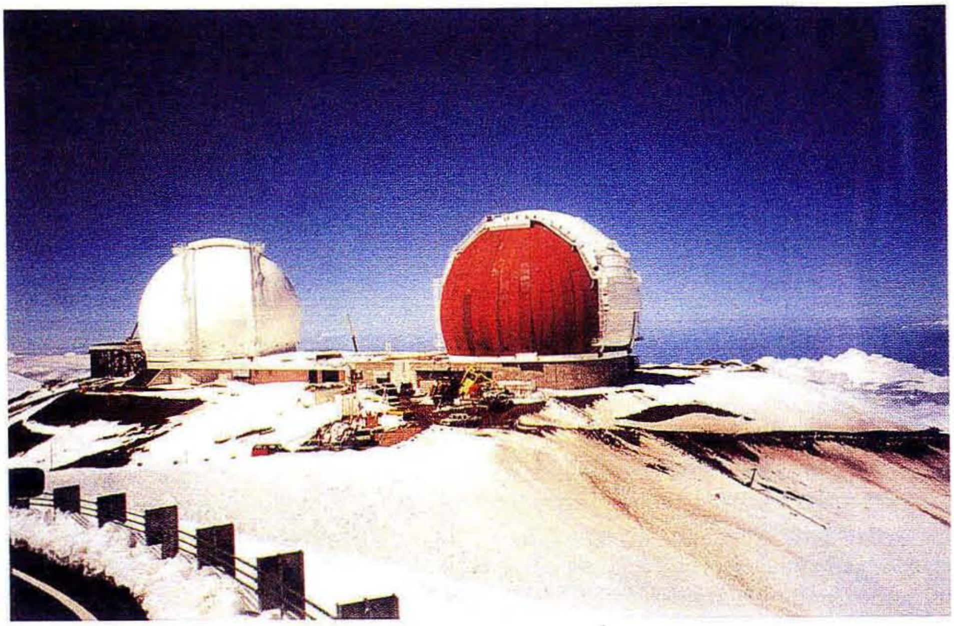
Herbert Keller, professor of applied mathematics, will receive the 1994 Theodore von Kármán Prize, presented annually by the Society of Industrial and Applied Mathematics, for his advances in numerical methods for solving important problems in mechanics.

Gilles Laurent, assistant professor of biology and computational and neural systems, has been selected as a 1993 Presidential Faculty Fellow, a distinction bestowed by the U.S. president and the National Science Foundation "to recognize the scholarly achievements and potential of the nation's most outstanding science and engineering faculty [only 15 of each per year, nationwide] members early in their careers."

Ray Owen, professor of biology, emeritus, has been awarded the Thomas Hunt Morgan Medal, for "his lifelong contribution to genetics, not only as a discoverer of key principles in immunogenetics, but also as a teacher" by the Genetics Society of America. Owen's discovery of immunological tolerance made tissue transplantation possible.

William Pickering, professor of electrical engineering, emeritus, and former director of JPL, has been named cowinner of the prestigious Japan Prize, for his work in aerospace technology. Pickering shares the honor with Swedish biochemist Arvid Carlsson. Pickering is also the first recipient of the François-Xavier Bagnoud Aerospace Prize, sponsored by the European-based Association François-Xavier Bagnoud.

John Roberts, Institute Professor of Chemistry, Emeritus, is the American Chemical Society's 1994 recipient of the Arthur C. Cope Award, a tribute to the "crucial role" he has played for more than four decades "in the explosive growth of physical organic chemistry," a development that has "profoundly influenced the way we currently think about and teach organic chemistry."

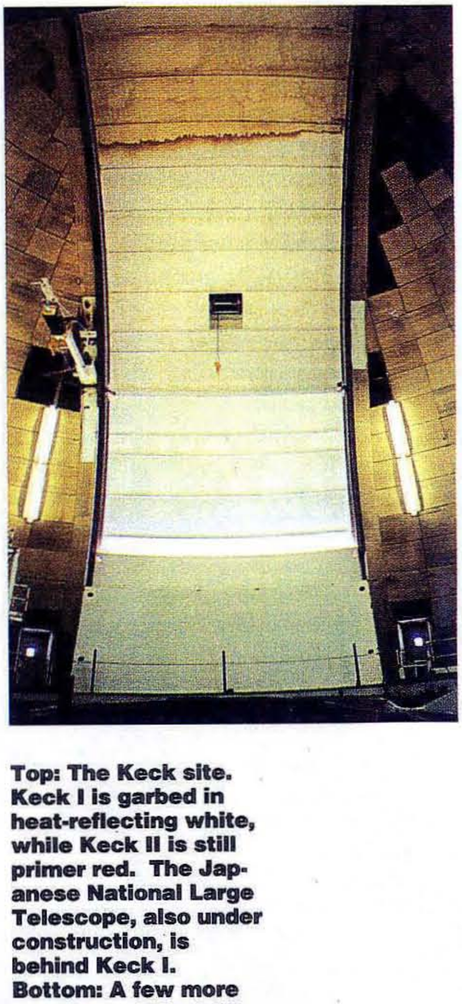


Ad Astra per Aspera

Blizzards on Mauna Kea haven't stopped construction of Keck II, the second of twin 10-meter telescopes collectively known as the W. M. Keck Observatory. Mauna Kea is on the Big Island of Hawaii, but winter at 13,600 feet is brutal anywhere. A recent storm dropped two feet of snow that hurricane-force winds piled into eight-foot drifts. The storm caused minor damage within the dome, and toppled a crane outside. However, these setbacks shouldn't delay the telescope's completion by 1996.

Keck I, now fully operational, is the largest optical and infrared telescope in the world. When Keck II is completed, the two telescopes can be aimed independently at different targets. But they can also be focused on the same point, and their light combined to create a single instrument with the resolving power of an 85-meter mirror—the distance between the two telescopes.

The Keck Observatory is a joint project of Caltech and the University of California.



Top: The Keck site. Keck I is garbed in heat-reflecting white, while Keck II is still primer red. The Japanese National Large Telescope, also under construction, is behind Keck I. Bottom: A few more panels, and Keck II's dome's inner wall will be ready for the next step—a five-inch layer of foam insulation.

Lee DuBridge, then Caltech president, strolls with undergraduates along the colonnade between Ricketts and Fleming houses in 1957. DuBridge died in January at the age of 92.

Engineering & Science

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