California Institute of Technology

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IN THIS ISSUE

At the Bonneville Salt Flats, all sorts of strange and wonderful machines wait in line to break the speed limit and perhaps even a world record. A motorcycle whose streamlined body shape was designed and tested at Caltech did just that last fall. **To see this remarkable bike, turn to page 32.**

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On the cover: The taxi stand outside Grand Central Station in midtown Manhattan is really a laboratory for some experimental economists. Their findings blend psychology and economics to explain human behavior much better than standard economic theories do. The story begins on page 10.

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It can't leap school buses the way Evel Knievel did in his prime, but an eight-wheeled prototype suspension system strutted its stuff by climbing over Brownie Troop 51 of Monrovia, California, during JPL's Kid's Day last summer.

Random Walk

How's My Driving? Call 1-800-NASA-JPL

The only likely disappointment is Sojourner's top speed—a blistering 16 inches per minute.

If everything goes according to plan, a six-wheeled all-terrain vehicle named Sojourner will start exploring the surface of Mars this summer. Brian Cooper, a member of the technical staff at Caltech's Jet Propulsion Laboratory, will be sitting in the driver's seat. Well, sort of. Sojourner is the size of a microwave oven, and Cooper will be sitting at a computer in his JPL office. Cooper has created the user interfaces for all the robotic vehicles leading up to Sojourner since 1985, and he drove them during testing, so he was the natural choice for this job.

Sojourner is any kid's Christmas wish. It's solar powered, so it never needs batteries (although it has some for nighttime operation). And it's a killer on the obstacle course. Each wheel has an independent motor in the hub, and any one wheel can lift the rover's full weight. The suspension system is flexible enough that a wheel can drive over things larger than its own diameter—Sojourner can even climb stairs! (Not that NASA expects to find any.) The only likely disappointment is Sojourner's top speed—a blistering 16 inches per minute. And speaking of toys, Cooper has the world's coolest sandbox, too—while the real Sojourner is en route to Mars, Cooper practices weekly with an identical copy, which he drives around in the Space Flight Operations Facility, in a large room that has been converted into a Mars-scape of sand and rocks. The other team members rearrange the rocks out of Cooper's view, and he then has to negotiate the terrain using the same data he'd get from Mars.

So here's the plan: JPL's Mars Pathfinder mission hits Mars at 10:00 a.m. Pasadena time on the Fourth of July, and "hits" is the operative word. Cushioned by its air bags and given some extra spring by the red planet's low atmospheric density, Pathfinder will make several multi-story bounces, perhaps rolling a kilometer or more before coming to rest. By 1:00 p.m., the air bags will have deflated, with three of the tetrahedral spacecraft's four faces then opening like a time-lapse movie of a rose blooming. The lander rights itself (the petals open with enough force that they can actually lever the lander out from between rocks, should it be unlucky enough to get wedged), and its stereo camera makes a sweep of its surroundings. At 6:25 and 6:30, two metal ramps unroll in opposite directions from one of the petals, like red carpets preceding the queen's entrance. The queen in this case is Sojourner, which spent the trip bolted to the petal. Meanwhile, back at the Lab, Cooper and the flight team will be studying the panorama to decide which ramp Sojourner should drive down. Sojourner will do so at 8:20 p.m., posing for a press photo at 9:30. Later, the rover will use its own stereo camera to see how the lander fared. These photos, and the other images the mission sends back, will be posted on the Web at http://mpfwww.jpl.nasa.gov as soon as they are processed.

Cooper will have a joystick and 3-D goggles, but that's where the similarity to an arcade game ends. For one thing, it will take 11 minutes for his radioed commands to reach Mars. So instead of steering in real time, Cooper will move a cursor the size and shape of the rover over

Below: The 60- by 120-mile oval in this Viking Orbiter photograph marks Pathfinder's planned landing site in the Ares Valles outwash plain. The mouth of Ares Valles is at the bottom right of the image, and north is toward the top.

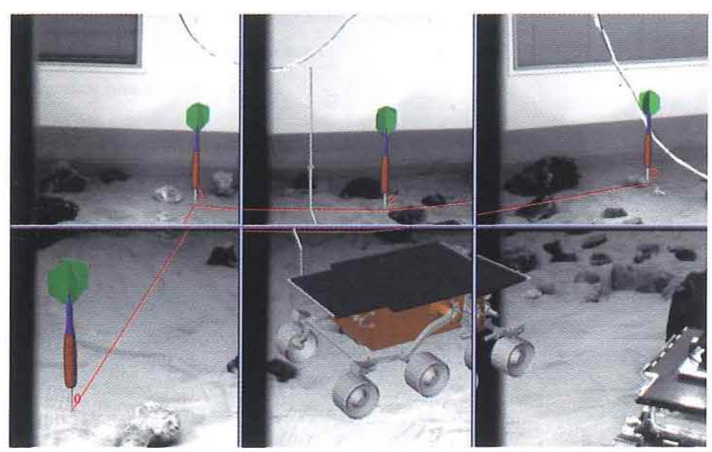
the 3-D landscape he sees. (This landscape is a mosaic of image pairs, corrected for camera distortions and perspective, from either the lander or the rover.) Starting from where Sojourner is in the picture, he clicks the cursor on an itinerary of points to be visited. Sojourner navigates to those "waypoints" on its own, using an onboard gyroscope to determine its heading and taking the average of the odometer readings from each wheel to calculate the distance traveled. "It's like a blind person with a cane," says Cooper. "It knows where it wants to go, and feels its way there." The cane is a set of laser beams that draw a five-by-five grid on the ground in front of the rover, starting at the front wheels and extending outward in a fan shape. Measuring how the grid points deviate from where they'd be if the ground were flat tells Sojourner the size of the rocks in its path. Onboard tiltmeters give warning of the potential for toppling over. If the hazard-avoidance software chickens out (it can be made more gutsy as Cooper gains confidence in the rover's abilities), Sojourner backs off and tries another route to the next waypoint. Sojourner prefers to drive in smooth arcs, like a car, but it can turn in its own length, like a tank, if it has to.

The rover and the lander talk to each other all the time, and once per Earth day (the Martian day is 41 minutes longer than a terrestrial day) the lander downlinks the accumulated conversations to Earth, along with all the images and data from the lander's other experiments. To ease the competition for transmission time, the images Cooper uses are compressed, but there's a fine line here. "On some tests, it was like we were looking through Vaseline-covered glass." Cooper and the science team study the images and negotiate where Sojourner should go next—the geologists decide which rocks to sample, and he tells them if Sojourner can get there. Once a consensus has been reached, Cooper assembles the command sequence, which gets uplinked back to the lander. He figures he'll be at it for 12 or more hours a day, at least to begin with; there's a backup driver, Jack Morrison, should Cooper get an inopportune flu bug. The primary mission only lasts a week, but with luck and TLC, Sojourner might keep going for months. Sojourner will stay within 10 meters of the lander to start with, but in a few days Cooper hopes to go out of its view—perhaps as far as 300 meters away—and rely on the rover's camera.

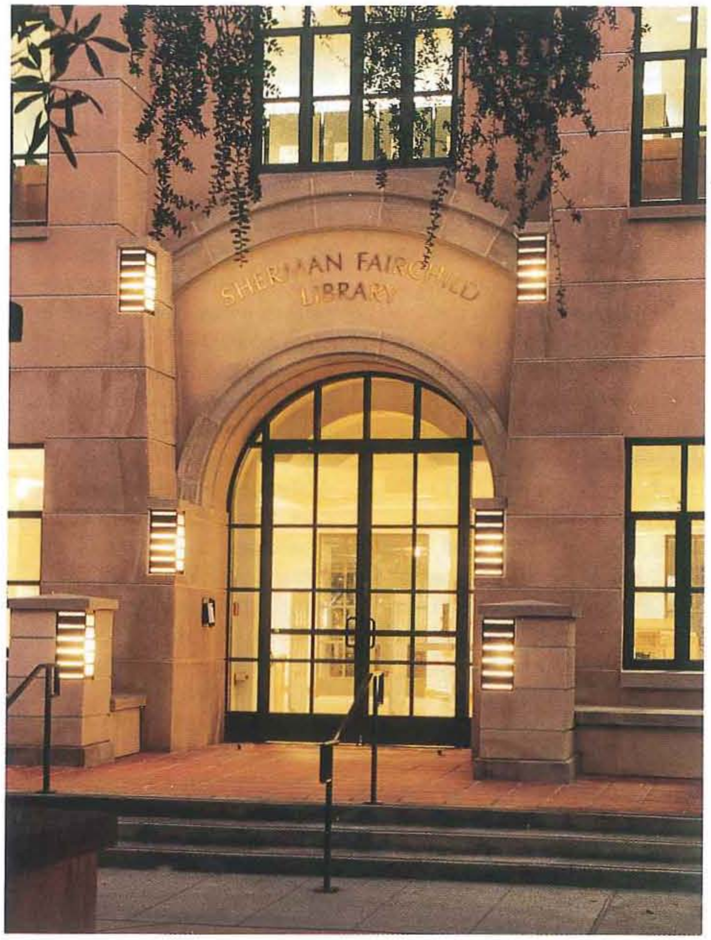
The point of all this wandering is to study the rocks and soil. Sojourner carries an Alpha Proton X-Ray Spectrometer (APXS), built by the Max Planck Institute in Mainz, Germany, and by the University of Chicago. To determine the surface's elemental composition, the APXS bombards a sample with alpha particles (helium nuclei) and measures the alpha particles, protons, and X rays that come back. The sensor head, which is about the size of a demitasse cup, is mounted on a flexible neck because the APXS has to remain pressed tightly against the sample for the up to 10 hours it takes to get a complete profile.

Pathfinder's landing site (19.5°N, 32.8°W) was chosen because it's a geologic souvenir store. Eons ago, a wall of water hundreds of feet high and 10–15 miles across tore through the Martian highlands and ridged plains, gouging out a 700-mile-long arroyo named Ares Valles. Although nobody knows for sure, the water is believed to have come from the sudden, catastrophic melting of roughly enough ice to cover California's Central Valley one mile deep from Bakersfield to Redding. The water, and the collection of rocks from all the terrain it had scoured clean, eventually spilled out into the lowlands of Chryse Planitia. And it's there, in the floodplain of Ares Valles, that Sojourner can sample rocks from thousands of square miles of Mars without straying far from the lander. Of course, the geologists won't know where any one rock came from, but they can get a lot of information about the region's overall mineralogy.

Oh, and by the way, 1-800-NASA-JPL rings at a paging service; don't call that number. Sojourner is unlisted. □—*DS*



Right: A view of the Mars Room as Cooper sees it. The colored rover in the center of this mosaic of six images from the lander's camera is the cursor that Cooper manipulates to drive the real rover, a corner of which can be seen in the lower right frame. The darts mark the waypoints the rover will visit. Superimposing the cursor exactly on the real rover's image tells the computer where the rover is and which way it's facing. The computer then tells the rover the distance and heading to each waypoint.



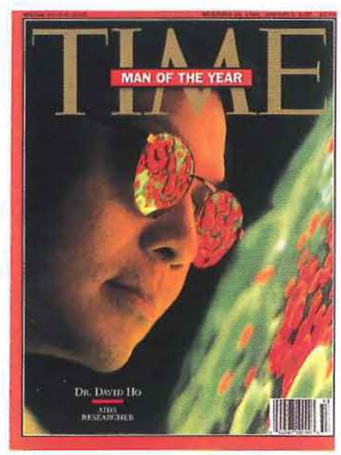
The Sherman Fairchild Library of Engineering and Applied Science, featuring the latest in "technological information delivery," opened for business January 2. Made possible by a $9.6 million grant from the Sherman Fairchild Foundation, the new library arose on the site of the old steam plant, between Thomas and Spalding Laboratories. Besides providing a central home for the collections from the department libraries in aeronautics, applied physics, electrical engineering, chemical engineering, computer science, engineering, earthquake engineering, and environmental engineering, the building is computerized, wired, and networked up to its gutters. But it's also user-friendly, with comfortable furniture and cozy reading spaces (yes, there are still books and journals).

MAN OF THE YEAR

AIDS researcher David D. Ho '74, was named *Time* magazine's Man of the Year for 1996 because his (and others') work brought real hope this past year that AIDS can be beaten. The tide against AIDS has turned, and those who accomplished this feat, *Time* claims, history will anoint as true heroes of the age.

Time devotes 16 pages of its December 30, 1996, issue to Ho, his career, and the assault on AIDS. After graduating from Caltech in biology (not physics, as the magazine states) Ho went on to Harvard Medical School and, as a virologist, began work on AIDS immediately after the disease was discovered in 1981. According to *Time*, he was among those who, from the very beginning, suspected that a virus was the cause. While most researchers concentrated on the later stages of the disease, Ho's most important insights came from his focus on the early stages of infection with HIV, characterized by a long period after initial infection during which, it had been presumed, the virus was dormant. Ho showed that HIV is extremely active during this symptomless period, replicating itself billions of times over and causing the immune system to wear itself out in a counterattack.

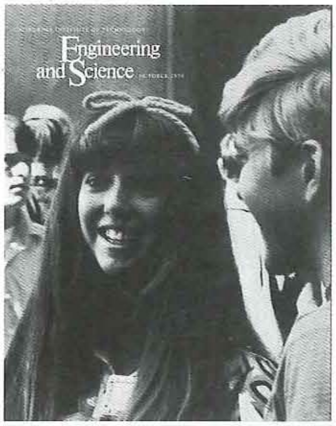
New therapies with combinations of drugs including protease inhibitors have shown promise for holding AIDS in check in its later stages, although not for curing it. Ho, who has been director of the Aaron Diamond AIDS Research Center in New York since 1991, used these drug combinations to treat 24 subjects in the very early stages of HIV infection, theorizing, according to the *Time* article, that if the virus could be knocked out at this point, it could be completely eradicated and the immune system would still be healthy enough to rebound. He announced at the 11th International Conference on AIDS in Vancouver last summer that none of his subjects showed a trace of the virus in their blood after a year of treatment, although this does not necessarily assure that the virus has been eradicated in their bodies. Remnants of the virus could still be hiding, infection may recur if treatment is stopped, and the drugs are very expensive. "But the worst fear—the one that seeded a decade with despair, the foreboding sense that the AIDS virus might be invincible—has finally been subdued," says *Time*.



Twenty-five Years of Women at Caltech—But Who's Counting?

A few end-of-year events casually marked the 25th anniversary of the admission of female undergraduates to Caltech: some concerts alluded to the date, a Women's Club luncheon celebrated it. Not exactly hoopla. But then, 1996 was not exactly the 25th anniversary; it was probably the 26th, depending on how you count.

Certainly people at Caltech can do the math. Maybe the anniversary just slipped by before anyone noticed because it's now so hard to imagine Caltech without women. But the change to coeducation was neither an easy battle nor a sure thing. At a Women's Club luncheon in November 1996, Professor of Biology, Emeritus, Ray Owen (without whose crusading efforts we might be celebrating the 10th year, or the first) described the chronology. Owen began lobbying for the admission of women as early as 1962, citing the pervasive social changes of the time; by 1967 he was calling the exclusion of women from undergraduate work at Caltech "an intolerable anachronism." And in November 1967 the Faculty Board minutes record that "Professor Owen moved . . . that the Faculty Board recommend to the Faculty that the Faculty recommend to the Administration and the Board of Trustees that the Institute proceed with all deliberate speed to the admission of women to undergraduate work at Caltech" — academic bureaucracy's version of a call for revolution.



Woman of the Year
***E&S*, October 1970**

By 1967 [Owen] was calling the exclusion of women from undergraduate work at Caltech "an intolerable anachronism."

Evidently the Board of Trustees was persuaded but cautious, because in November 1968 it passed a resolution approving "in principle" the admission of women both as entering freshmen and as upperclass transfers. Caltech's first truly coeducational class entered two years later, in 1970, and included 29 first-year women and two sophomore transfers. (The present freshman class includes 67 women). A few more women came as upperclass transfers in 1972, so 1973 marked the first graduation of women from Caltech, and 1974 the graduation of women who had entered as freshmen.

And how did that pioneering class of women fare? Surprisingly well, if the several alumnae who spoke to the Women's Club are a representative sample. Attorney Debbie Dison Hall remembers being greeted by a homemade banner strung from Millikan Library reading "Welcome Co-Techs," which she thought more clever than malicious. Otherwise, she says, she was not treated any differently than the male students, was not subjected to different pressures, and was made to feel "welcomed, successful, and valued." Kim Fisher Thomas, an environmental economist at Whittier College, says she passed her whole first year (she entered in 1971) with a knot in her stomach, but that can probably be said of many Caltech freshman.

The speakers emphasized, however, that they had been a diverse group whose experiences differed. One alumna—not at the luncheon—has another freshman-year memory: a professor privately advising her to drop his class, because she wasn't going to be up to it.

For those committed to celebrating 25 years of women at Caltech, several authentic milestones loom on the horizon: 1998, the 25th anniversary of the first female undergraduates to graduate; and 1999, the actual 25th anniversary of the graduation of the first genuine class of women at Caltech. Mark your calendars. □—*RR*

More Things To Worry About

A popular treatment for kidney stones may not be as safe as has been thought. The procedure, called extracorporeal shockwave lithotripsy by the doctors and plain old lithotripsy by the rest of us, uses tightly focused shock waves to pulverize the stones (which really *are* rocks—they're mineral deposits, usually calcium oxalate) while they're still inside the kidney. The procedure is quick, doesn't involve surgery, and can be done even in a mobile clinic. But there can be short-term side effects: internal bleeding, blood in the urine, and bruises where the shock wave enters and exits the body. This would seem to indicate tissue damage and possible long-term effects, so Bradford Sturtevant (MS '56, PhD '60), Liepmann Professor of Aeronautics, and grad student Danny Howard (MS '91, PhD '96) decided to find out what was going on. They used a laboratory lithotripter, as the stone-crushing machine is called, built by Dr. Bruce Hartenbaum of H-Tech Laboratories in Santa Monica, California.

Sturtevant and Howard suspected that the shock waves weren't so tightly focused after all. The speed of sound in blood is about 5 percent higher than in tissue, and the kidneys, which filter the blood, are filled with blood vessels. Some simple calculations showed that the portion of a shock wave passing through a blood vessel can get far enough ahead of the rest of the wave to distort the overall wavefront. As the wave travels through the maze of blood vessels en route to convergence on the kidney stone, the wavefront can get hopelessly folded. The pulse's energy can blur, forming microscopic regions of intense shear that can tear the surrounding tissue and cause additional damage with each pulse.

The first set of experiments established a baseline by finding out what a properly focused pulse looks like. By using a pressure sensor immersed in four test fluids (water, ethylene glycol, glycerin, and castor oil), it was established that a pulse generated at the focal point a transient pressure of nearly 4,000 pounds per square inch—a single, crisp peak.

In the second round of tests, hollow glass beads averaging 65 microns (0.00004 inches) in diameter were added as stand-ins for blood vessels. (Blood vessel diameters in the kidney range from 20 to 200 microns.) The beads did, in fact, scatter and weaken the pulse. And the more beads that were added—the denser the blood-vessel network, in other words—the smaller and more broken up the pulse became. At spacings comparable to the average density of blood vessels in the kidney, the pulse's peak was a hundred-fold less intense, and the pressure trace looked like a seismogram.

The third set used tissue samples from a local supermarket. Howard made a sandwich, layering skin (one millimeter thick); muscle—that is, steak (19 mm); fat (10 mm); and kidney (30 mm) between the shock source and the pressure sensor. Again, the peak dispersed and lost most of its punch.

To assess the potential for collateral damage, Howard repeated the bead and tissue tests, replacing the pressure sensor with a nitrocellulose membrane about 10 microns thick. This membrane, which represented cell walls in the vicinity of the kidney stone, emerged looking like it had been hit by buckshot. Small holes and tears could appear after as few as 20 pulses; after 100 shots there generally wasn't much left. Since several thousand pulses are generally administered in a lithotripsy treatment, this is clearly cause for concern. "When these machines were originally approved," says Sturtevant, "basically none of the right questions were asked." Sturtevant is now collaborating with Dr. James Lingeman, of Methodist Hospital in Indianapolis, and Dr. Andrew Evan, a professor of anatomy at Indiana University, in an attempt to learn what the right questions are. □—*DS*

"A Conversation with Walter Cronkite" launched the Lee DuBridge Distinguished Lecture Series last November. Playing to a capacity crowd in Beckman Auditorium, the veteran newscaster recounted stories of presidents he has known (from Hoover onward) and events he had witnessed in his career—most of the major ones of this century since World War II. Jess Marlow, news anchor of KNBC, was the other half of the conversation, keeping Cronkite supplied with questions. Caltech honored Cronkite with an asteroid named for him, discovered by Eleanor Helin (see below) in 1990. The asteroid, 6318 Cronkite, has a "high profile and slightly eccentric orbit" and only a very slight chance of a collision with Earth some 10 to 30 million years hence.

Still More Things to Worry About

Yet another kind of stone that can cause you grief is the Aten class of asteroids, one of several classes of asteroids whose orbits bring them very close to Earth. JPL astronomer Eleanor Helin discovered the first Aten back in 1976, and she and colleagues Steven Pravdo, Kenneth Lawrence, and David Rabinowitz of JPL's NEAT (Near-Earth Asteroid Tracking) program have just located the 24th one. This new find, provisionally named 1977AC11, is about 1,000 feet in diameter, so it's really going to hurt when it hits us. *If* it hits us, that is—it missed Earth by 10 million miles on January 23rd (Pasadena time), and in its current orbit doesn't get any closer to Earth than about one million miles, which is four times farther away from us than the moon is. Also, its orbital plane is skewed 31 degrees from ours, so it gets far fewer chances at us than an asteroid with a nearly coplanar orbit would. But its orbit is gradually evolving, Helin says. "These guys are always being nudged around over the eons. This one is unusual in that its orbit crosses both Earth's and Venus's. They each give it a little kick every time it gets close to them." So over the next several million years, who knows?

In the meantime, Helin has no plans to name 1977AC11 for a prominent journalist. □—*DS*

Decompression Chili Heats up Students

Wan, staring, night-dwelling, they stagger forth from whatever dark place they've nested. It's the smell that draws them, that and the warm red liquid. They need it to replenish vital energy, to rebuild neural circuits, to go on. They need it badly. And they need a lot of it. About 3,000 bowls, in fact.

The Caltech Y's Decompression Chili has been a custom—"permanent if sporadic," says Sue Borrego, executive director of the Y—since the 1970s. But in the past few years it's become a regular part of undergraduates' Decompression, an event designed to lighten those hellish weekends before fall, winter, and spring finals. (Decompression was December 7–8 last term.) When Borrego became director of the Y several years ago and heard about the chili, she asked innocently, "Who cooks it?" "You do," she was told. So she became a chef on an industrial scale.

She has a lot of help: three to five undergraduates each time, and sometimes alumni. (In December, Greg Steiert '96, nostalgic for all-nighters, came down from his job at Intel in Oregon to sauté and stir). And they have it down to a science, she claims; she procures the vanful of ingredients for both beef and vegetarian batches at her local supermarket—look for prechopped onions, she's learned; much easier on the eyes—then the whole crew mixes up the concoction (see recipe below) in the kitchen of Chandler Dining Hall, and doles out steaming bowls on the sidewalk between Chandler and Winnett Student Center.

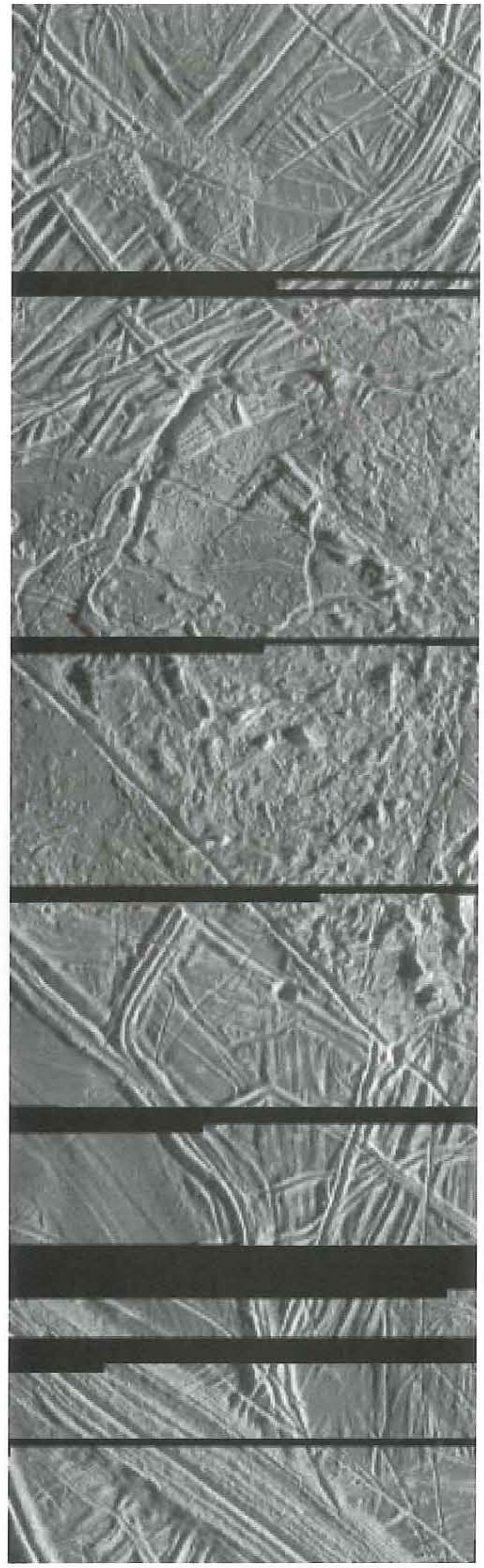
The final, and most crucial, step of the process is the addition of spices, and here, Borrego admits, she was a failure: her chili was too bland. The students mutinied, dumping in three times more chili powder. "I'm from the Midwest," she says apologetically, "where ketchup is a spice." □—*RR*

Decompression Chili

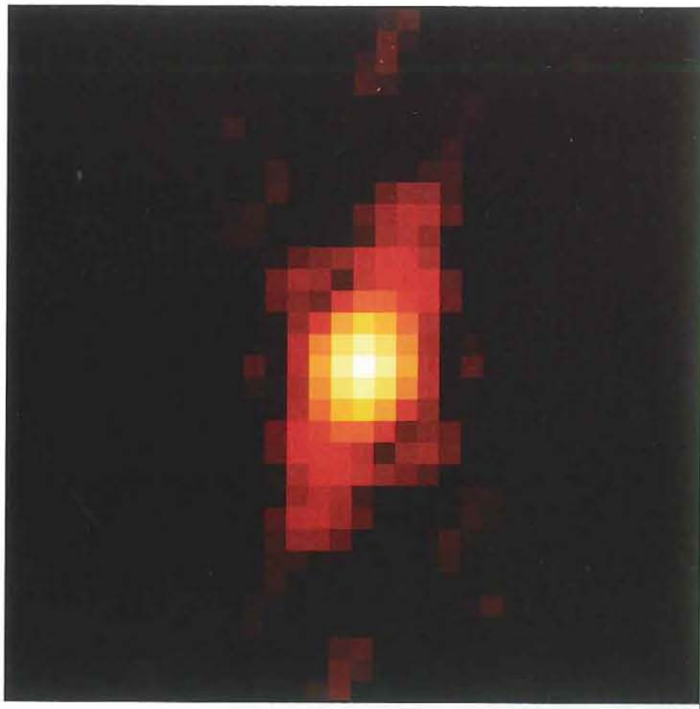
Sauté:
- 90 lbs ground beef (substitute carrots, broccoli, mushrooms, other vegetables in season for meatless recipe)
- 30 lbs onions, chopped
- 20 lbs (one case, or 25 bunches) celery, chopped

Simmer with:
- 24 32-oz cans each tomatoes, beans
- 60 oz chili powder (to taste)

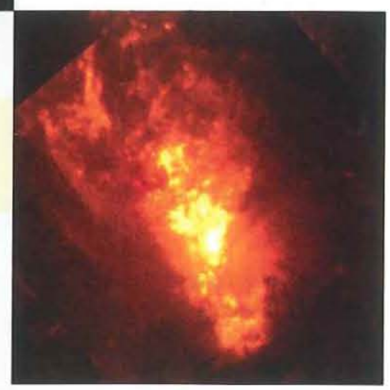
Yield: about 3,000 bowls



Not the tracks left over from a dirt-bike convention (or a lost robotic vehicle), these complex faults and ridges were created by tectonic processes in Europa's surface. Jupiter's icy moon was captured in this very high-resolution mosaic image made during Galileo's first close pass (2,119 miles) by Europa in December, during the spacecraft's fourth orbit around Jupiter. The resolution of the mosaic (the section shown here spans about 10 miles by 30 miles) is 50 times better than the best Voyager images and 500 times better than Voyager pictures of Europa. The sun is illuminating the surface from the right.



The new infrared image of the nucleus of NGC 1068 (far left), made at a wavelength of 2.2 microns, shows the same level of detail as the Hubble Space Telescope image (at ultraviolet and visible wavelengths) of a much wider view of the galaxy's nuclear region (below). Weinberger's image is 160 light-years on a side and sits just at the bottom apex of the central bright cone in the HST picture, which spans 1,200 light-years by 1,700 light-years. The two pictures are oriented in the same direction here, but the close-up infrared image shows an elongated structure, probably made of stars, which does not point in the same direction as the cone in the larger-scale picture. A distribution of stars like this could help force gas into the black hole at the center. In both pictures, the colors from red to white represent increasing brightness.



Close-up View of a Galactic Nucleus

The most detailed infrared image of the environment of an active black hole has emerged from work at the 10-meter W. M. Keck Telescope on Mauna Kea, Hawaii. Grad student Alycia Weinberger and her collaborators have used the computer-intensive technique of speckle imaging at the Keck to image the nucleus of NGC 1068 and have uncovered a new structure in this nearby active galaxy. The galaxy, found in the constellation Cetus at a distance of about 50 million light-years, reveals a a bright active nucleus at infrared wavelengths. This nucleus has long been thought to harbor a black hole as its central engine and, because it is bright and nearby, has been intensely studied by astrophysicists.

Made at a wavelength of 2.2 microns, Weinberger's near-infrared image has the capability to reveal structures which are only 12 light-years across. This is an extremely small distance by galactic standards, as small as about three times the distance between the sun and its nearest stellar neighbors. Although taken from a ground-based observatory, this image has resolution as fine as the Hubble Space Telescope achieves in the visual part of the spectrum. The space telescope does not currently have an infrared camera, but is scheduled to receive one as *E&S* goes to press. The elongated feature discovered by the Caltech group has not been seen in Hubble's optical images.

There are two very interesting aspects of this image. First, the image does not show a circular distribution of light, but rather an elongated one, and second, the axis of the emission points in a direction different from that of previously observed visual emission. The near-infrared light used to make this picture typically traces the distribution of hot dust and cool stars.

In NGC 1068, however, it is very unlikely that there could be dust 100 light-years from the central black hole that would be hot enough to produce the observed emission. Rather, Weinberger says, it is likely that the observed extended near-infrared light is from stars. Furthermore, since it points in a different direction, this newly resolved infrared emission is likely to come from a source entirely different from that of the visual emission.

It has long been proposed that elongated, noncircular distributions of stars in the shape of a "bar" are a way of funneling material to an active nucleus. As gas moves in a noncircular distribution of stars, such as what may be seen in Weinberger's image, it is forced into orbits likely to take it near the central black hole. This provides a continuous mechanism for "feeding" the central engine.

"The significance of this research is that it finds a brand-new feature in this galaxy," Weinberger says. "And even more, this new feature may provide observational evidence for a theoretically predicted means of channeling material to the black hole on very small scales." The image is by no means detailed enough to show the in-fall of the matter itself, Weinberger stresses. For this, one would need a resolution of less than a light-year, and there is currently no way to make such finely detailed pictures.

Nonetheless, the quality of this image is unparalleled because it relies on the unique resolving power of Caltech's 10-meter Keck Telescope and the technique of speckle interferometry to remove the distorting effects of Earth's atmosphere. With this technique, a series of very rapid exposures are made of the object, freezing the atmospheric distortions that cause stars to twinkle. Then the distortions are removed in computer post-processing. As the largest infrared telescope in the world, the Keck Telescope provides the best obtainable resolution.

Weinberger is currently completing work on her doctorate. She will continue doing observations to support this research, a part of her thesis. "It will be exciting to look at NGC 1068 with similar resolution in other infrared wavelengths," she says. "The more information we have across the spectrum the more we'll understand about the nature of this extended emission."

Also collaborating in this research are her thesis supervisor, Gerry Neugebauer, and Keith Matthews, both of the Caltech physics department. □—*RT*

A Brief History of Nakedness

It's not every day that a theoretical physicist pays off a bet on nakedness, but Stephen Hawking did so February 5. Hawking, the Lucasian Professor of Mathematics at Cambridge University, had been at Caltech for the previous six weeks as a Sherman Fairchild Distinguished Scholar. He conceded, just before leaving for home in England, a 1991 bet he had made with Kip Thorne and John Preskill that a phenomenon known as the naked singularity is possible.

Thorne and Preskill, both professors of theoretical physics at Caltech, think that naked singularities are allowed by nature. Hawking does not, but conceded the bet "on a technicality," he said.

In accepting Hawking's payoff, Preskill said that "We're much more tolerant of nakedness" than the British physicist.

"It comes from living in Southern California," Thorne added.

Hawking paid off the bet by presenting his two American colleagues with adequate raiments to shield their nakedness from the vulgar view. Specifically, the goods consisted of two T-shirts, which the bettors would only say were inscribed with "an appropriate message" from Hawking.

From left, John Preskill and Kip Thorne assist Stephen Hawking in sealing with his thumbprint a new bet on whether general conditions for the creation of a naked singularity will be found.

The big breakthrough came when another physicist named Matthew Choptuik (now at the University of Texas at Austin) developed a supercomputer simulation showing how a naked singularity could occur. To back up a bit, a singularity is a place where matter or light crashes in by force of its own weight to form a region of exceedingly high density.

No one disputes that singularities can exist, but Hawking believes that a singularity can occur only inside a black hole, where it cannot be seen. According to Thorne and Preskill, there should be situations in which singularities could exist outside of black holes and therefore be observed.

Hawking has always rejected the idea of the naked singularity, but admits that Choptuik's computer simulation shows how one could conceivably exist. There's virtually no possibility that Choptuik's naked singularity would ever arise in a real universe, however.

"Basically, it could exist only in a computer," Preskill said. "But it's the sort of event that would be allowed to happen, and that's what the bet was all about."

For his part, Hawking said that he's still a betting man when it comes to theoretical physics, even though he is now 0-2. In fact, a new bet is already on (see above).

"I'm going to win this time, but I don't know when," he said. □—*RT*

Political Scientists Ranked Sixth

Even with only nine faculty members, Caltech's political science group ranks sixth (after adjusting for size) in the nation, according to a report issued in December by *Political Science and Politics*. The top 10 with numbers of faculty are:

1. Rochester Institute of Technology (18)
2. Stanford (28)
3. Harvard (48)
4. Yale (29)
5. University of Michigan (44)
6. Caltech (9)
7. UC Berkeley (41)
8. University of Iowa (22)
9. University of Indiana (27)
10. University of Minnesota (30)

"It is not a case of choosing those [faces] which, to the best of one's judgment, are really the prettiest, nor even those which average opinion genuinely thinks the prettiest. We have reached the third degree where we devote our intelligences to anticipating what average opinion expects the average opinion to be. And there are some, I believe, who practise the fourth, fifth and higher degrees."

Taxi Drivers and Beauty Contests

by Colin F. Camerer

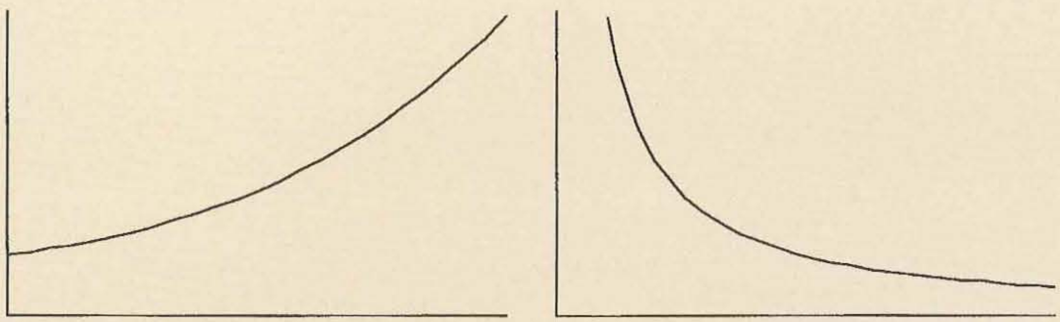
The trading floor of the New York Stock Exchange. The British economist John Maynard Keynes likened playing the market to voting for the prettiest face in a beauty contest; hence the second part of this article's title.

I spent a year in New York City not long ago, and I took a lot of cabs. Most cabdrivers in New York are independent contractors. They rent the cab from a taxi company for $76—paid in advance—for 12 hours. They keep all the fares they collect, and they can call it quits and return the cab at any time before the 12 hours are up. Because Manhattan is so crowded, drivers usually just cruise the streets waiting for someone to hail them. Some days are especially good—when it rains or snows, during the holidays, or when a convention is in town, for example. Other days are bad—weekends, when fewer businesspeople are around; and the summer is slow because people leave Manhattan to escape the heat, humidity, and gunfire. So Linda Babcock (whose father, Charles Babcock [MS '58, PhD '62], was a professor of aeronautics and applied mechanics at Caltech until his untimely death in 1987) and George Loewenstein of Carnegie Mellon University, Richard Thaler of the University of Chicago, and I became curious about a simple question—how does the amount of hours a cabbie works vary with that day's average hourly earnings? There are two basic theories that might apply. One is called the law of supply. The other, which we crafted from bits and pieces of psychology, we call "daily income targeting."

The law of supply is the twin sister of the law of demand—people should want to sell more of something when the price is high than when the price is low, assuming everything else is constant. So you'll sell more of your labor hours when wages are high, and the so-called labor-supply curve slopes upward, as shown on the page after this one. The law of supply says that you should work a lot when it pays to do so, and when it doesn't pay, go home! Take time off.

The other theory, daily income targeting, was taught to us by the cabdrivers, many of whom are amateur philosophers, political scientists, and labor economists. (The late Harry Chapin appears to have been familiar with it as well, as the opening couplet of his ballad "Taxi" bears witness: "It was raining hard on a Saturday / I needed one more fare to make my night.") Many of the drivers we talked to said they decided how long to work by setting themselves an income target every day—for example, they might want to earn $150 in cash in order to clear $75 beyond the rental fee—and when they reach that target, they quit. Target setting can be very motivating in unpleasant or tedious activities, like exercise. There's also substantial psychological evidence that people dislike losing a lot more than they like corresponding amounts of winning. This implies that drivers hate to quit before they reach the target, but once they reach it, they aren't very enthusiastic about trying to go beyond. So income targeting perversely predicts that cabbies are going to quit earlier on good days. If you want to make $150 and you're earning $25 an hour (which would be a pretty good day for these guys), you can go home after six hours. But on a bad day, when you're earning $15 an hour, you've got to drive ten hours. The labor-supply curve will be a hyperbola, which is also shown on the following page.

These two theories thus give very different predictions, which we tested in our study. We analyzed 3,000 observations of cabdrivers' behavior from the years 1988, 1990, and 1994. The data came from the New York City Taxi and Limousine Commission, ironically known as the TLC, which had collected it for other studies. These data were in the form of taximeter readings, and the TLC was kind enough to give them to us (for free!) on floppy disks—bureaucracy has its moments. When you get into a cab, the driver punches a button and a meter automatically records the number of miles driven and the amount of time spent sitting in traffic. From the meter records we could compute a driver's earnings, except for tips. Tips aren't recorded anywhere, so we left them out of our analysis, but



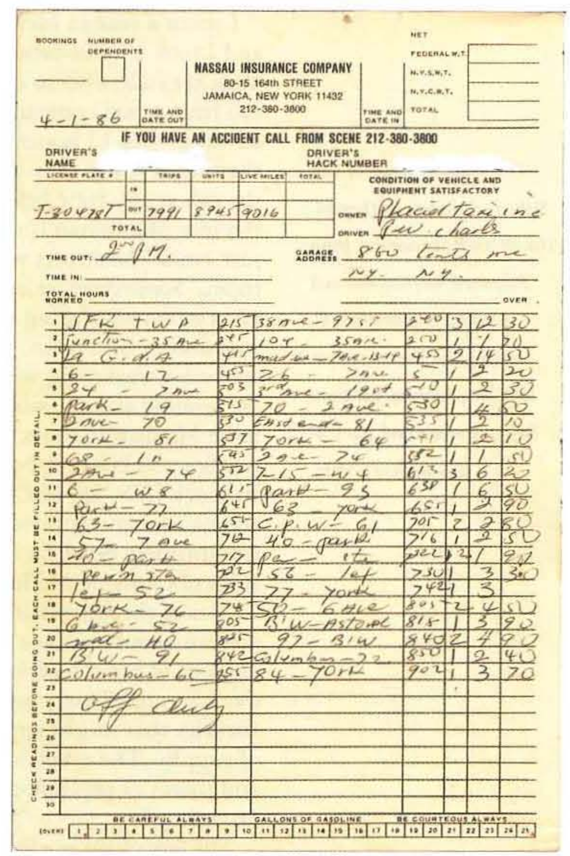
Above: The labor-supply curve. Wages are plotted on the vertical axis; hours worked on the horizontal one. The law of supply says that when the hourly wage goes up, people will work longer hours (left). The income-targeting theory predicts the opposite: people will work less as their hourly wage rises (right). If hours and wages were plotted on logarithmic scales instead of linear ones, this hyperbola would plot as a downward-sloping line.

on average they're probably 10–15 percent of the driver's income and do not vary much from day to day. The meter data should represent the bulk of the driver's income accurately.

A scatter plot of some of our data is shown at the top of the opposite page. I should warn the faint of heart that it looks very messy, but as economic data go—particularly in areas like labor economics, where there are lots of external factors that influence the data—this is actually a pretty strong correlation. I hope you can see that the line of best fit through this cloud of data slopes downward. In fact, the slope is significantly negative to a confidence level of more than 99.9 percent. So the data clearly support the targeting theory rather than the law of supply.

There's an objection that can be raised here—in order to follow the law of supply, you've got to have a certain level of economic security. These guys may have to keep driving until they make $150 because they need the cash—they don't have enough savings to buy groceries and pay the rent if they quit early on slow days. The reason we don't think this explains our findings is because some drivers in our samples own their own cabs. In order to legally operate a cab in New York City, you have to own a taxi medallion—an ugly, plastic-metal thing that's pasted on the hood of the cab. These medallions are restricted in supply (there are only 11,387 of them, and that number has been fixed for 60 years), so they're quite valuable. They're worth about $150,000, yet 10 percent of the drivers in our samples own one personally. If we assume that the drivers who can afford to own a medallion have some cash in the bank, we might predict that they would behave differently than the renters. But both groups seem to behave about the same way.

Another important consideration is that cabdrivers vary in experience. Happily for us, New York City cabdriving licenses are numbered chronologically by date of first issue, so the person


NASSAU INSURANCE COMPANY
80-15 164th STREET
JAMAICA, NEW YORK 11432
212-380-3800

BOOKINGS NUMBER OF DEPENDENTS

4-1-86 TIME AND DATE OUT — TIME AND DATE IN

NET — FEDERAL W.T. — N.Y.S.W.T. — N.Y.C.W.T. — TOTAL

IF YOU HAVE AN ACCIDENT CALL FROM SCENE 212-380-3800

DRIVER'S NAME — DRIVER'S HACK NUMBER

LICENSE PLATE # T-304787T — TRIPS — UNITS — LIVE MILES — TOTAL
IN — OUT 7991 — 8945 — 9016
TOTAL

CONDITION OF VEHICLE AND EQUIPMENT SATISFACTORY

OWNER Placid Taxi Inc
DRIVER

TIME OUT: 2 PM.
TIME IN:
TOTAL HOURS WORKED
GARAGE ADDRESS
OVER

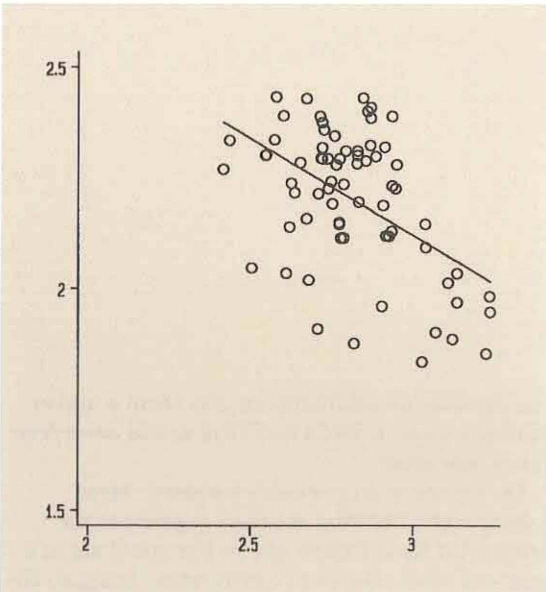
CHECK READINGS BEFORE GOING OUT. EACH CALL MUST BE FILLED OUT IN DETAIL.

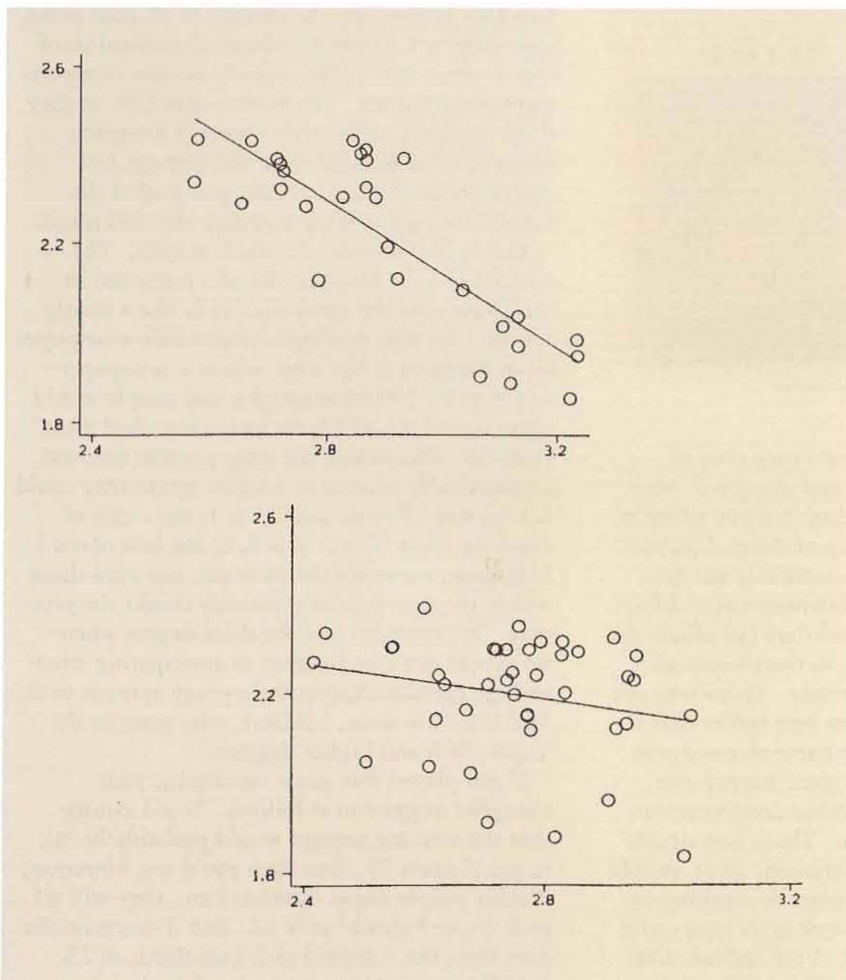
Off duty

BE CAREFUL ALWAYS — GALLONS OF GASOLINE — BE COURTEOUS ALWAYS

Above: Some of the meter data were verified by examining "trip sheets" such as this one, in which drivers log (from left) pickup point, pickup time, destination, dropoff time, number of passengers, and fare.

Top: If you look at a logarithmic plot of the labor-supply curve for a sample of taxi drivers, you can see that the line of best fit slopes downward, contrary to the law of supply. Bottom: But if you analyze the sample's inexperienced (upper) and experienced (lower) drivers separately, you will find that the experienced drivers' curve is more nearly horizontal.





with license number 14,682 got it just after the person with number 14,681. Therefore, we can sort drivers into high- and low-experience groups by their license numbers. Look at the difference in their labor-supply curves, as shown at left. Again, the data are noisy, but the low-experience drivers on the left have a slope very close to −1, which is what the income-targeting theory predicts. (A slope of −1 means that if your wage goes up by 10 percent, you cut your hours back by 10 percent to keep your income constant.) The high-experience drivers on the right still don't look much like they're obeying the law of supply, but it does appear as if experience is teaching them to make hay while the sun shines—to drive longer hours on good days.

This distinction between new and old drivers is important because about half of the cabbies in New York have been driving cabs for less than a year. In 1991, over 40 percent of all New York cabdrivers were born on the Indian subcontinent, 11 percent were from Africa, and another seven percent each were from the Caribbean, the Middle East, and the former Soviet Union. Only about 10 percent were born in the United States. The point is that driving a cab is an entry-level job for many immigrants, so there's a constant inflow and outflow of new drivers. These inexperienced drivers may be using the income-targeting rule because they haven't yet figured out that they can do better by obeying the law of supply.

We learned two basic lessons from this study. The first is that cabbies would get an automatic raise of 8 percent if they drove the same number of hours every day, rather than knocking off early on the good days and working late on the bad days. If they obeyed the law of supply, they could earn 15 percent more income. The median annual wage of these drivers in 1995 was about $22,000 a year, so they could have made about $2,000 more per year by simply changing their driving habits.

The second, more important lesson is that

Photo courtesy of the John W. Hartman Center for Sales, Advertising and Marketing History, Duke University

The "Miss Rheingold" campaign, run by the J. Walter Thompson Co. for Liebmann Breweries, Inc. for over 25 years, is the best-known American example of a Keynesian beauty contest. At the height of its popularity, between 15 and 20 million votes were cast per year—a turnout second only to the Presidential elections.

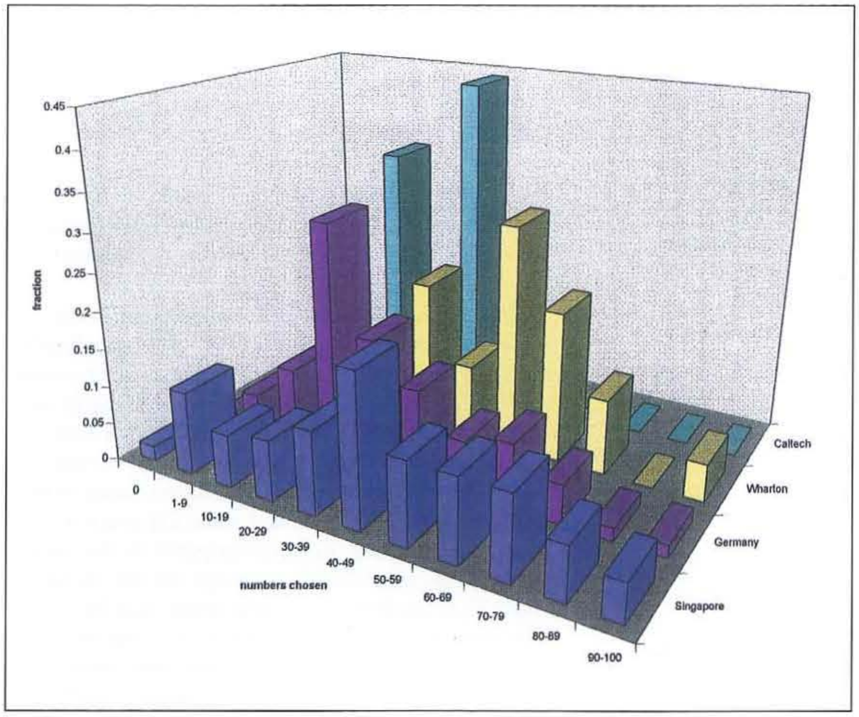
perhaps we should be skeptical about simple economic principles like the law of supply. Most previous studies were inconclusive about whether the supply curve even went up or down, because most people's salaries change relatively rarely—once a year, perhaps. But cabdrivers earn a different hourly wage every day, and they can adjust the numbers of hours they drive, so there's enough variation in the data to see trends. That's why our study shows more clearly than ever before that for taxi drivers, the labor-supply curve slopes down, not up. During the Reagan years, supply-side economists argued that if income taxes were cut, the after-tax wage would rise. That's just simple arithmetic. And then, the argument went, people could earn more spending money by working an extra hour, so people *would* work extra hours, and everyone would be better off. Very logical. Our results suggest the opposite—if you were to lower the tax rates on cabdrivers to give them a higher after-tax wage, it looks as if they would drive *fewer* hours, not more.

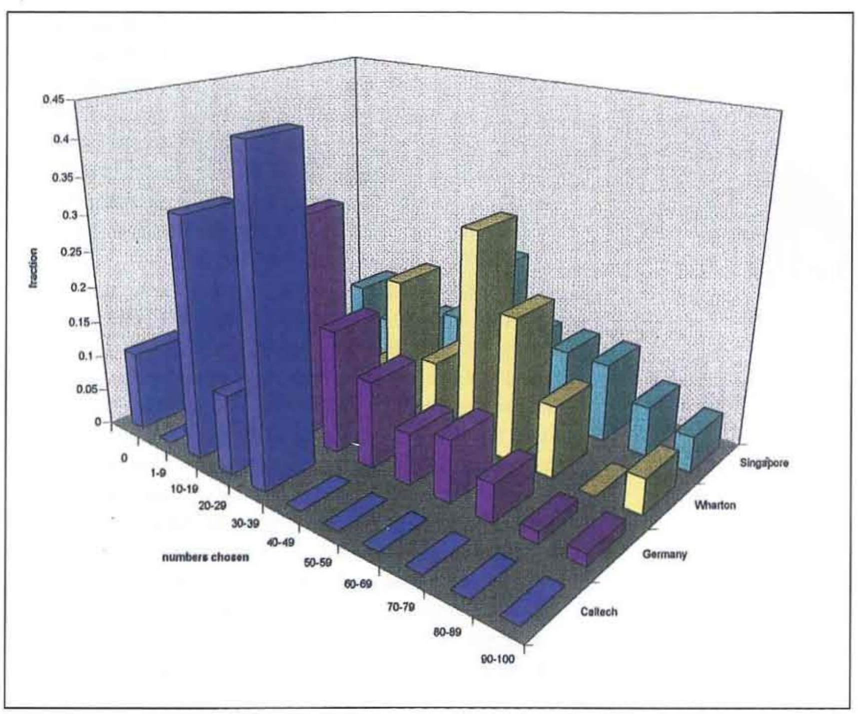
Let me move on to beauty contests. Here I don't mean the Miss America pageant or the tryouts for Rose Parade queen, but you'll see in a moment where the term comes from. Imagine the following game: Everybody picks a number from 0 to 100. I compute the average of all your picks, and whoever's number is closest to two-thirds of that average wins. (We actually do this in experiments on students. The winner gets $20, so they think carefully before they choose.) Everyone wants to be at two-thirds of the average, but everyone else does, too, so the real goal of the contest is to guess what everyone else will guess.

This is like playing the stock market. The economist John Maynard Keynes remarked in the 1930s that the stock market is like a beauty contest. He had in mind contests that were popular in England at the time, where a newspaper would print 100 photographs, and people would write in and say which six faces they liked most. Everyone who picked the most popular face was automatically entered in a raffle, where they could win a prize. Keynes wrote, "It is not a case of choosing those [faces] which, to the best of one's judgment, are really the prettiest, nor even those which average opinion genuinely thinks the prettiest. We have reached the third degree where we devote our intelligences to anticipating what average opinion expects the average opinion to be. And there are some, I believe, who practise the fourth, fifth and higher degrees."

If you played this game repeatedly, your thoughts might run as follows. You'd assume that the starting average would probably be 50, so you'd guess 33. But then you'd say, hmmmm, if other people are as clever as I am, they will all pick 33, so I should pick 22. But if everyone else does that, too, I should pick two-thirds of 22. And if you carry this through infinitely many

How real people behave in a one-round beauty contest. The two graphs show the same four sets of data, but in two different front-to-back orderings to minimize the number of short bars that are obscured by taller bars in front. (The colors, however, don't travel with the bars.)

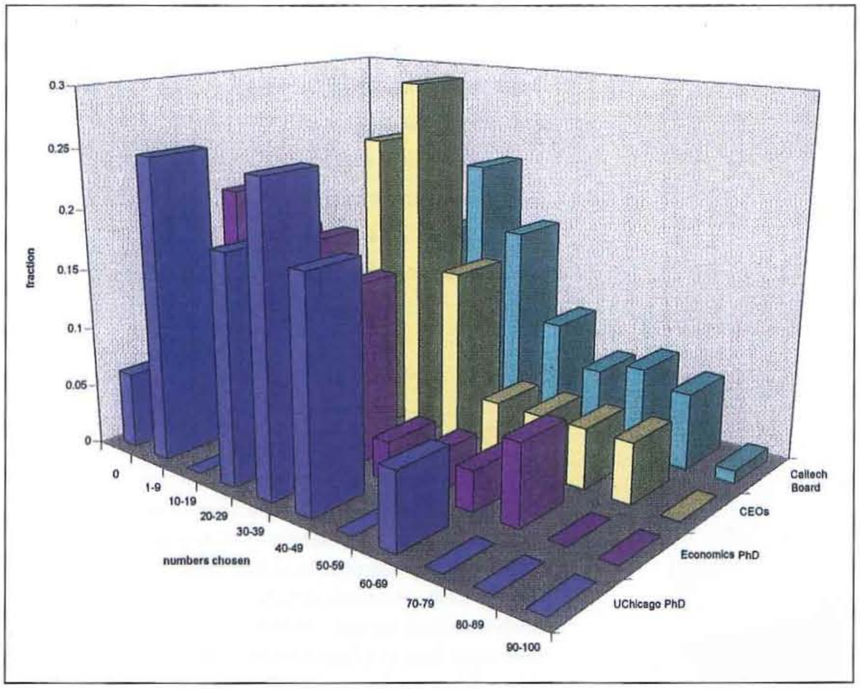


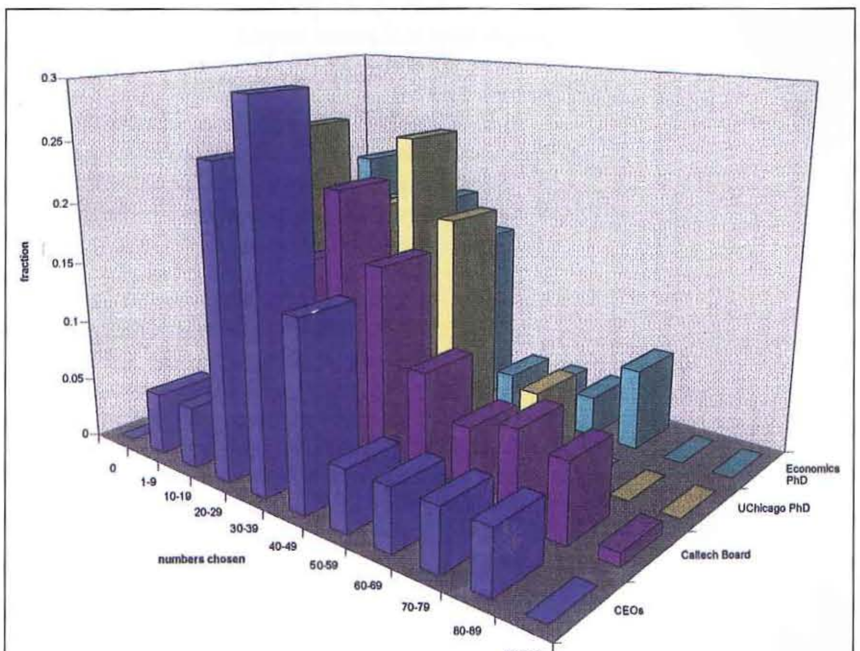


levels of reasoning to the logical end, you'll wind up picking zero. If I were speaking to a game-theory audience, people would nod profoundly, because zero is what game theory predicts for this situation. Game theory is the branch of social science that analyzes strategic interactions in mathematical terms. It was founded quite a long time ago, but it's had a slow fuse—only in the last 10 or 15 years has it come to the fore in reasoning about economics and political science. (In fact, people here at Caltech helped establish the use of game theory in political science, and still do quite a lot of it.)

So how do people actually behave? Do they pick zero? The data at left are from experiments on undergrads from Singapore, Germany, the Wharton School of Business at the University of Pennsylvania, and Caltech. The German data were collected by Rosemarie Nagel of the University of Pompeu Fabra. The Singaporean data were collected by Teck-Hua Ho and Keith Weigelt; they also collaborated with me on the Wharton data. (Ho and Weigelt, who are now on the faculty at Wharton, were both students of mine when I was there.) The average pick across all these experiments was around 40, so if you guessed about two-thirds of 40, or 27, you'd probably win. Notice that 40 is somewhat less than 50, so if we use these data to gauge how many steps of reasoning people are doing about other people's reasoning, some number from one to three seems reasonable. It's clearly not the game-theory prediction of infinity, but it also clearly demonstrates the performance of at least one step of reasoning.

We're now trying to refine this estimate of how many steps of reasoning seem natural, and how it varies with education and other factors. For example, no Caltech student chose above 40. Most Techers picked numbers between 30 and 40. Several picked in the neighborhood of 10 or 20, and 10 percent of them did, in fact, actually pick zero. The Caltech students and the German stu-



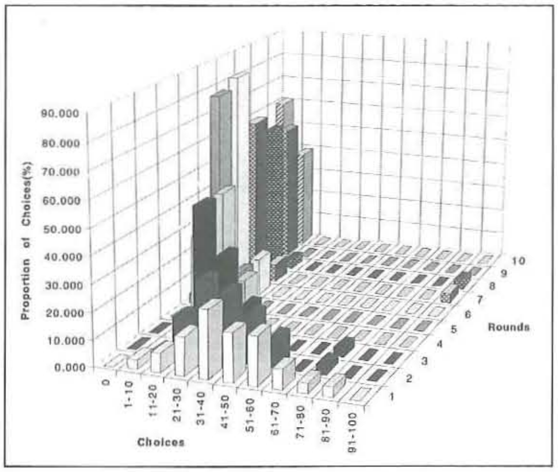


Four more sets of data, again presented in two different orders from front to back. The University of Chicago PhD data is courtesy of Richard Thaler.

dents appear to have been reasoning one or two steps more deeply than the Wharton students and the students in Singapore.

We've also conducted this experiment, more informally, with other groups of subjects. (Replication with different groups of people is, of course, essential if we want to generalize our findings to all human beings.) The plot at left shows four more groups. The front two groups in the top figure are PhD students in economics (none from Caltech), who may have had some exposure to game theory. And, in fact, compared to the undergrads in the previous plot (except for the Techers), these PhD students do choose lower numbers. The average pick here is around 25—one step beyond the undergrads. The additional education is doing something. The group labeled "Caltech Board" is from an experiment I conducted when I gave a talk at a meeting of Caltech's Board of Trustees in the fall of 1995. There were about 80 or 90 people there, including spouses and some people from the faculty and administration, and I just couldn't resist the opportunity to see how they would behave. The Caltech Board is a truly amazing group that includes many extremely successful businessmen, some billionaires, several brilliant scientists, and two former judges. Notice that they act pretty much like the college students—the average pick is about 40. But a few people do choose very low numbers, like zero. And several people, who may have been confused because I didn't explain the procedure as carefully and thoroughly as I would have in a real experiment, picked very high numbers. This was not a well-run experiment, but the subject pool is so unusual that I'll show it nonetheless.

The sample labeled "CEOs" is really remarkable. We've seen that college students do not obey game theory, which assumes that people are perfectly rational. (This is hardly surprising to anybody with teenagers in college.) So it's easy to criticize our experiments by saying that what *really* matters



This strategy is sometimes called the "greater-fool theory," because even though you're a fool to pay as much as you did, you're betting that there's a greater fool just down the road. And if you're right, then of course you aren't being foolish.

When people play the beauty-contest game for several rounds against the same group of opponents, the behavior quickly converges to what game theory predicts will happen.

is not what a bunch of college kids do, but whether the people who run large businesses behave according to economic theory. Well, the Caltech Board includes 20 chief executive officers, presidents, and corporate-board chairmen. These titans of industry are the "CEOs" sample. As you can see, none of them picked zero; and if any one of them had, that person would have lost. So they obviously knew who they were playing with. A few of them picked surprisingly high numbers, but the tallest spike is between 30 and 39, and there's another tall spike between 20 and 29. If you do the math, it turns out that they were reasoning about one step further than the other people at the meeting. The numbers they chose are statistically indistinguishable from the numbers the Caltech undergraduates and the econ PhD students chose.

The game-theory prediction was flat-out wrong. The same pattern emerged across three continents, both genders, and a tremendous variation in age, wealth, and educational background.

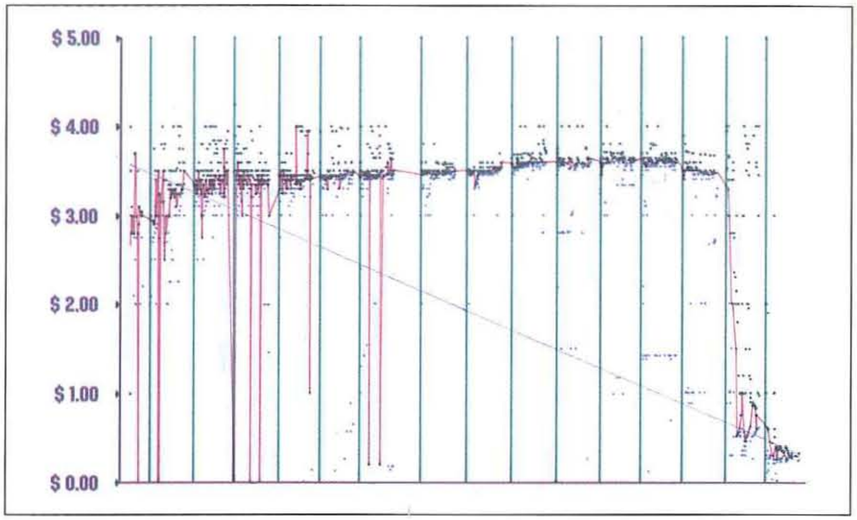
But what happens if we allow people to learn by announcing the winning number and repeating the game? Then we see a steady, slow convergence toward the game-theory prediction. The graph above shows what happened when the Singaporean students played a multi-round version of the game. After 10 rounds, about 50 or 60 percent of the students were choosing numbers between zero and 10. So game theory, which seemed so laughable at first, does predict what people will do with repetition. Again, psychology helps us understand what happens at first, and game theory tells us what will happen eventually as people learn. We need both to understand the entire picture.

This brings me to the stock market. That passage from Keynes describes a market in which investors care about what other investors will buy in the future. Here, you often pay more than a firm is worth, because you think that somebody else will pay even more later on. This strategy is sometimes called the "greater-fool theory," because even though you're a fool to pay as much as you did, you're betting that there's a greater fool just down the road. And if you're right, then of course you aren't being foolish.

Economists call this a bubble. Prices rise simply because people expect them to rise, and it's a self-fulfilling prophecy right up to the moment when the bubble bursts. One famous example is that of tulip bulbs in Holland during the 1600s. People were paying several months' income for rare tulip bulbs. Thoroughbred horses in the 1970s, and L.A. real estate in the 1980s are other examples, as are booms in works by dead artists (who can't produce any more supply). The Japanese economy in the 1980s might be the most spectacular example in world history. However, a business-school professor who teaches about the stock market would probably be reluctant to admit that these episodes are bubbles, in the sense that I've defined the term. I'm asserting that people are consciously paying more than the intrinsic value of the asset, but the professor would probably say that we don't know its intrinsic value. How do you measure the intrinsic value of, say, Van Gogh's *Sunflowers*? Maybe it was a bargain at $50 million. Instead, most of the experts believe in the so-called efficient-market theory, which says that information about a stock's worth will quickly be reflected in its price.

It would be nice if we had an example to convince the experts who believe that markets are efficient. Until a couple of decades ago, people thought that economics, like astronomy, was not an experimental science—all you could do was study the data that the market provided. But in fact, many of the most interesting propositions in economics can be tested experimentally. About 10 years ago, Charles Plott, the Harkness Professor of Economics and Political Science, founded the Experimental Economics and Political Science Laboratory at Caltech. The whole thing is run

In this plot of an experimental market, the horizontal axis is time and the vertical axis is the price per share. Every dot is a proffered transaction; the actual transactions are connected by the red line. The vertical green lines denote the end of each five-minute period, at which point dividends are paid.



by computer and functions very much like a real market. (You can also study elections and other processes in it.) Each participant is isolated in a booth, and cannot communicate with other participants in any way except through the computer. People type in offers to sell x number of shares at such-and-such a price, or bids to buy, and all the offers and bids are displayed on everyone's computer screens. Players consummate trades with the push of a button. The computer records all offers, bids, and transactions sequentially; keeps track of who owns what; and calculates everyone's earnings. (Again, the students get paid real money, so we can be sure that they're taking this seriously and are giving it their best effort.) Everything is recorded as it happens, and software developed by Plott enables us to make a "movie" of how the market behaves, and analyze it in detail.

In these experiments, we created a market for an asset we invented whose value we chose. The students traded a share—a bond, if you will—for 15 five-minute periods. Each share paid a dividend of 24 cents at the end of each period, so if you held on to a share for all 15 periods, you'd earn $3.60. Everyone had a couple of shares to start with, and some money to buy more shares if they wanted to. The question we wanted to answer was, what would the price of the shares be? The efficient-market theory is very clear on this. It says that since everyone knew the share paid a total of $3.60 in dividends (we told them that, by the way—we gave them a table of dividends versus periods remaining), then the price of the share should be $3.60 in the first period. In the second period, the price should drop by 24 cents to $3.36, and so on.

Shown above is what real traders did in a typical experiment. The slanting purple line shows the shares' declining dividend value. Each dot is an attempt to sell or buy; all the completed transactions are connected by the red line. Dots above the red line are sellers asking too much, and dots below the line are buyers offering too little. Notice that the price remains flat at around $3.50—even close to the end, where the efficient-market theory says the shares are worth less than a buck. (This is like those of you who bought a house in L.A. a few years ago, and refused to sell as the market collapsed.) The traders are trying to forecast whether the market will crash, or whether some nut will buy shares that are about to expire. And finally, of course, the market collapses.

We know that everyone knew a share's intrinsic value because we gave them a quiz before the first trading period began, so this is the clearest example of a bubble that you could possibly have. When we asked the subjects how it came about, they'd tell us a story that sounded very much like the greater-fool theory. They'd say, sure I knew the prices were way too high, but I saw other people buying and selling at high prices. I figured I could buy, collect a dividend or two, and then sell at the same price to some other idiot. And, of course, some of them were right. As long as they got out before the crash, they earned a lot of money at the expense of the poor folks who were left holding the bag.

We can see harbingers of the crash in what we've come to call nervousness in the market. Near the end, some people who think that the market has lost its mind will make extremely low bids. These people probably know that a lowball bid of a dollar won't be accepted when the going rate is three times that, so we think this is their way of expressing their surprise and warning everybody. It's the same as when somebody offers you $350,000 for the house you're desperately trying to sell for the half million you paid for it a few years ago. This is their way of politely saying you're nuts—your house isn't worth half a million.

After doing a number of such experiments, we've learned how to turn these bubbles on and off. To turn the bubble off, we bring the same group of subjects back and run the entire 15-

The traders are trying to forecast whether the market will crash, or whether some nut will buy shares that are about to expire.

period market again. We usually see a smaller rise that crashes much earlier. And if we bring that same group back a third time, we hardly get any bubble at all. The market-price line now follows the intrinsic-value line very closely, so experienced traders do obey the efficient-markets theory. We can turn a bubble on by having had our subjects participate in a previous experiment in which we created inflation by adding money to the economy, just the way the government does. If prices rose in that earlier market—if they've lived through an inflationary experience—then we've planted a belief in their minds that prices will rise, like seeding clouds to make rain. Then, when we put them in the bubble experiment, prices *do* rise, because of this self-fulfilling prophecy based on their common experience. We don't always see bubbles—sometimes we see just what the efficient-market theory predicts, with prices sliding down along the intrinsic-value line. But bubbles are very common—the several of us doing this kind of research have observed about a hundred of them.

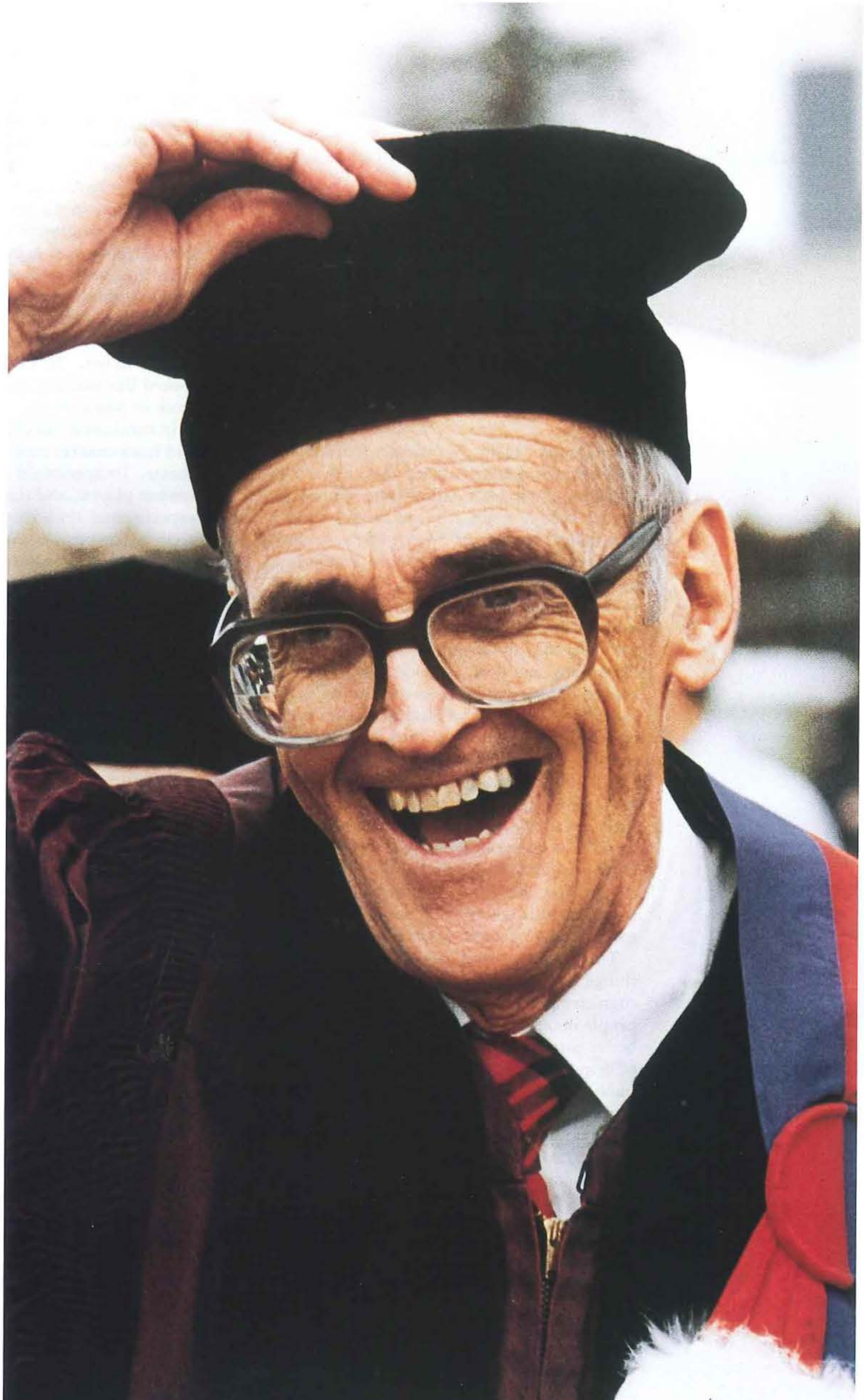
This research is very new, and there are many things we have yet to learn. We need help from cognitive psychology to understand what the people in our experiments are thinking. We need better pattern-recognition and data-analysis tools to help us look at the data and forecast when bubbles will start and crashes occur. Compared to other experimental sciences like physics, chemistry, and biology, the amount of work that's been done in experimental economics is relatively modest.

What does all this mean in the real world? Perhaps one-third of the market's trading volume is due to a handful of mutual funds and other large institutions. These portfolio managers may not behave rationally, either, although for other reasons. For example, they operate in a world where if they have one bad quarter—worse than everyone else—they may get fired. So they ask their colleagues, what are you guys buying? They want to buy what the other guys buy, so they don't finish last. That, again, is very much like a Keynesian beauty contest, and I think the prevailing theories need to address it. Peter Bossaerts, an associate professor of finance here at Caltech, is actually working on this now. I should also point out that nothing I've said addresses the issue of stocks that haven't paid dividends yet, but may at some time in the future. This is a very common situation with growth stocks, such as those of startup companies in biotechnology, software, and other high-tech fields. The closest we've come to studying those was a couple of experiments where the dividend wasn't guaranteed—there was a large chance you'd get nothing, and a small chance you'd win big. We did see some things that looked like bubbles, but we haven't done much work in that area yet.

In conclusion, cabdrivers, beauty-contest games, and stock-market experiments have a common theme. Inexperienced cabdrivers, novice beauty-contest players, and traders participating in an experimental stock market for the first time don't seem to conform to standard economic theory, which assumes complete rationality by all participants. However, their actions are reasonably well explained by psychological theories that allow people to have normal, limited reasoning ability, and limited faith in others. The subjects of these experiments aren't dumb, but they're not perfectly brilliant, either, and they're not willing to bet a lot of money that other people are. But the behavior of experienced drivers, players who play the beauty contest over and over again, and traders who return to the stock market is often explained quite well by economic theories. Experimental observations help us figure out which theories are true, and which are false, and under what conditions. So we think that combining the best ideas in psychology and economics will make for the best social science of all. □

Colin F. Camerer, the Axline Professor of Business Economics, studies corporate strategy, decision sciences, and experimental economics. Camerer earned his BA in quantitative studies from Johns Hopkins University in 1976. He got an MBA from the University of Chicago in 1979, followed by a PhD in behavioral decision theory in 1981. He arrived at Caltech as the Axline Professor in 1994. He is now taking advantage of Caltech's proximity to Santa Anita to research an upcoming article to be titled "Ambiguity in Betting on Unraced Thoroughbred Horses," but his interest is purely academic—he does all his retirement investing through TIAA-CREF.

The beauty-contest and bubble work was supported by the National Science Foundation. The taxi-driver study was supported by the Russell Sage Foundation. This article is adapted from a Watson lecture.



"We'll get the lead out of the iron meteorite. You measure its isotopic composition and you stick it into the equation. And you'll be famous, because you will have measured the age of the Earth." —***Harrison Brown***

I said, "Good, I will do that." And he said, "It will be duck soup, Patterson."

Duck Soup and Lead

Oral History — Clair C. Patterson

Clair C. "Pat" Patterson, who first measured the age of the Earth (4.6 billion years) and whose work on the ubiquitous high levels of lead played a significant role in the passage of the Clean Air Act of 1970 and in the removal of lead from a number of substances including gasoline, died December 5, 1995.

He had come to Caltech as a research fellow in 1952, eventually becoming a senior research associate, and finally accepting a professorship (he didn't believe tenure should be a scientist's goal, just one of his many iconoclastic views) only in 1989, three years before he retired. He was awarded the $150,000 Tyler Prize for Environmental Achievement in 1995.

The following article is adapted from interviews conducted in March 1995 by Shirley Cohen for the Caltech Archives Oral History Project. It has been edited and condensed, and some portions, for want of space, have had to be omitted completely. Patterson begins his story with his childhood in Mitchellville, Iowa, in the farmland outside Des Moines, where he was born in 1922. His father was a rural mail carrier; his mother, who was a member of the school board, gave him his first chemistry set. He earned his AB in chemistry in 1943 at Grinnell College, where he also met his wife Lorna (Laurie). A year later he got his MS, also in chemistry, from the University of Iowa, which is where we join the story.

Left: Clair Patterson was photographed at Caltech's 1993 Commencement by Yigal Erel, his last graduate student, who was awarded his PhD at that ceremony. Right: Laurie and Clair Patterson at their own graduation from Grinnell College in 1943.

Clair Patterson: I got a master's degree in nine months. Then there was a chemistry professor at the University of Iowa who said, "Patterson, you've got to go to the University of Chicago and work on the atomic bomb."

Shirley Cohen: But he wouldn't have known about the atomic bomb then?

CP: Yes, because that's where he went.

SC: I thought it was such a big secret.

CP: Oh, it was only a secret to chimpanzees who didn't know what they were doing. He was going to the University of Chicago to work on the atomic bomb, and he wanted to take me along. I had become a spectroscopist. I had done research in molecular spectra at the University of Iowa for my nine-months, whiz-bang, master's degree. While I was there I got into atomic spectra a little bit. So now, at the University of Chicago I was doing atomic-emission spectroscopy. They were analyzing the various products of the uranium when it disintegrated.

[Here Laurie Patterson fills in some gaps: "Pat and I left Iowa City to work on the Manhattan Project in the spring of 1944, soon after we were married. We lived in an apartment hotel across the street from the Museum of Science and Industry in Chicago. Pat became more and more unhappy at being in the city, doing work we both felt would let the 'genie out of the bottle' much too soon. We went to Iowa for a weekend late in the summer of 1944 in order for Pat to enlist in the army. (He had been rejected because of nearsightedness during our senior year in college, when all but 19 men of our graduating class left Grinnell to go into the services.) The physical requirements had been lowered, and he felt he would be accepted at this time. He set this in motion with his draft board; I enlisted in the Waves. Three days later his draft board reported that they could not draft him

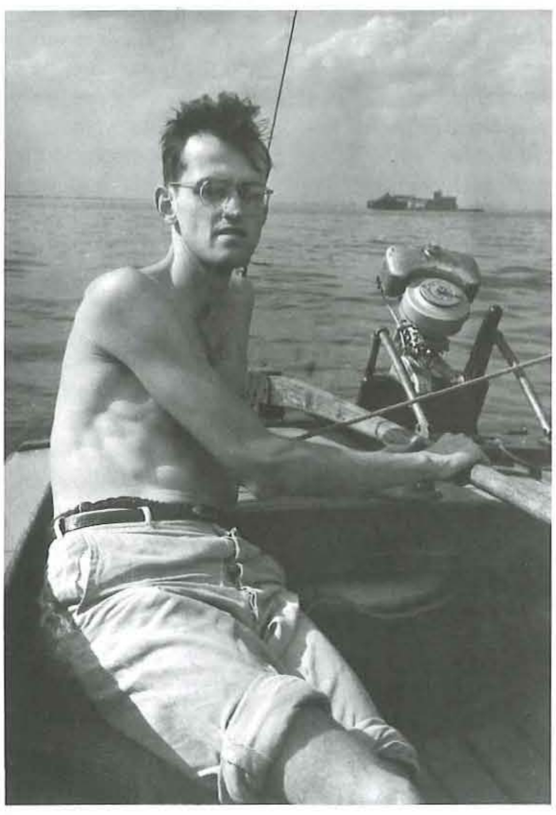
because of his high security rating and he must return to the University of Chicago. Fortunately, I had not turned in the final papers and was not yet formally a Wave. We returned and were asked to meet with the colonel in charge of the Manhattan Project at 5th Army Headquarters. He suggested that he send us to Oak Ridge, where there were many young people. Pat felt he was the only young male on the streets of Chicago and that he was a 'draft dodger.'"}

So we went down to Oak Ridge, and that's where we spent another year and a half or two years, working at the uranium 235 electromagnetic separation plant. At Oak Ridge I got into mass spectrometers. You see, the isotope of uranium that they wanted was uranium 235, which is what you made the nuclear bomb out of. But 99.9 percent of the original uranium was uranium 238, and you couldn't make a bomb out of that. But the little tiny bit that was U^{235} had a different mass, and you could separate them using a mass spectrometer.

SC: Is this where you met Harrison Brown?

CP: No, he was there, but I didn't meet him until after the war. Harold Urey wasn't there but he had done the theoretical work at Columbia that the Oak Ridge diffusion plant was based on. (There were both electromagnetic separation and diffusion separation at Oak Ridge.) That's where Urey got his ideas about isotopic fractionation being a function of temperatures. This was the insight that enabled him to develop the concept that led to what we call paleotemperatures—the measurement of temperatures 200 million years ago. This is Sam Epstein's stuff. *{Epstein is now the William E. Leonhard Professor of Geology, Emeritus.}*

The Pattersons enjoyed escaping to the Cumberlands from nearby Oak Ridge, Tennessee (1945).



The University of Chicago wasn't all work and no play. Here Patterson sails on Lake Michigan in 1947.

These guys during the war developed these concepts, you see, and they kept them on the shelf. They knew that they were working as engineers on a hideous weapon of warfare. They were the same types as my mentor at the University of Iowa who told me, "Patterson, we are saving democracy for the world against fascism." These professor-mentors, who were no longer at the university but working on the bomb project, told young people like me that this was the thing to do—this hideous crime we were committing was a necessary thing.

I didn't think this way then, only later. But during that time I learned a lot of new ideas and concepts and patterns of thinking. So when the war ended, I said, "I want to go back to the university. I love the University of Chicago. I'm going to go there and I'm going to get my PhD in science and study some of this important stuff."

Harold Urey, Harrison Brown, and all these guys flocked back to the University of Chicago. And all these ideas that had been cooking around in their minds during the war then came to fruition as goals. This had nothing to do with making the atomic bomb. These were scientific concepts that dealt with atomic physics and chemistry.

So when I went to the University of Chicago, Brown found out about me *{Brown became Patterson's research adviser}* and said, "Hey Pat, you're familiar with mass spectrometers. Now, there's this other youngster, George Tilton. What we're going to do is learn how to measure the geologic ages of a common mineral that's about the size of a head of a pin. It's called zircon." Little tiny zircon crystals occur as a minute trace constituent of common ordinary igneous rocks.

The laboratory proved too small to contain Harrison Brown, who eventually became professor of science and government as well as professor of geochemistry.

When those rocks crystallize and form from magma, a lot of different crystals form in there, and among them are tiny bits and pieces of zircon. They have uranium but no lead. And as they sit there, the uranium decays to lead, and you can do uranium-lead measurements. However, the amounts of uranium in there are only a few parts per million, and that's decayed to even smaller parts of lead.

What Brown wanted Tilton and me to do was to develop mass spectrometric techniques to measure amounts of uranium and study the isotopic compositions of amounts of lead that were a thousand times smaller than anything anyone had ever looked at before.

Three different methods had been developed for measuring ages: the uranium-lead age, the potassium-argon age, and the strontium-rubidium age. I didn't work on the latter two, only on the uranium-lead. I was the lead man, and Tilton was the uranium man.

If we only knew what the isotopic composition of primordial lead was in the Earth at the time it was formed, we could take that number and stick it into this marvelous equation that the atomic physicists had worked out. And you could turn the crank and blip—out would come the age of the Earth.

Tilton only had to measure concentrations. I had to measure isotopic compositions, and that is different. And Brown said, "Well, Pat, here's the deal. Once you do that, then here's what you do." Brown had worked out this concept that the lead in iron meteorites was the kind of lead that was in the solar system when it was first formed, and that it was preserved in iron meteorites without change from the uranium decay, because there is no uranium in iron meteorites. Now, this is crucial because when other parts of the solar disk of the planets were forming—for example, the Earth—they took in both lead and uranium. Therefore the lead in the Earth today is a mixture of two things: the primordial lead that was there in the beginning, and the lead that has been created by uranium decay since the Earth was formed.

There are two isotopes of uranium that decayed to two different isotopes of lead, and there's also thorium, which decays to another isotope of lead. So you have three different isotopes of lead. And the whole thing gets mixed up. You've got all these separate age equations for the different isotopes of uranium and different isotopes of lead that were formed. And it was not known what the isotopic composition of lead was in proportion to these different isotopes in the Earth when it was first formed. If we only knew what the isotopic composition of primordial lead was in the Earth at the time it was formed, we could take that number and stick in into this marvelous equation that the atomic physicists had worked out. And you could turn the crank and blip—out would come the age of the Earth.

So Brown said, "Pat, after you figure out how to do the isotopic composition of these zircons, you will then know how to get the lead—you will have it all set up. You just go in and get an iron meteorite—I'll get it for you. We'll get the lead out of the iron meteorite. You measure its isotopic composition and you stick it into the equation. And you'll be famous, because you will have measured the age of the Earth."

I said, "Good, I will do that." And he said, "It will be duck soup, Patterson."

SC: Did he ever work in the lab with you?

CP: No. He came in one time. He was trying to show us something. It blew up in his face. He had to stick his head under the water faucet. He was better out of the lab.

It was a very fruitful type of pioneering work. But I hadn't yet gotten to the measurement of the age of the Earth and studying the lead in iron meteorites, which Harrison had told me was duck soup five years earlier.

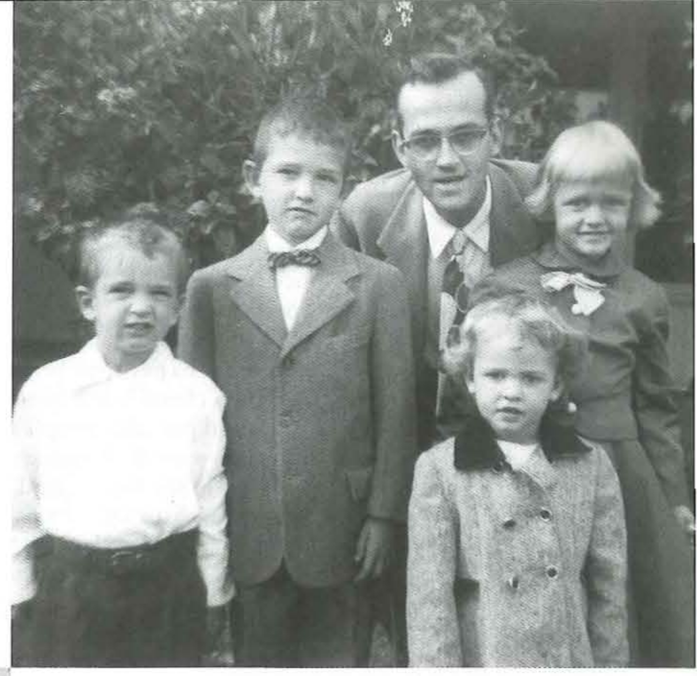
Patterson uses a distillation apparatus to purify reagents in his Caltech lab in 1957. He didn't trust the purity of commercial chemicals, so he redistilled them.

I became aware of the contamination problem because I kept getting the wrong answer for lead in these zircons. We knew what the amount of lead should be because we knew the age of the rock from which it came and because of George Tilton's measurement of the amount of tiny bits of uranium in there. We could calculate how much lead there should be and what its isotopic composition should be. And it kept coming up with the wrong number, so I had to figure out why—to go to all these sources for different possibilities. That's how I found out all about this contamination—that there was lead coming from everywhere. And in the process of finding this out, I learned how to analyze very low concentrations of lead in everything—in common ordinary things like hair, clothing—that people had never thought about.

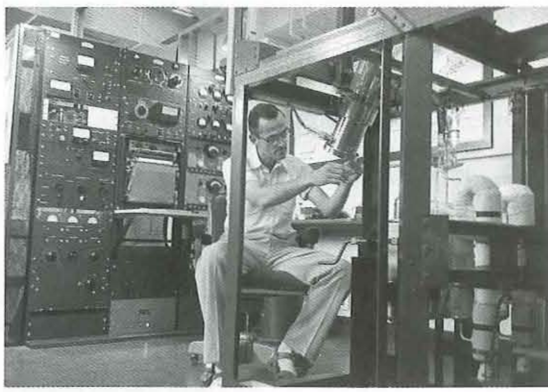
SC: I'd like to talk a little bit more about your interaction with Harrison Brown over the years that you were his student.

CP: Well, I got my PhD at the University of Chicago after five or six years of work there. George Tilton and I published a paper on how to determine the ages of the tiny zircon crystals in the rocks. This was very important because it is one of the three major methods that were subsequently used to delineate the geologic history of the Earth. It was a very fruitful type of pioneering work. But I hadn't yet gotten to the measurement of the age of the Earth and studying the lead in iron meteorites, which Harrison had told me was duck soup five years earlier.

When I was finishing my PhD, I said, "Well, Harrison, I really would like to continue this work and measure the age of the Earth and get the lead out of the meteorite, but I need to work as a postdoc here at Chicago to do that." And he said, "OK, Pat, go ahead." The Atomic Energy Commission had financed George's and my work for



Right: Patterson and his family in 1955—Charles, Cameron, Claire, and, in front, Susan. Below: 1957—Patterson loads a sample for analysis in the mass spectrometer.



the five previous years, and since they had financed us for work that led up to this, I wrote a new proposal. They turned it down. They said they weren't interested in measuring the age of the Earth.

I cried on Harrison's shoulder. He said, "Pat, that's all right. I'll rewrite your proposal in my name." And you know, he's very good at explaining things to people in a nonscientific way. So he rewrote the darn thing. Boom. I was awarded a postdoc fellowship. I did about half the work in one year in getting lead out of meteorites to do this, and then Harrison got offered a big job here at Caltech in the geology department. And he brought me along with him.

SC: And you just said yes?

CP: Yes, of course. Because I wanted to continue this work. After he came, he got more money out of the Atomic Energy Commission to build a mass spectrometer here, to build me a laboratory to work in. And in that laboratory I isolated iron-meteorite lead. But I didn't have a mass spectrometer built yet, so I flew back to the University of Chicago and used their new one to measure this stuff. It was wonderful. And that was the data that I used to publish this paper delineating the measurement of the age of the Earth. This was in 1953. It was the first measurement of the age of the Earth to be published.

It was not understood by the geological scientific community at all—how this was done or anything about its meaning or significance. It was a dozen years before this number got into the geology textbooks. Before then the age of the Earth was very vague—some billions of years. Then when the correct number began to appear in the geology textbooks, they never said how it was determined, only that it was due to uranium-lead geochronological measurements.

But what was said was incorrect, of course. It wasn't until maybe 10 or 15 years later that a few of my colleagues were able to really do this correctly. You must recognize that this number that I had measured related to the time of the coalescing of this planet out of the solar disk. Now, that is a finite period of time. But do you have a billion years for this to take place—which is a substantial fraction of the time that's passed since the Earth was formed—or was it just a very short time? Well, they were working on that. And I didn't give two hoots for that. My attitude was: "I don't want to work on that stuff anymore. What I want to work on is the evolution of the Earth—what happened to the Earth itself during the time it was coalescing."

Now, the reason I could do that was that we could use lead isotopes as measures. But the isotopic composition of the lead was changing—it was dynamic, because uranium was decaying all the time, and there were three radioactive progenitors of three different isotopes in this lead that were being added all the time the Earth was there. These parts were moving around all over the place, and uranium and thorium were being separated from each other and from lead, due to the different chemical properties in these different components that were moving around. Some had sulfur, some had oxygen, some had silicon. And these different components would grab onto different chemical strengths of the lead, uranium, and thorium, and segregate them in different parts, so that the proportions of lead and uranium and thorium would change for millions and hundreds of millions of years at different areas. The lead within would have a different isotopic composition. And you could track this.

So, today, you could look at the lead in rocks and begin to put together a picture of how they had been chemically related in past times. And you could get other people's work to help you interpret what that chemistry meant in terms of position in the Earth. And then we could get times. So that's what I was interested in, what I started out doing. I said to hell with this damn stuff about cosmology of the sun to the planets. There were other people who were very interested in that, so they worked their hearts out to prove I was wrong.

SC: They didn't like your number?

CP: No, no. They wanted to make discoveries. And in order to make discoveries, you can't just prove somebody's right. You want to prove that somebody's wrong.

SC: But meanwhile, you were getting grants to do this.

CP: No, Harrison was getting them. Let's get back to Harrison. In all this time, I was trying to shift back to using lead isotopes. Now, in order to do that, believe it or not, Harrison got money from the Atomic Energy Commission to do this kind of work at Caltech. He was talking about, oh, how my work was related to uranium, of course. He went through all these calculations, and he told the Atomic Energy Commission how there was enough uranium in ordinary igneous rock, that if you ground that rock up and then leached it with hydrochloric acid, you would get enough uranium to use in an atomic generator that would be equivalent in energy to 10,000 tons of coal. In other words, 10,000 tons of coal would equal the amount of energy of the uranium in one ton of granite.

SC: And they bought that?

CP: They bought that! It was that kind of sales pitch he used. Now listen, you know what I would say. I would say, "Well, I want to know how this chunk of North America evolved and then got thrown around and came over here, and how this other chunk came up later. And we want to know when this chunk came up and when that chunk came up, and how they were related to each other, what their ancestry was." And the Atomic Energy Commission would say to me, "To hell with you, Patterson! We don't care about that stuff at all."

SC: You have to know how to do it.

CP: Yes, but that's the way I would write my proposals. And I never got funded. Harrison would get them funded for me. I stopped interacting with him when he moved over to Baxter Hall *{as of 1967 Brown held a joint appointment as professor of science and government and professor of geochemistry}* and I had to start getting my own money, which I failed at, of course. But Brown protected me.

There was an important phase here in Brown's helping me get money. He shifted from the Atomic Energy Commission after about four or five years; he had a new idea. I was studying sediments. In order to figure out what was happening in the past, I would have to get oceanic sediments. You see, the rocks would erode (they'd have lead in them), and then they would form sediments, and you could measure the age of the sediments. I wanted to sample all the continents of different times, and the oceans were a mixing reservoir. And we would look at this mixture in the sediments as a function of time.

And Harrison said—he was a brilliant guy politically—"Oh, heck, the oil companies should be interested in this." Why? "Well, the isotopic composition of the lead is a tracer that helps identify the stage, or the age, to characterize the time or the type of the sediment that you have." So he convinced the oil companies that they should finance my research because it would assist them in identifying oil deposits. You know, when you drill a core, you're looking at bands in a rock. And if you measure the lead isotopes in there, it can give you more information than you had before. It could help characterize the type of sediment, so it could help you locate and identify oil deposits and reservoirs here and there. So they started. It was a national consortium of oil companies that had this big research fund that they doled out to help them do this stuff. Harrison got money from them

I want to know how this chunk of North America evolved and then got thrown around and came over here, and how this other chunk came up later.

1971

every year, huge amounts, to fund the operation of my laboratory, which had nothing whatsoever to do with oil in any way, shape, or form.

SC: That's called basic research.

CP: And then a very bad thing happened. We found from measuring lead in these sediments how much lead had been passing through the oceans and depositing in these sediments. Now, there are two kinds of lead: there's a soluble lead that's in the water, and then there's lead in particles. These particles are what the sediments are made out of. However, a small fraction of sediments are made out of residues of organisms that are living in the water—like zooplankton poop. And they fall down through four miles of water!

When the zooplankton residues got into the sediments down there, there'd be a chemical reconstitution, what we call "formation of autogenic minerals." It would be a rearrangement of that stuff over a few thousand years into lattices of minerals. You could get the lead out of those minerals by just taking a piece of sediment and treating it very gently with a little diluted acid. But you wouldn't get the lead in the clay particles that had migrated out from the rivers and then fallen down and formed the bulk of the sediments. You wouldn't touch that lead. You'd only get this little tiny amount of lead that had been in the zooplankton, because that soluble lead collects on the outside of their little bodies.

The drilling project was a Scripps *{Institution of Oceanography}* operation, a gigantic one. I don't know whether this was paid for by the government or by the oil companies, or both. They would lower drills down through four miles of water, and then they would bring these cores back up. The cores were down at Scripps, and we got segments of that stuff. They came from all over the Pacific. There was a pattern scattered throughout the basin of the Pacific, so we could see what was coming from China, what was coming from North America, what was coming from South America at different times. Different stuff came at different times, depending on the climate. Now, this isn't over thousands of years, or even hundreds of thousands of years. I mean, we're talking about millions of years.

In addition to dating this stuff, Ed Goldberg at Scripps and a guy by the name of Gustaf Arrhenius (who's the grandson of the famous Swedish chemist Arrhenius) had worked out this autogenic mineral business. They didn't study lead, but they knew that this mineral was made of reconstituted plankton poop. They had identified this mineral, which nobody had done before. I knew that when I leached it with acid, I'd been taking their mineral and getting the lead out, which came from the soluble lead.

When we measured that, using the ages of the sediments, we had a measurement of the rate of the past flow of lead through the oceans all over—and this was millions of years ago. So we knew the quantity, the rate per square centimeter of sediment surface (the bottom of the ocean), grams per square centimeter per year. We knew how much was flowing through there.

Then I got some data from the rivers. There were these idiots who were measuring lead in river waters who didn't know anything about how to measure lead. And I knew that because I had previously worked out how to do the measurements for meteorites. But I took their data for river water, and I multiplied by all the rivers how much river water there is in the oceans each year. And I came out with a number for lead that was a hundred times greater than the amount that we had measured that was flowing through the oceans in the past. I thought, something is wrong here. Are these guys wrong? Or is there really that much lead coming into the oceans today?

At that time we were working out methods for

We collected the snow and brought it back here and we analyzed it and found huge concentrations of lead increasing since the 1700s until now—a 200- or 300-fold increase in the concentrations of lead.

taking what we call a profile. A ship would sit there and it would lower something and grab some water, hoist it up, and then it would lower it deeper and hoist it up. We'd collect water up and down for vertical miles, and we'd measure it.

SC: Did you have somebody on the ship doing this? You weren't on the boat yourself?

CP: Yes I was! And I got sicker than a dog!

Now, in these profiles, you look at how the concentration changes with depth. We found a huge increase in the upper portions of the oceans, which decreased to lower concentrations with depth. Now, why is that? Why should the lead be so high? The waters don't mix that rapidly. And the waters up near the top are much younger than the waters lower down. It takes a long, long time for them to mix.

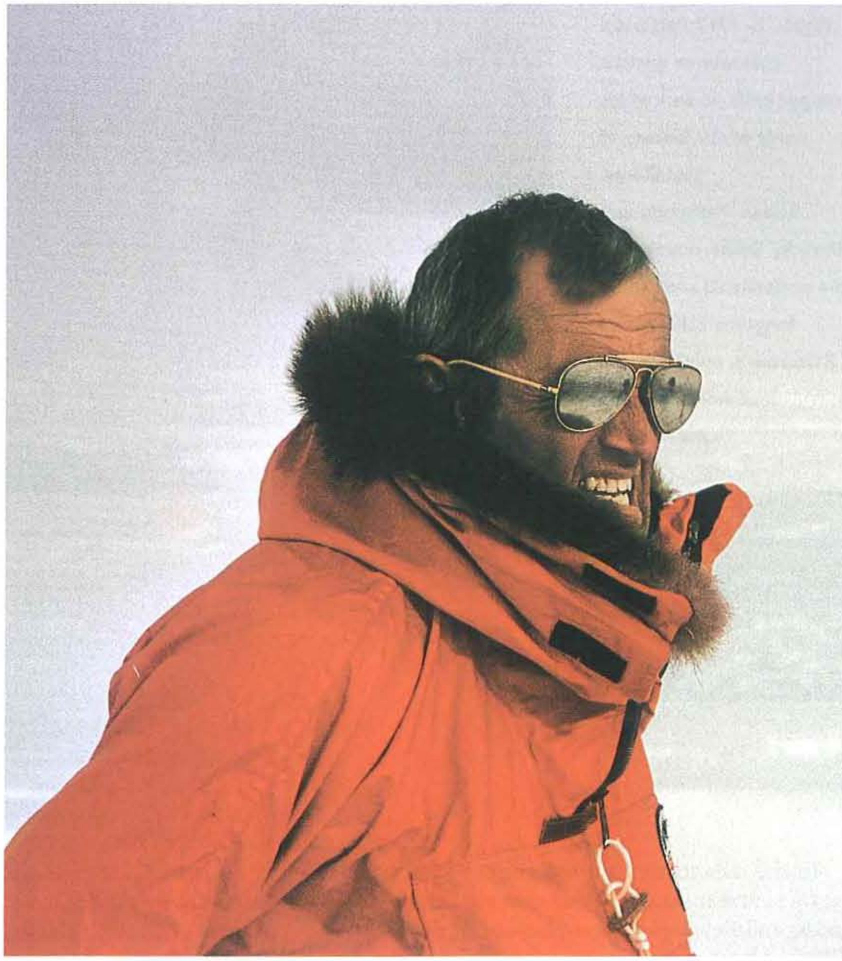
So I made some calculations. What about the lead in gasoline? We extrapolated from our profiles how much lead was in the upper part of all the world's oceans, and it could easily be accounted for by the amount of lead that was put into gasoline and burned and put in the atmosphere. We had more tons put in the atmosphere from the tetraethyl lead added to gasoline than we could see in the upper part of the world's oceans. And that's what caused the problem. The oil companies were financing my work. We're in serious trouble.

SC: Even Harrison Brown would have trouble with that one.

CP: Oh, he did! And that's when he disassociated himself from me. He stopped getting money from the oil companies, and I had to start getting it myself. I needed money because since I got this idea about lead coming from gasoline, I wanted to look at the record. Where do you see that record? You see it in the snow that never melts in the polar regions. It comes out of the air that has lead in it. Lead is in the snowflakes. It goes down and you have a layer there. Next year you have another layer.

I started working at the North Pole. We had to collect the concentrations of lead in the snow there; it was a thousand times lower than the concentrations of lead in the "pure" laboratory water in most laboratories. I had to measure concentrations that they couldn't measure. In other words, these scientists were using the purest water in the laboratories as a baseline, and they couldn't go below that level. I had to see variations with time, and the techniques for doing that at these levels were not at all developed. I required a two-foot cubic block of snow. And then we had to dig shafts down 200 or 300 meters to get these blocks of ice over a period of time to see what was happening. We sawed even larger blocks out of the walls of a tunnel, while wearing acid-clean plastic gloves and suits and using clean saws. We collected the snow and brought it back here and we analyzed it and found huge concentrations of lead increasing since the 1700s until now—a 200- or 300-fold increase in the concentrations of lead.

About the same time I had proposed a concept that I call biopurification, which has to do with what the natural level of lead should be in people. You start out by looking at the calcium in our bodies and asking where it comes from. You track it back—you look at the food that we eat, you look at the organisms that made that food, and you keep going down the food chain until you come to plants. Then you go down to the soil, and you go from there to the rocks that the soil came from. It so happens that there are calcium-like trace metals such as barium but in very small abundances—a tiny, tiny, infinitesimal trace. Barium is like calcium but it's chemically different. For one thing, it's a very massive atom. It has different chemical properties than calcium—not grossly different, just enough different so that barium is poisonous as hell. When we evolved through all these millions of years of evolution,



On their Antarctic expedition in 1966, Patterson and his crew sawed large blocks of snow from shafts 300 feet deep and from tunnels, then shipped them back to Caltech to analyze. They found an enormous increase in concentrations of lead since the Industrial Revolution.

nature devised an exclusion mechanism for handling this. Each one of the organisms in this food chain has to have calcium, but nature evolved a process for the exclusion of barium when they take up calcium. There are certain kinds of proteins that grab hold of the calcium and pull it, and they don't do that efficiently for barium. So there's an enormous reduction of the barium-to-calcium ratio.

Lead comes in with the calcium too. And it's distributed in the body much like calcium (not on a molecular basis—there are different proteins) but in a general, morphological distribution. Ninety-nine percent of it is in our bones.

I knew what the lead/calcium ratio is in average people today. (The people who measured this couldn't measure lead properly in bones, but I used their data anyway.) So I had the ratio going from rocks to food to people. And do you know, the ratio of lead to calcium in people was about the same as that in rocks?

Now, I compared that with barium. I got this data from my old, evil, atomic-bomb people; they measured barium in our food and in our bodies because they were measuring radioactive barium. And you know what? The barium-to-calcium ratio in rocks dropped in our food and in us by a factor of a hundred. The barium ratio shows that lead should be a hundred times less than it actually is in us today. We are being poisoned by lead. And guess where it is coming from? Look at the ocean—that's coming from tetraethyl lead.

Others thought this was a pile of crap! They said, "Patterson, would you please start worrying about science instead of this health crap. What a waste! Here you are; you measured the age of the Earth, and you're worrying about tetraethyl lead and this stupid stuff about lead in bones." But I was right. When we finally actually measured the lead ratio—it took about 25 years to do this accurately—it turned out to be a factor of a thousand instead of a hundred. I was off—the wrong way.

{In the last 15 years Patterson came back to work on the "lead content of people," using the barium/lead/calcium ratios to analyze the bones of current cadavers and those of ancient buried bones. After accounting for the absorption of lead from soil moisture in the buried bones, Patterson arrived at a "natural" lead level in human bones that was a thousand times less than the level in our bones today.}

Right: In 1972 Patterson examines an ancient copper knife as part of his study of the history of metallurgy.
Below: Patterson and Dorothy Settle, member of the professional staff and a longtime colleague of Patterson's, point out the lead solder on tuna cans in 1980.

In the meantime I started going to the poles to get ice, and measuring lead coming out of volcanoes, and developing complicated devices for getting seawater.

SC: What was your motivation at this point? Were you thinking in an environmental sense?

CP: No, I was not! Science, science, science! I wanted to know what this natural level of lead is. I didn't care two hoots about verifying what the contamination was. I was forced to measure the contamination in order to arrive at the natural level. But there were friends and colleagues who were environmentalists, and they used my work. My work was used to get the lead out of gasoline. As a matter of fact, I wrote a paper on this biopurification concept where I said that we have a hundred times more lead than we should have. And that's when I really got shot down by the oil companies.

I got some money from the National Science Foundation for quite a while. And then I shifted to the National Institutes of Health and the Department of Health, Education, and Welfare and that sort of stuff. And then there was the International Geophysical Year. I had colleagues who were not working with lead, but who were working with other things that were related to lead. And we put in proposals together to get support for sample-collection procedures, the costs afield, and then I could add my laboratory and my salary and my visiting colleagues.

SC: So it was a lot of cooperation with a lot of people.

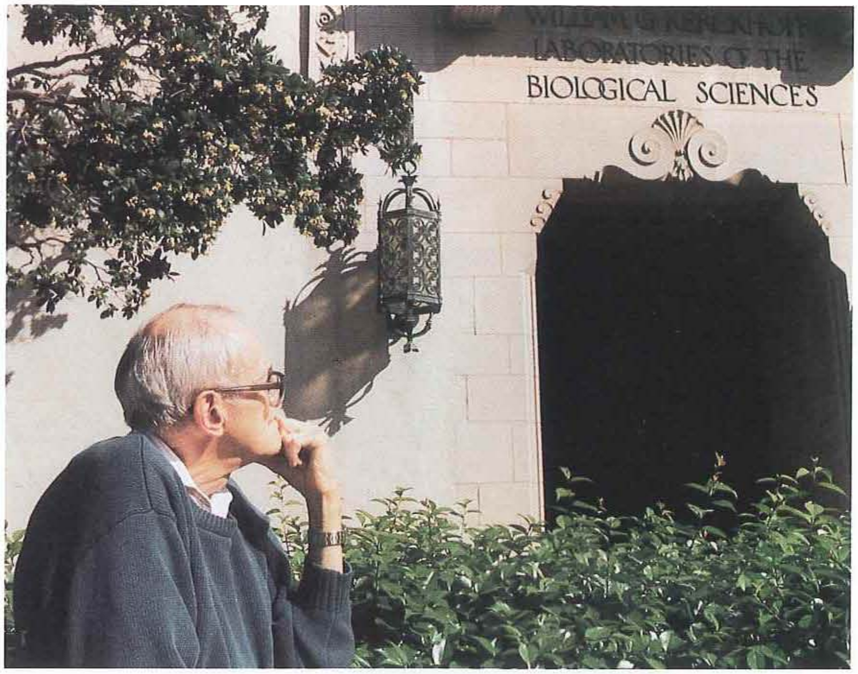
CP: Yes. Then they could use my findings as a glowing example of what was being done by these cooperative research projects. Because other people couldn't measure these numbers, they had to come to my laboratory from all over the world to find out how to do that.

We continued measuring the lead, not just in oceans, volcanoes, and the poles, but also the land areas—plants, animals, high mountains. And finally we got a picture that confirmed and clearly showed that Earth's entire biosphere was heavily contaminated with industrial lead emitted into the atmosphere from smelters and from automobile exhaust. Urban areas were further polluted by other sources of lead being moved around—solder in tin cans and so on.

At the same time, I had been investigating the history of the production of lead. I went back 9,000 years to when metallurgy began; lead came into the picture two-thirds of the way along. And then I showed how lead was related to the development of coinage in metal. I made quantitative calculations based upon data that were available from historical records of production of lead. I worked out ways to estimate from ancient data how much lead had been mined by the Greeks and Romans. I had to figure out the rate of production indirectly, about five different ways, including figuring out the half-life of coins.

I got this curve for the production of lead, which I published about 20 years ago. Some of my colleagues that I'd been teaching how to analyze lead in polar snows got together to work on this. I had dug shafts in the snow back to around the Industrial Revolution but I wasn't able to go back 2,000 years to the time of the Romans and Greeks, so I couldn't measure the lead concentrations then. But these French and Australian colleagues used new techniques that we had devel-

The Institute has a whole lot of things wrong with it—everything has something wrong with it. But because this Institute existed, I existed.



1995

oped in my laboratory to do that. *{This work was published in 1994.}* Guess what their curve was as a function of time? It fell right on top of my lead-production curve.

Do you think I was proud? No! You know what I said? "This proves that for 2,000 years we have been unable to understand the evil that we are doing to ourselves and the biosphere." Because, you see, this lead was coming out of the Greek and Roman smelters into the atmosphere, going around the Earth, part of it working its way up, and was incorporated in the snow that fell at the North Pole.

Many of my colleagues immediately jumped on *{the evidence of the increased level of lead in human bones}*—not the people working with me but scientists who were concerned about the environment and about people being hurt by all this. They wanted to use this information, and they did, to reduce the effects of lead on people and the environment today.

But I myself asked—what is the meaning of this? How did we think? What led us to poison the Earth's biosphere with lead? I therefore shifted to trying to figure out how we thought, and this is where I evolved this new concept of human consciousness in terms of pathways, neuronal circuitries, that are used within the brain to think in two different modes.

{It was, in particular, his work on Roman lead production —"the relationships between social interactions and the metallurgical technologies"— that led Patterson in this direction. His concept involved what he called a utilitarian, or problem-solving, type of thinking, and nonutilitarian thinking, which includes religion, art, philosophy, history, and science, in which "you're trying to understand; the individual brain becomes aware of something and it asks why?" He viewed the latter as less hard-wired in the brain than utilitarian thinking, although it would have had an evolutionary role in conferring advantage on tribes who thought this way.}

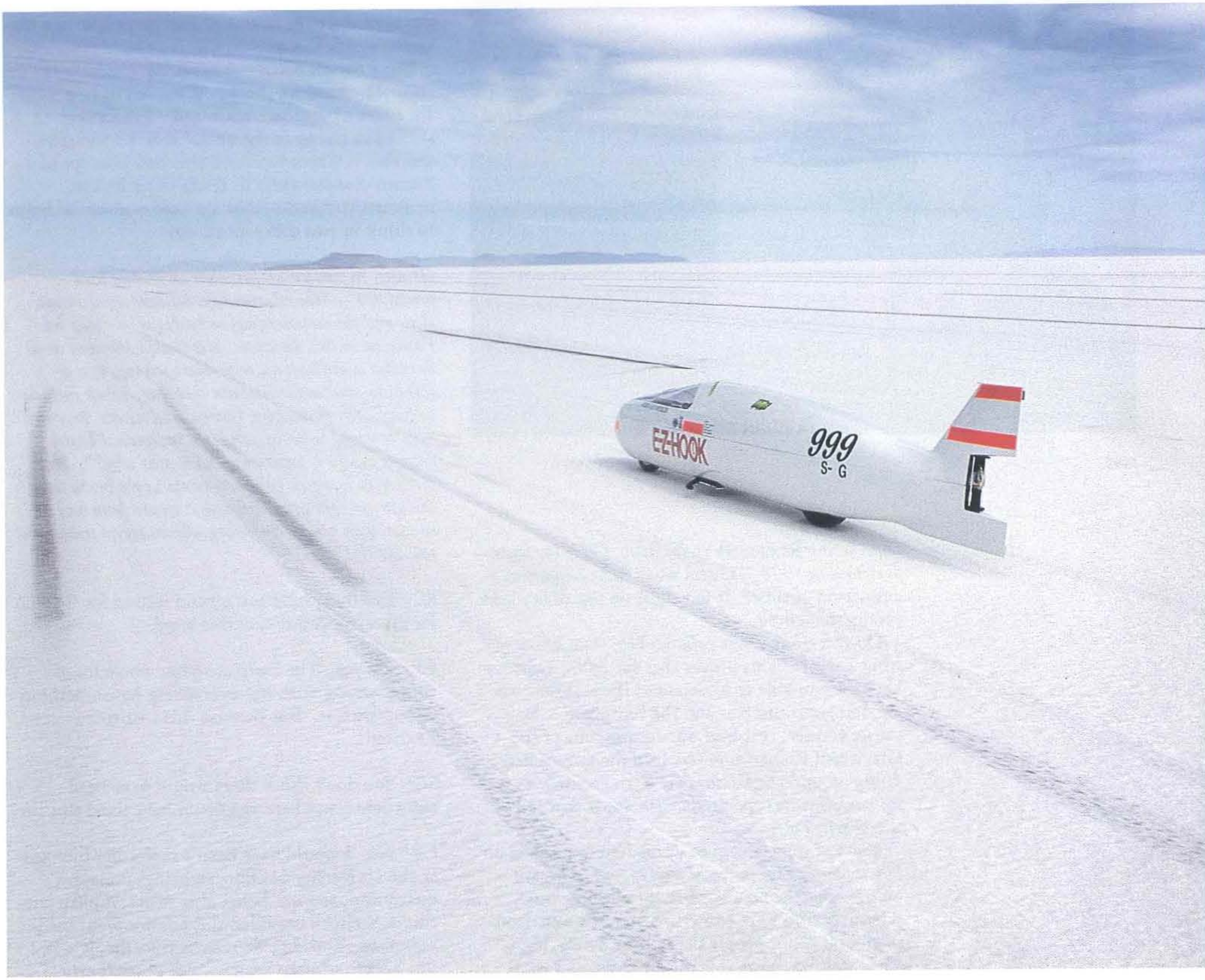
SC: You must have some good feeling for Caltech for allowing you to live this way?

CP: Oh yes. The Institute has a whole lot of things wrong with it—everything has something wrong with it. But because this Institute existed, I existed.

SC: You don't think there would have been anywhere else where you could have lived this life?

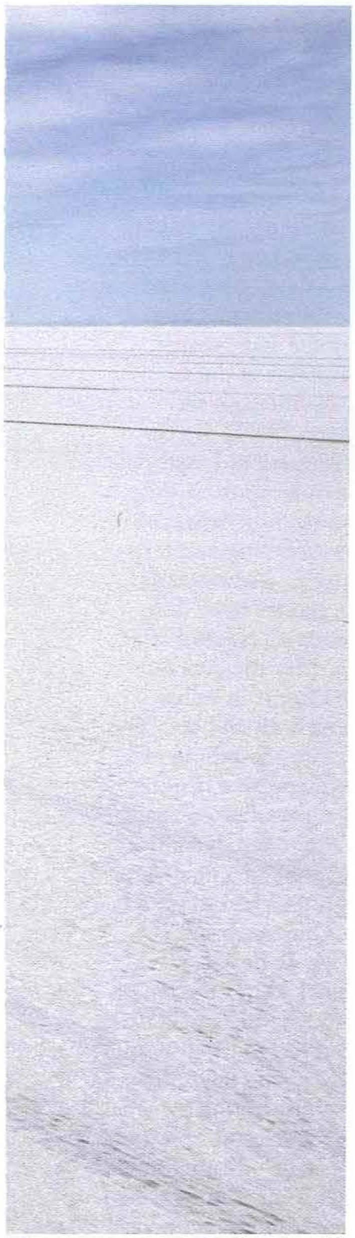
CP: No. I would have been a molecular biologist at the University of Ohio, fighting, unhappy, quarreling, and not being able to accomplish anything. Caltech provided this environment inadvertently—it didn't do it intentionally. It was just there. It was a magnificent opportunity. □

The machine that resulted from all this effort is a motorcycle only in the sense that it has two wheels, one in front of the other, and an engine that sends power to the rear wheel through a drive chain. Otherwise, the 18-foot, 4-inch vehicle looks like a wingless 1950s-vintage jet fighter—complete with a scoop-nosed air intake, a cockpit canopy of clear, shatterproof plastic, and a fuselage tapering to a vertical-finned tail.



I'm Not Driving Fast, I'm Just Flying Low

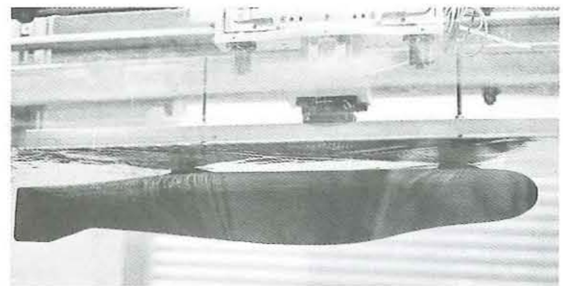
by Douglas L. Smith



In September 1996, Sam Wheeler returned to the Bonneville Salt Flats to break the record he had set only a month before.

On August 19, 1996, a streamlined motorcycle whose aerodynamic shell was designed by a bunch of Caltech grad students as a class project set a land-speed record at the Bonneville Salt Flats International Speedway. The flats, about 100 square miles in extent, lie entirely in Utah, but the closest hotels—or habitation in any form!—are in Wendover, Nevada, just over the state line. (Nevada's more liberal approach to gambling and liquor may have influenced this.) The flats are part of the Great Salt Lake Desert, the desiccated corpse of Lake Bonneville, which in the late Pleistocene epoch covered most of Utah west of what is now I-15 and spilled over into Nevada and Idaho. The drying lake's bequest of dissolved minerals that had washed down from the surrounding mountains left a dead-level salt pan that, after baking under the summer sun, is as hard as concrete and ideally suited for going way too fast—there's absolutely nothing to run into for miles and miles. Numerous speed and endurance records have been set there since 1935. And there are records galore—in the interest of equity, all competing vehicles are classified by body configuration, engine size and type, and fuel type. Each class has its own record, and there are also overall records.

Back in 1970, a gent named Sam Wheeler set a class record there of 208 miles per hour in a streamlined motorcycle. (To be classified as a streamliner, the vehicle must have the front wheel or wheels covered by an aerodynamic skirt, and must be powered by a mechanical drive train (as opposed to jet and rocket cars, whose wheels merely support the vehicle's weight).) This historical footnote would have nothing whatsoever to do with Caltech, except that some 15 years later, Wheeler would once again catch record fever. He had long since retired from racing by then, and had become chief engineer for E-Z-Hook, a manufacturer of test probes and leads in Arcadia, California. His boss, company owner and Caltech Associate Phelps Wood, quickly agreed to underwrite him. Wheeler designed and built the motorcycle's frame in E-Z-Hook's machine shop on evenings and weekends over the next three years. Meanwhile, Wheeler and Wood became concerned about the possibility of going airborne, and not in a good way. Wheeler's earlier bike had handled well enough in its record run, but subsequent wind-tunnel tests had shown that its front end was generating a lot of lift—perhaps enough to flip the bike on its back at higher speeds. As Wheeler says, "At 300 miles an hour, you can make a rock fly." In the spring of '89, Wood mentioned this concern to another motorcycle enthusiast, Elliott Andrews, Caltech's division administrator for Engineering and Applied Science. Recalls then-graduate student C. Dennis Moore (PhD '96), "Upside-down and backwards is not really a good way to set a record. So Andrews called Professor [Hans] Hornung [Johnson Professor of Aeronautics and director of the Graduate Aeronautical Laboratories (GALCIT)]. Professor Hornung said, 'Hmmmm, Dennis rides a motorcycle.' I came in one morning and found a note on my desk saying call Sam



Above: (from left) Grad students Roderick Daebelliehn, Michael Dominick, Elizabeth McKenney, Yvan Maciel, and Dennis Moore with the 1/4-scale model in the 10-foot wind tunnel's test section. Right: In the water-channel experiments, a 1/8-scale clay model of each proposed body shape was submerged upside-down with its wheels just touching the water's surface.

Wheeler. Who is this Sam Wheeler guy and why do I have to call him?" But call he did, and quickly got hooked.

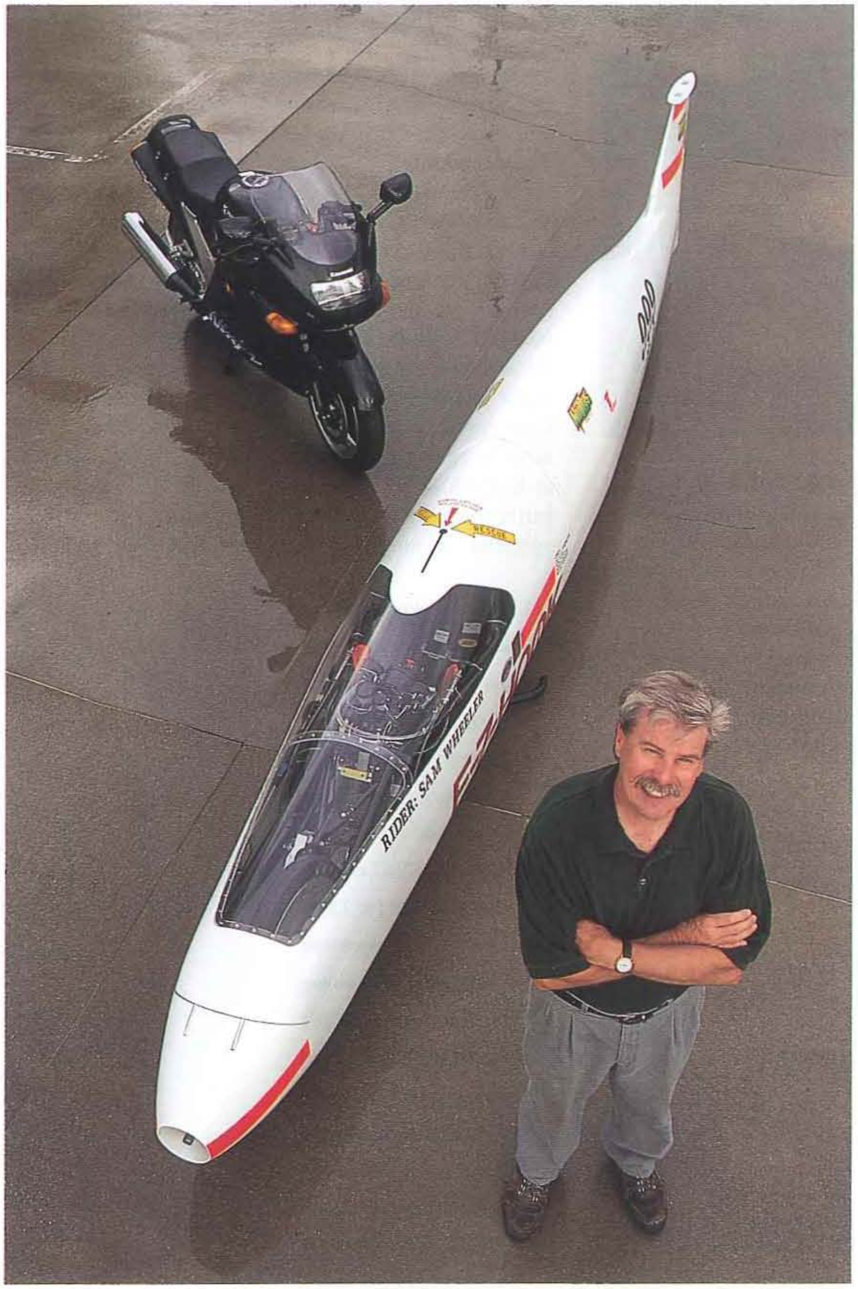
Determining how the bike would handle was clearly a wind-tunnel exercise. But the bike's stability would depend on what sort of body was put on it. Since the body hadn't been built yet, Andrews proposed to Hornung and Gerald Landry, manager of GALCIT's 10-foot wind tunnel, that designing the body and determining its handling characteristics would make a good assignment for Aeronautics 104 (Experimental Methods). At Ae 104's first meeting, the professors hawk a bazaar of term-length projects, from which five or six actually get chosen by the students. This project made for a nice confluence of interests—the students got a real-world project of manageable scope (most of the offerings tend to be academic), and Wheeler couldn't afford to use the 10-foot tunnel at the outside rate. "E-Z-Hook only had $2,500 to spend," recalls Landry. "Normally, $2,500 gets you one day." But interdepartmental charges for tunnel time at the student rate are much lower, so the company donated the money to the class. The class kicked in some of its operating budget as well, and the project wound up in the 10-foot tunnel for two and a half weeks. Moore TA'ed the assignment, which was offered in the spring of 1990, and sold it to the class.

The students who bought into the project—Yvan Maciel (MS '90), Roderick Daebelliehn (MS '90), Elizabeth McKenney (PhD '95), and Michael Dominick (MS '90)—were given carte blanche to try any shape they liked, provided that it fit around Wheeler's frame. The goal was to find the shape with the lowest possible drag that was still aerodynamically stable—no lifting, no pitching, and no flopping over sideways. The choices were endless—rounded nose or blunt nose? Are tail fins good? How many? How big? How oriented? And what about the windshield? A near-vertical one would give better visibility but significantly increase the drag. A more horizontal one would have little drag but might be hard to see through.

The students sculpted a variety of 1/8-scale clay models around a wooden armature that represented the frame members and such other givens as the engine, wheel, and drive-chain clearances, and tested them in the low-speed water channel in Karman Lab. The tests were run by hanging the model upside-down in the channel, so that the wheels' bottoms just touched the water's surface, which stood in for the ground. The class tested six bodies "based on airfoils, previous streamliner designs, and wild ideas" says Moore, beginning with the variant with the largest nose and tail—it's a lot easier to shave clay off accurately than to stick it on. The winner was then scaled up to 1/4 size for further tests in GALCIT's 10-foot wind tunnel. In the 1/4-size tests, the students even worried about the engine's intake and exhaust. Plastic tubes scaled to the diameter of the air ducts ran from the intake scoop through the model's interior. Recalls Moore, "We'd dig the tubes out of the clay, move them around, and check the effect of the exhaust's location on drag." Yvan Maciel did the mathematical modeling of stability that guided the clay modeling and testing done by the rest of the group. For example, a big fin would lend stability but increase drag. The final design was a compromise, reached after several iterations. "We'd give him the data out of our wind tunnel. He'd go plug it in to his computer simulation and come back and say, 'The bike's not stable. You have to do something else.'"

Designers of land-speed vehicles have historically favored brute horsepower over finesse. Says Moore, "People don't worry about aerodynamics. They'd much rather spend $5,000 on a new set of pistons and stuff for their engines, than to spend $5,000 to reduce the drag." One exception is *Goldenrod*, a streamlined car whose design was also tested in the 10-foot tunnel. The creation of Walter Korff, a Lockheed aerodynamicist, *Goldenrod* held the overall land-speed record of 409 miles an hour for over 25 years. But most streamliners are laid out by eye, based on a gut feeling for what looks fast, leavened with experience and imitation.

Streamliners tend to have needle or bullet noses and stubby rear ends, because using a blunt tail so the thing will fit in the garage is of greater practi-



Wheeler, the streamliner, and the street bike that the streamliner's engine normally comes with.

cal importance than the aerodynamic virtues of a tapered tail. A cut-off rear end also makes it easier to install things like exhaust pipes, parachute tubes, and push bars. (Twin parachutes are required at Bonneville if you'll be running over 250 miles per hour; between 175 and 250, you only need one. A push bar is a stubby projection bolted to the rear of the chassis—remarkably, very few of these speed machines can get under way unassisted because of their extraordinarily high gear ratios. Most streamliners come with oversized, testosterone-oozing pickup trucks, sometimes with matching custom paint jobs, to push them until they get up enough speed to engage first gear. But the GALCIT bike is light enough that a couple of people running alongside and pushing suffices to get Wheeler up to 10 mph, where he can slip the clutch and power away from the starting line.)

However, the laws of aerodynamics say that it really doesn't matter what you do to the front (as long as you round the corners)—it's what's out back that counts. The airflow off the rear of the vehicle tends to keep going straight, even though the body isn't there any more, just as Wile E. Coyote can run for some distance off the edge of a cliff as long as he doesn't look down. Wherever the flow separates from the body, a partial vacuum forms, creating drag. But a properly tapered tail guides the flow and prevents separation. A typical streamliner has a drag coefficient of 0.15–0.20. This is pretty good, actually—for comparison, a VW bus has a drag coefficient of about 0.45, while an '87 Camaro scores a sleeker 0.30. A sheet of plywood held perpendicular to the wind has a drag coefficient of approximately 1.0.

The Ae 104 design has a mean drag coefficient of 0.103—the lowest ever measured for a stable land vehicle in the 10-foot tunnel—and no visible separation. (The rear tire did generate a wake, but it reattached itself to the underside of the bike further downstream; there was no observed separation off the front wheel or the body.)

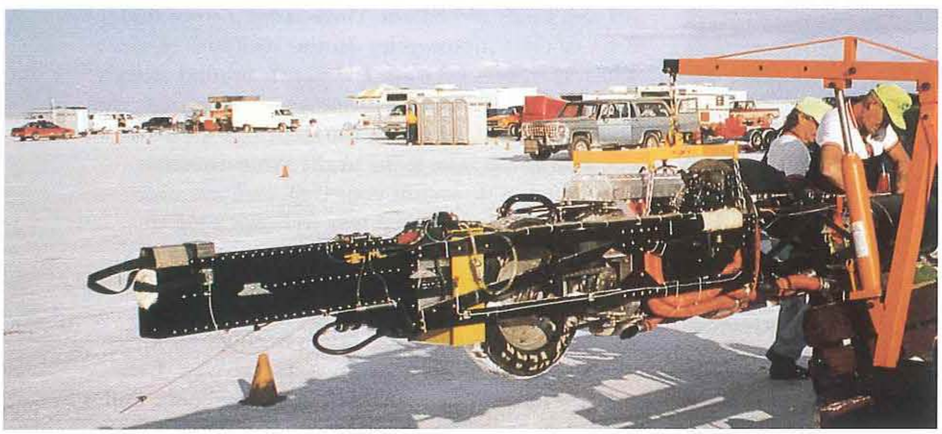
And what of the stability, which was why Caltech had gotten involved in the first place? With lift and pitch zeroed out, there remain three basic ways to tip a motorcycle. In the "wobble" mode, a wheel oscillates ever more violently around its axis until the bike falls over sideways. Then there's the "capsize" mode, in which the front tire kicks to one side as the bike leans in the same direction. The bike lies down (on your leg), and you slide to a stop. This is a good way to break a leg if you don't have a roll cage, but otherwise it's a comparatively safe crash. And finally, in the "weave" mode, which makes for the most spectacular video footage, the bike's front wheel kicks one way as the bike leans the opposite way. Centrifugal force brings you back but overshoots, and the sideways swing gets wider each time. Eventually the front wheel digs in and the bike vaults skyward, rear wheel first, and tumbles end-over-end for some distance before coming to rest. Maciel predicted, and tests in the 10-foot tunnel confirmed, that the final design was extremely stable against wobbling and capsizing. It was unstable in weave mode, but only under 40 mph—above that speed, it was rock-steady.

The machine that resulted from all this effort is a motorcycle only in the sense that it has two wheels, one in front of the other, and an engine that sends power to the rear wheel through a drive chain. Otherwise, the 18-foot, 4-inch vehicle looks like a wingless 1950s-vintage jet fighter—complete with a scoop-nosed air intake, a cockpit canopy of clear, shatterproof plastic, and a fuselage tapering to a vertical-finned tail. The wheel has turned full circle, in a way, because the body for Wheeler's previous record-breaking bike was

Right: Phelps Wood and the streamlined body pose in his backyard. Below: The frame hanging around in the pit area on the salt. From left: the parachute tubes (only the top one is loaded), the gas tank (with red cap), the radiator enclosure (yellow), the rear wheel and drive chain, the engine (between the silver air box and the orange air-intake hoses), and the firewall. The driver lies forward of the firewall (where the people are standing), but most of his roll cage is obscured by the hoist.

fashioned from a drop tank off an old fighter jet.

The new body, however, is a sandwich of carbon fiber, foam, and carbon fiber—the same material they make surfboards and racing-yacht hulls out of. Within that body, the frame, which doubles as the roll cage, encloses and protects the front wheel, the driver (who lies feet-first, straddling the wheel), the engine and drive train, the rear wheel, the radiator, and the fuel tank. The parachute tubes, bolted to the frame's rear, are enclosed in the tail beneath the fin.



This basic layout was copied from Wheeler's old bike, with one key difference: the old bike had a full-sized motorcycle wheel in front, which blocked Wheeler's view of the course ahead. Wheeler was traveling almost twice as fast as Charles Lindbergh did in the *Spirit of Saint Louis*, but both steered by looking out the side windows. Of course, at 208 miles per hour—305 feet per second—you don't steer so much as aim. This time around, the front wheel is small enough that Wheeler can look out over his toes to steer.

Turning the frame and a bunch of computer files into a motorcycle took another year; although the class had ended, Moore remained involved. Beth McKenney printed out full-sized cross sections of the bike, spaced at 10-inch intervals from nose to tail. Wheeler glued the printouts onto 0.030-inch aluminum sheets that became templates for carving 10-inch-thick Styrofoam blocks with a hot wire. The Styrofoam shapes, glued together, became the full-sized pattern. R. D. Boatworks in Dana Point created a mold from the pattern, and built the body up layer by painstaking layer. Recalls Moore, "Four months later, we got back a shell. We then had to make all the wheel cutouts and stuff, which took a significant amount of time." Furthermore, because of the custom design, virtually every part that went into the motorcycle had to be machined by Wheeler by hand. Commercial steering linkages wouldn't fit, and you couldn't buy drive sprockets with the right number of teeth for the gearing ratios the bike needed—the list goes on and on. Moore, who by now was spending almost as much time at E-Z-Hook as he was at GALCIT, designed and built the bike's electrical, pneumatic, and coolant systems. Meanwhile, Andrews, who used to run a Kawasaki dealership, persuaded Kawasaki to donate the engine—a four-cylinder, 1,052-cc Ninja, the highest-horsepower engine Kawasaki makes for a motorcycle—and cover the cost of making the mold, as well as provide some money for general expenses. (E-Z-Hook sprang for the body.) The bike finally reached the salt in August 1991.

The bike melds high- and low-technology, Caltech cleverness and real-world pragmatism. Moore's pneumatic system, which operates the parachute doors and the parking skids, uses the roll cage as the reservoir for 1,000 cubic inches of compressed nitrogen. His cooling system runs the engine's exhaust through a Venturi tube to suck outside air into the radiator. On the other hand, a Coke can was pressed into service as an overflow chamber for radiator water.

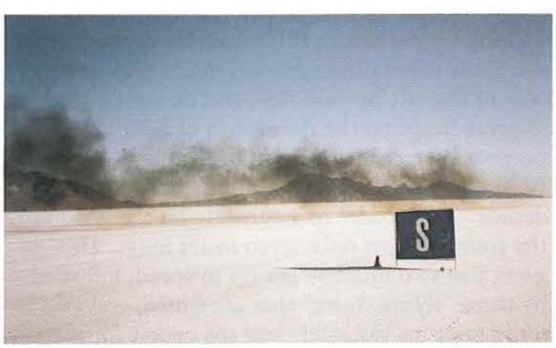
The Bureau of Land Management (BLM) opens the flats to the Southern California Timing Association (SCTA), which organizes the meets and certifies the records, one week a month from July through October—weather permitting. Wet salt often cancels the July meet, which is usually seen as a tune-up for Speed Week, held in August. Speed Week is Woodstock for gearheads. Bonneville is the last bastion of mom-and-pop

Above: The pit area just goes on and on. Below: Here's something you don't see every day—a six-door pickup truck. It's pushing a much-altered 1930s coupe.

Speed Week is Woodstock for gearheads. Bonneville is the last bastion of mom-and-pop racing, and the pit area consists of rank upon rank of motor homes and utility trailers, interspersed with tarps pitched by homegrown mechanical geniuses to shade the products of their ingenuity from the blistering sun.

racing, and the pit area consists of rank upon rank of motor homes and utility trailers, interspersed with tarps pitched by homegrown mechanical geniuses to shade the products of their ingenuity from the blistering sun. (Try picking up a socket wrench that's been sitting out in the 100-degree glare of midday some time. Sunglasses are de rigueur, and it's important to wear sunscreen on the underside of your chin and ears, or the reflected light off the salt will burn some really sensitive skin really fast. "Many people are surprised when their tan lines don't stop at the bottom edge of their shorts," says Andrews.) People spend more time working on their machines, cruising around the pit area to see how everybody else is fixed, swapping lies with old pals, and offering sage advice to newcomers than they do actually racing. You can see every sort of rig imaginable—from stock Trans Ams and tricked-out, chromed-to-the-max street rods with snappy paint jobs that have no business being on the highly corrosive salt, to the semi with a tug-boat engine that set a record for diesel trucks at 238 miles per hour. But the streamliner is king.

The racecourse itself, which is located several miles from the pit area for safety reasons, used to be 11 miles long, but the salt (originally about three feet thick) has been getting very thin of late. In some places, the dirt beneath is starting to show through. For the past 50 years, a mining concession on the east side of the flats has been sucking up the salt as brine, extracting the potash and some other minerals—all told, only a few percent of the salt by volume—and dumping the rest in great heaps on the flats south of I-80. Despite protests by racers and environmentalists alike, the BLM has just renewed the mine's lease for another decade. So in order to stay safely on the good salt, the course is now only seven miles long. This gives you two miles to get up to speed, followed by three "flying miles" that are timed, and two more miles to stop and clear the course for the



Left: Brute force personified—the only concession to aerodynamics is the small spoiler on the hood. And this guy don't need no stinkin' push truck, either!

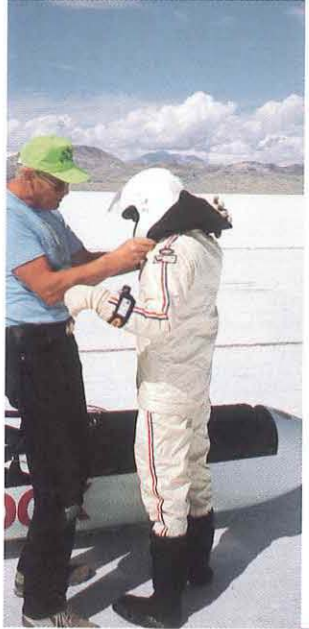
Right: Racing apparel can get awfully hot, as this rider of a partially streamlined motorcycle can tell you.

next guy. You break a beam of light at each end of each flying mile, and this information is relayed through a wireless system to the timing booth, which is actually a couple of folding tables under a sunshade pitched at the three-mile mark. The fastest of the three flying miles is your official time. The electronic timing system is accurate to one one-thousandth of a mile per hour.

It used to be that in order to set a record, you had to cover the same piece of real estate twice, in opposite directions, and take the average speed. But in 1996, for the first time, all runs were made heading west, away from the potash works—the salt had shrunk so much that the company's dikes were getting uncomfortably close to the course's eastern end. However, an average speed over two runs is still required for a record.

To run the course, you register once your vehicle has undergone an elaborate safety and technical inspection—a ritual that often takes an entire day—and you join the staging line by the timing booth. (On the first couple of days of Speed Week, you can wait in line all day for one run. The line shortens considerably over the week, as people break down, crash, or give up.) The stager sends the vehicles, a dozen or so at a time, out to the starting line to wait their turn. Going fast enough to qualify for a record attempt sends you to an impound area to await a second run. Otherwise, it's back to the tail of the staging line—or, most likely, back to the pits to see what went wrong.

And lots can go wrong. The maiden run in August 1991 was literally a flop. The problem wasn't *sta*bility, but *visi*bility. The cockpit canopy slopes a mere 10 degrees from the horizontal when lowered into position, and ripples in its plastic that were imperceptible when it was upright suddenly caused the horizon to waver worse than a heat-shimmery parking lot on the Fourth of July. Undaunted, Wheeler tried to balance the bike by feel but wiped out at about 60 mph. He was unhurt, which is a tribute to the SCTA's stringent



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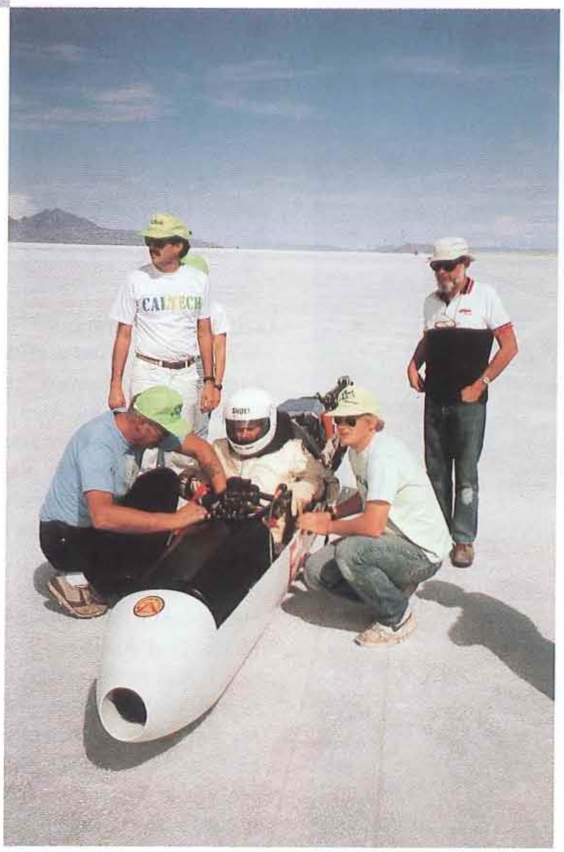


Above: Besides a fireproof suit, the SCTA requires drivers to wear fireproof boots and gloves, a helmet, a neck brace (here being adjusted by Joe Von Harten), and wrist restraints to keep your arms safely inside the roll cage in the event of a crash. Right: You don't sit down in a streamliner as much as put it on. Clockwise from left: Von Harten helps Wheeler with the seven-point safety harness (another SCTA requirement), while Andrews (standing in front of his wife, Jill), Frank Sherrill, and Moore stand by.

safety rules—drivers routinely walk away from crashes at much higher speeds—and the bike suffered less than $300 worth of damage.

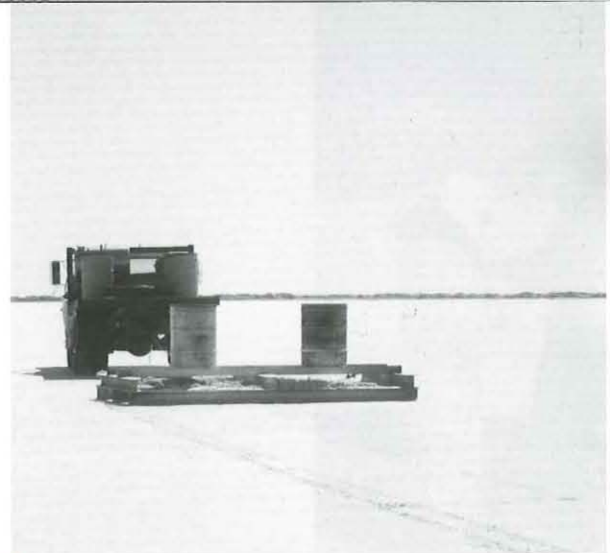
"What did do damage, however, was when we were inspecting for damage back [at E-Z-Hook] in Arcadia," says Moore. "We slung the bike up on straps that were rated to 3,000 pounds apiece; the bike, with Sam in it, weighs 1,000 pounds. The straps, which admittedly were old, broke, and dropped the bike four feet onto concrete. We sprung some welds, bent up the parachute doors, and dented the front wheel." Rubber doesn't dent, of course, but aluminum does. You see, the SCTA requires all Bonneville racers to use tires rated for the appropriate weight and speed, and some sizes—including those small enough to see over—are very hard to get. Thus the streamliner had been designed around a solid front wheel machined from aircraft-rated aluminum—a decision that was to dog the project. (The rear tire is the front tire off a funny car.)

The fall had also subtly screwed up the wheel alignment, a fact that wouldn't be discovered until the next trip back to the salt in September. (One might ask why these teething troubles couldn't be worked through closer to home, instead of towing a trailer nearly 700 miles to Utah. The answer hinges on that aluminum wheel—you can't run it on pavement, gravel, or even hard dirt without chewing it up. It's only good for salt or really soft silt.) Even so, Wheeler managed to clock 178 mph—in second gear with the canopy off. The ripples, which had been polished out in Arcadia, reappeared in Bonneville's drier, thinner air. Wheeler whiled away the winter by rebuilding the canopy with a narrow inset of thinner, more optically perfect plastic in his line of sight.

"1991–1993 was the most discouraging period of the project," Wheeler recalls. There were endless late nights and early mornings to prep the bike before each meet, followed by a 10-hour drive to the flats to discover that Murphy still wouldn't

Below: The reason for all that safety gear—the driver didn't suffer so much as a scratch. That's his front suspension system sitting on the salt behind his door. His other door got so thoroughly folded that it would easily fit in his trunk.

Right: Whenever somebody crashes, or the salt gets a little rough, they groom it with this saline zamboni: a grid of welded-together I-beams weighted down with drums of salt.



Right: The crew tried everything during those frustrating post-crash months. Here, Moore and Andrews use a piece of string to do a quick-and-dirty rear-wheel alignment check between runs.

leave them alone. It took most of '92 to solve the alignment problem, at which point "I found that we hadn't been getting any stiction between the salt and the front wheel," says Wheeler. "But before I discovered this, I probably solved a whole lot of problems that didn't need fixing." In the final analysis, that inch-and-a-half-wide aluminum wheel just wasn't getting a very good grip on the salt, making the bike hard to steer. That winter, Wheeler whittled a wider wheel with a more aggressively grooved "tread" zone. No sooner was the new wheel finished, than the elements turned sour. Lousy weather and wet salt wiped out the entire '93 season. It wasn't so bad if the crew knew in advance that the meet was off—as in July, when the salt was still under 18 inches of water from the winter's snowmelt—but sometimes, to find out, they had to caravan up there and spend a day or two waiting in line not to run. Moore and Andrews were regulars on the prep team, and nearly always on the running team as well during those years. (The crew's other regulars—Frank Sherrill, Bill Ocheltree, Mike Holliday and Chuck Bullwinkle—were racing cronies of Wheeler's.) But as '94 rolled around, Moore's thesis research and Andrews' involvement in the construction of the Gordon and Betty Moore Laboratory of Engineering left no time to spare for the streamliner.

Ronald Howe, of JPL's atmospheric research group, picked up the slack. Howe, a mechanical engineer and machinist who designs and builds components for instrument packages flown on high-altitude balloons, had actually come to bat for the project once before. "I made a maximum of maybe five percent of the [bike's] parts in my machine shop at home when Sam got rushed towards the end." Howe used to drag-race motorcycles back in the '60s and '70s and had known Wheeler by reputation then, but the two didn't meet until much later. "I met Sam about 15 years ago, out in the desert with a bunch of JPL guys

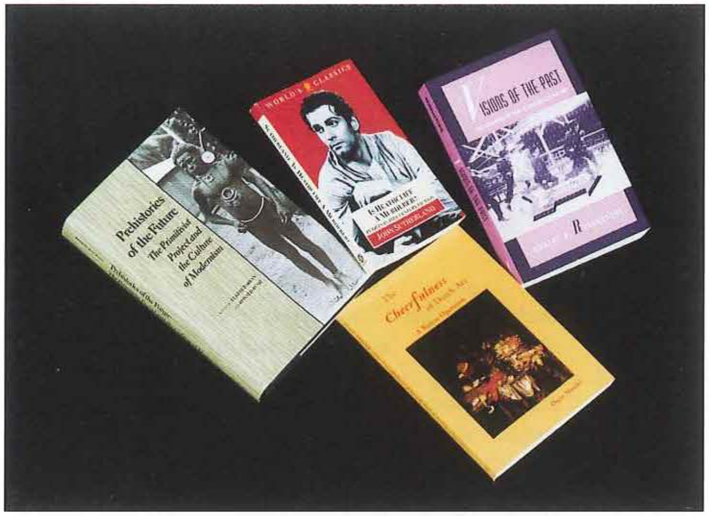
"He was still accelerating because the course is too short. That makes him the third-fastest bike ever—the fastest single-engine, fastest on gasoline."

Moore checks the tail fin's alignment. The fin, which was damaged in the crash, eventually had to be rebuilt completely; sadly, only one set of GALCIT decals had been made.

riding motorcycles. I'd been trying to go up to Bonneville for years, but I never got there before I went up with Sam." However, their dads, both test engineers/machinists and lifelong JPL employees, had worked together in the '50s, building a hypersonic wind tunnel (since decomissioned). The two also helped construct the Space Simulator, a high-vacuum chamber 25 feet in diameter and 85 feet tall in which full-sized spacecraft can be tested.

The salt was wet during most of '94, too, but in the one dry meet—September—Wheeler did 206 mph with the new wheel and the bike handled as well as it had before it got dropped. At the same time, the SCTA started talking about banning metal wheels, on the grounds that they rutted the salt and made it unsafe for other racers. This sparked a quest for yet *another* wheel, this one with a rubber tire. Goodyear's racing division eventually donated a set of undersized tires that had been custom-built for a streamlined motorcycle that proved to be too heavy to use them. The smallest tires Wheeler could find, they were still four inches in diameter larger than the aluminum wheel, so an entirely new suspension system had to be built. In September 1995, with the rubber tire in hand, or rather on bike, Wheeler beat the class record, but it didn't count—the salt at the far end of the course was still soft from a rainstorm a few days earlier, and he couldn't make a return run. That was the bad news. The good news was that the bike handled even better with the rubber tire, and he was still only in fifth gear. The October meet got canceled for lack of entries.

In 1996, everything finally clicked. July got rained out again, but during Speed Week in August, Wheeler set the class record at 256 mph. On September 25, the first day of that month's meet, he raised it to 285 mph. Then, says Andrews, "Two days later he went 295 mph one-way, with an exit speed of 301 mph. He was still accelerating because the course is too short. That makes him the third-fastest bike ever—the fastest single-engine, fastest on gasoline." The record for any kind of motorcycle is 323 mph, held by a 2,600 pound behemoth sponsored by *Easy Rider* magazine and powered by twin 1.5-liter Harley-Davidson engines (a total of 3,000 cc) burning 80 percent nitromethane. Wheeler thinks he can take the Easy Rider; he'd just barely gotten into sixth gear at 301 mph. But he's going to need those missing miles of salt—Moore has estimated the drag based on the vehicle's performance in coast-down runs, in which they timed how the bike slowed down when Wheeler shifted into neutral at a predetermined speed; knowing the bike's weight and the deceleration rate allows you to calculate drag. Moore's estimate is slightly below the values predicted in the Ae 104 class. "If there were two more miles of salt, or 25 more horsepower in the bike, we'd be there," he says. Absent the salt's miraculous healing, a switch to alcohol fuel would get them the extra horsepower. But that's a project for the coming year—the October meet got canceled, so the books are closed on 1996. □



Visions of the Past: The Challenge of Film to our Idea of History

by Robert A. Rosenstone
Harvard University Press, 1995; 271 pages

When Professor of History Robert Rosenstone first introduced movies in his Caltech classes in the 1970s, and then in 1977 taught a course entitled "History on Film," class enrollments soared. But ultimately this innovation made an even more profound impact on his own studies, luring the self-acknowledged "Dragnet" historian (just the facts, ma'am) into the theoretical issues of how film works to create or "re-create" history. "History does not exist until it is created," writes Rosenstone. Film, he found, offers a new relationship to the past and a new concept of what we mean by "history." His latest book, comprising a collection of essays exploring what happens when words are translated into images, suggests that film is an even more appropriate medium for showing us the past than are words on a page. There are, however, different and more complex rules for history on film than for history on the page, and in his book Rosenstone discusses how these "rules" are observed in the various forms of historical film: for example, documentaries, films that mix fictional and historical characters, films from other cultures, and experimental films with deliberate anachronisms and inventions that "re-vision" history. He discusses five films in depth, including *Reds* and *The Good Fight*. Rosenstone, who served as historical consultant on the former and narration writer on the latter, also practices what he preaches.

Is Heathcliff a Murderer? Puzzles in 19th-Century Fiction

by John Sutherland
Oxford University Press, 1996; 258 pages

So, *does* Heathcliff murder Cathy's brother in *Wuthering Heights*? Reasonable doubt. And how about Becky Sharp in *Vanity Fair*? Does she kill Jos Sedley in the end? Of course not, says John Sutherland, but Thackeray wants his readers to suspect her anyway. Victorian authors, not stupid by any means or even simply careless, had various reasons for slipping such red herrings and other enigmas and anomalies into their novels, and Sutherland plays detective in teasing out these reasons and suggesting imaginative interpretations of 37 literary "puzzles." Sutherland, the Lord Northcliffe Professor of Modern English Literature at University College London, as well as a visiting (annually) professor of literature at Caltech, where he taught from 1983 to 1992, is a closer reader than most people, perhaps not wholly unrelated to the fact that he has edited a number of these works for the World's Classics series. Most of the readers of this paperback, which made *The Times* bestseller list in London, probably never lost any sleep over these puzzles in the original texts. But even if you didn't notice that Jane Austen lets apple trees blossom in June in *Emma* and that Dickens gets sloppy with his seasons in *Martin Chuzzlewit,* this remarkably unstuffy book, with evocations of such nineties phenomena (1990s, that is) as date rape and the movie spoof *Frankenhooker,* not to mention reasonable doubt, is fun to read and might even lure you into the novels themselves. So why *did* Henry James rewrite the ending of *The Portrait of a Lady*? And why doesn't H. G. Wells's invisible man make himself an invisible suit and some invisible food?

The Cheerfulness of Dutch Art: A Rescue Operation

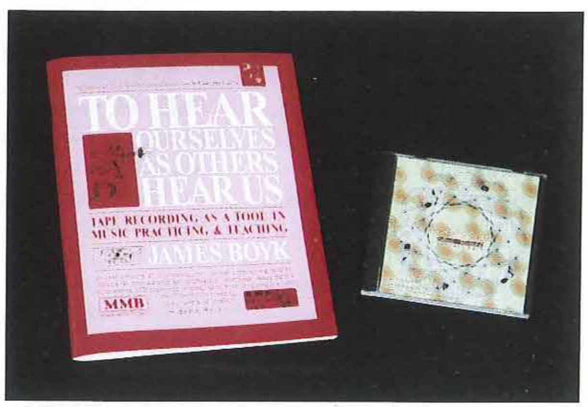
by Oscar Mandel
Davaco Publishers (Netherlands), 1996; 128 pages

English novelists in the 19th century may have planted puzzles in their work, but 17th-century Dutch painters most assuredly did not, according to Professor of Literature Oscar Mandel. In this short book Mandel takes on the current intellectual fashion of imposing 20th-century interpretations of "semi-veiled meanings" on these paintings, interpretations that invariably see gloomy, moralistic lessons beneath the surface of the most riotous peasant feasts, merry companies, and even innocent still lifes and landscapes. Mandel chalks this up to our own century's "assault on euphoria" and sets out to liberate the "self-evidently happy works" of the 17th-century Dutch painters "from the excesses of academic earnestness." The Dutch painted their hedonistic displays of food and flowers and depictions of the human drama of daily domestic life, he writes, to create images for pleasure and joy—and as a relief from the incessant moralizing of the past. He also argues that the heroic, allegorical paintings of the time, which appear to be loaded with obvious high-minded meaning, are really using Biblical and classical themes as a front to indulge in painting nudes—and some quite erotic ones at that. Mandel grants that a tradition of *vanitas* paintings did exist, with unambiguous, and legitimate, symbols—skulls, skeletons—of the transitoriness of life. But, he claims, not every snail nibbling a tulip petal connotes mortality, not every bird is a lewd proposition, and sometimes an empty shoe is only an empty shoe.

PREHISTORIES OF THE FUTURE: THE PRIMITIVIST PROJECT AND THE CULTURE OF MODERNISM

Edited by Elazar Barkan and Ronald Bush, Stanford University Press, 1995; 449 pages

Most of the 16 essays in this book exploring the influence of ethnography on what has become popularly known as modernism were originally presented at a 1991 conference jointly sponsored by Caltech and the Claremont Graduate School Humanities Center. In the late 19th century various technologies (for example, railroads, telegraphy, photography) brought Western culture into closer encounter with primitive cultures, ushering in a profound alteration in how Westerners perceived others—and themselves. This new fascination with the primitive pervades much of the literature, art, and music of the early 20th century. The book's editors, who also organized the original conference, Elazar Barkan, associate professor of history at Claremont Graduate School (as well as director of its Humanities Center and previously instructor in history at Caltech), and Ronald Bush, professor of literature at Caltech, don't follow the easier, more heavily traveled routes through the familiar modernism terrain. Rather, they and the other contributors shift backward and dig deeper into the political, social, and racial antecedents and complexities of encounters with primitive societies. Some of the essays deal with academic anthropology, but topics also encompass vampires and violence, Gauguin in Tahiti, Josephine Baker in Paris, the influence of African American music on Irving Berlin, T. S. Eliot's fascination with primitive peoples, and the effect of ethnographic photography's erotic images on Victorian morality.



TO HEAR OURSELVES AS OTHERS HEAR US: TAPE RECORDING AS A TOOL IN MUSIC PRACTICING AND TEACHING

by James Boyk, MMB Music, Inc., 1996, 78 pages

James Boyk, Caltech lecturer in electrical engineering and music, explains his own coaching techniques "for music students, teachers, performers, and those who enjoy a peek behind the scenes." Not just a technical how-to manual, the book teaches how to listen to oneself, and it is richly illustrated with anecdotes from the author's own career as pianist and teacher and with reflections on making music. Among other things, it advises us to "squint our ears" when listening to tape playbacks, and to dance and sing along. But the technical side is not overlooked: Boyk also includes a chapter on audio systems and components, giving readers the inside scoop from his many years testing recording equipment in his Caltech lab. Yehudi Menuhin has called the book "valuable to both teacher and student," and André Watts contributes that it's "a treasure-trove of information, advice and entertaining musical insights for both amateur and professional musicians. . . . [which] should be required reading for all lovers of music."

TECHNICALLY SOUND

Caltech-Occidental Concert Band compact disk

The Caltech-Occidental Concert Band, directed by Bill Bing, director of Caltech's instrumental music program, has recorded its first CD. It's loaded with such Caltechiana as the "Centennial Suite," written by alumnus Les Deutsch (BS '76, PhD '80) for Caltech's 100th birthday; "Throop March," written in 1900 and "unearthed" in 1987; and a medley of unforgettable songs from the 1920s including "Lead Us On, Our Fighting Beavers," "Fight, Men of California Tech," and the "Gnome Sweetheart Song" (all sans lyrics, unfortunately). There are pieces by two other local composers with a Caltech connection (but no beavers or Gnomes) and, oh yes, some Ives, Sousa, and Mozart too. The CD can be ordered from the Caltech Bookstore (818-395-6161) for $12.95 plus shipping and handling. □—*JD*

Honors and Awards

Faculty File

Tom Ahrens, MS '58, professor of geophysics, has been selected by the Meteoritical Society to receive its 1997 Barringer Award, which recognizes outstanding contributions to the study of impact craters and associated phenomena.

Jacqueline Barton, professor of chemistry, is the recipient of the 1997 William N. Nichols Medal, presented by the Nichols Medal Jury and the New York Section of the American Chemical Society.

John Bercaw, Centennial Professor of Chemistry, has been awarded an American Chemical Society Award for Distinguished Service in the Advancement of Inorganic Chemistry, sponsored by Mallinckrodt Baker, Inc.

Norman Brooks, PhD '54, the James Irvine Professor of Environmental and Civil Engineering, Emeritus, has been elected an Honorary Member of the American Society of Civil Engineeering.

Erick Carreira, associate professor of chemistry, is one of 60 young researchers named by President Clinton to receive the first annual Presidential Early Career Award for Scientists and Engineers. Carreira has also received an American Chemical Society Award in Pure Chemistry from the Alpha Chi Sigma Fraternity, and an Arthur C. Cope Scholar Award from the American Chemical Society.

Donald Coles, PhD '53, professor of aeronautics, emeritus, has been awarded the 1996 Otto Laporte Award by the American Physical Society.

Slobodan Cuk, PhD '77, associate professor of electrical engineering, has been elected a Fellow of the Institute of Electrical and Electronics Engineers.

Barbara Imperiali, associate professor of chemistry, has received a 1996 Arthur C. Cope Scholar Award from the American Chemical Society.

Wolfgang Knauss '58, PhD '63, professor of aeronautics and applied mechanics, has been elected to the International Academy of Engineering (formerly the Russian or Soviet Academy of Engineering).

Carver Mead '56, PhD '60, the Gordon and Betty Moore Professor of Engineering and Applied Science, was selected by the Institute of Electrical and Electronics Engineers to receive the IEEE John Von Neumann Medal for 1966. He has also received the 1996 Phil Kaufman Award from the Electronic Design Automation Companies (EDAC).

John Seinfeld, the Louis E. Nohl Professor and professor of chemical engineering, and chair of the Division of Engineering and Applied Science, has been elected a Fellow of the American Institute of Chemical Engineers.

Robert Sharp '34, MS '35, the Robert P. Sharp Professor of Geology, Emeritus, has been selected by the Geological Society of America as recipient of the Distinguished Career Award of the Quarternary Geology and Geomorphology Division.

Edward Stolper, the William E. Leonhard Professor of Geology, and chair of the Division of Geological and Planetary Sciences, has been chosen by the European Union of Geosciences as a joint recipient of the Arthur Holmes medal.

Edward Stone, the David Morrisroe Professor of Physics, vice president, and director of JPL, has been selected by the California Museum of Science and Industry Foundation to receive the International von Kármán Wings Award.

Peter Wyllie, professor of geology, has been invited to become a Foreign Member of the Academia Europaea in the Earth & Cosmic Sciences Section.

Ahmed Zewail, the Linus Pauling Professor of Chemical Physics and professor of physics, has received the 1996 Kirkwood Medal, presented by the New Haven Section of the American Chemical Society and Yale's Department of Chemistry. □

Anneila Sargent has been named executive director of Caltech's Owens Valley Radio Observatory (OVRO) in Big Pine, California, succeeding Nick Scoville, the Francis L. Moseley Professor of Astronomy, who has been director for 11 years. Sargent, a senior research associate in astronomy who is widely known for her research into the formation and evolution of stars and protoplanetary systems, has been serving as associate director for millimeter-wave operations at OVRO since 1992. She earned her BS from the University of Edinburgh (1963) and her MS (1967) and PhD (1977) from Caltech, where she has remained ever since. OVRO will celebrate its 40th anniversary in 1998.

ENGINEERING THE STOCK MARKET

Caltech electrical engineering students aren't expected to graduate with a knowledge of the stock market, but Fred Maloney attributes his profitable investment practices to his Caltech experience. Maloney received his bachelor's degree in electrical engineering in 1935 and his master's in electrical engineering a year later. He says that he was impressed by a graduate course in economics taught by Horace Gilbert, Caltech's well-respected business economics professor who died in 1990.

Gilbert, an efficiency expert, taught Maloney how to evaluate a company, and by following his advice, Maloney says he has been able to amass significant wealth. "If it hadn't been for Gilbert, I probably wouldn't have done so well," says Maloney.

Although Maloney started investing in the stock market as soon as he graduated from Caltech, he devoted most of his energy to his investments after he retired as division superintendent at Texaco. He says he made four times as much money during his first year of retirement as he did the year before at Texaco. He continues to be assiduous about his securities, doing his own research and seldom relying on the advice of a broker.

Maloney has supported Caltech through donations to the Institute's Alumni Fund and through several charitable remainder trusts. As his stock portfolio increased in value, the concept of a charitable trust became increasingly important for tax purposes.

If you've enjoyed success in the stock market, why not consider following Maloney's example and establish a charitable trust at Caltech? The advantages to you are considerable: an income tax deduction, avoidance of capital gains taxes when the trustee sells your appreciated stock, broader diversification, an income to you for life, and, ultimately, significant support for the Institute.

Contact us (see below) for more information, or ask for our brochure that explains these trusts in greater detail.

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