California Institute of Technology

Engineering & Science

Fall 1988

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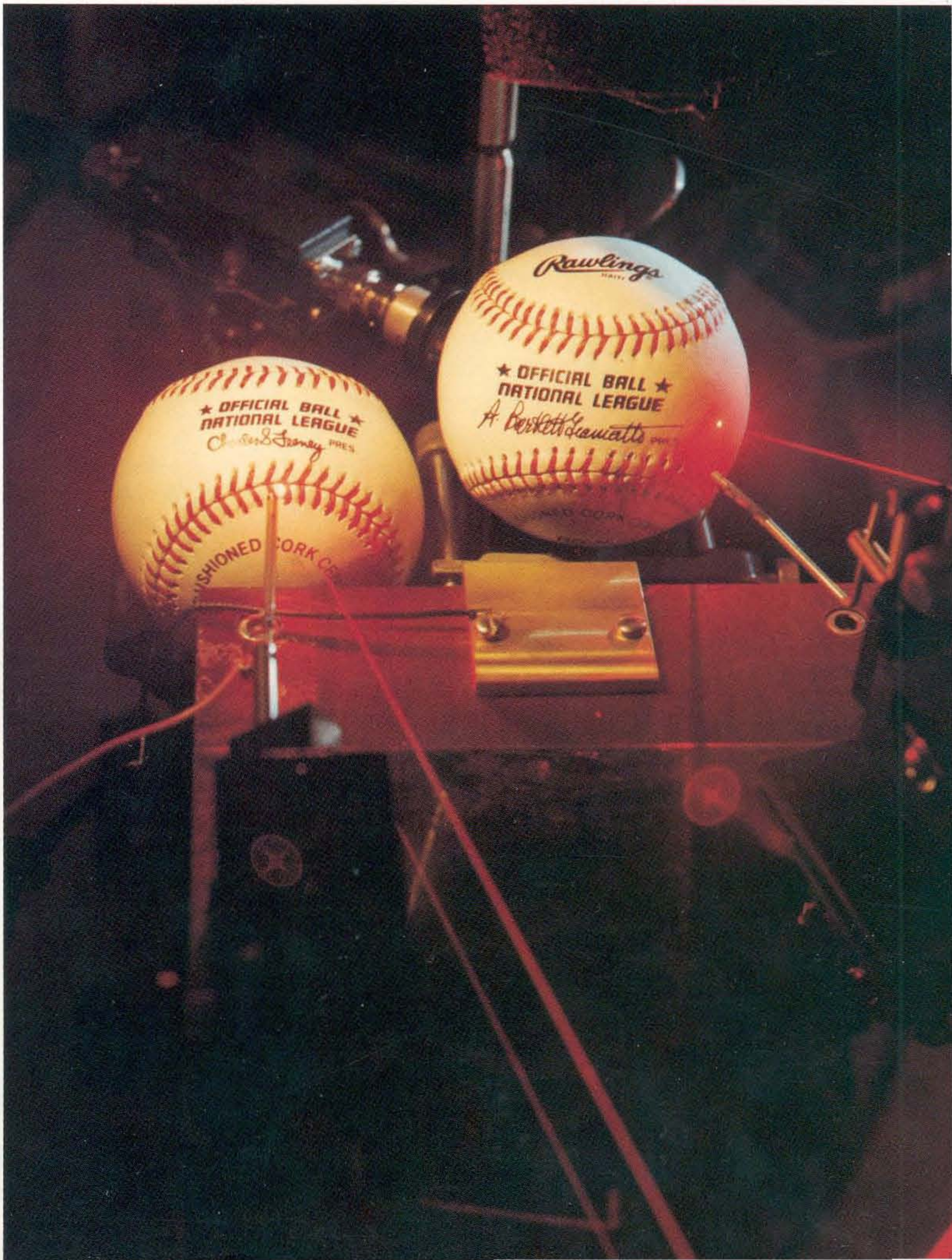
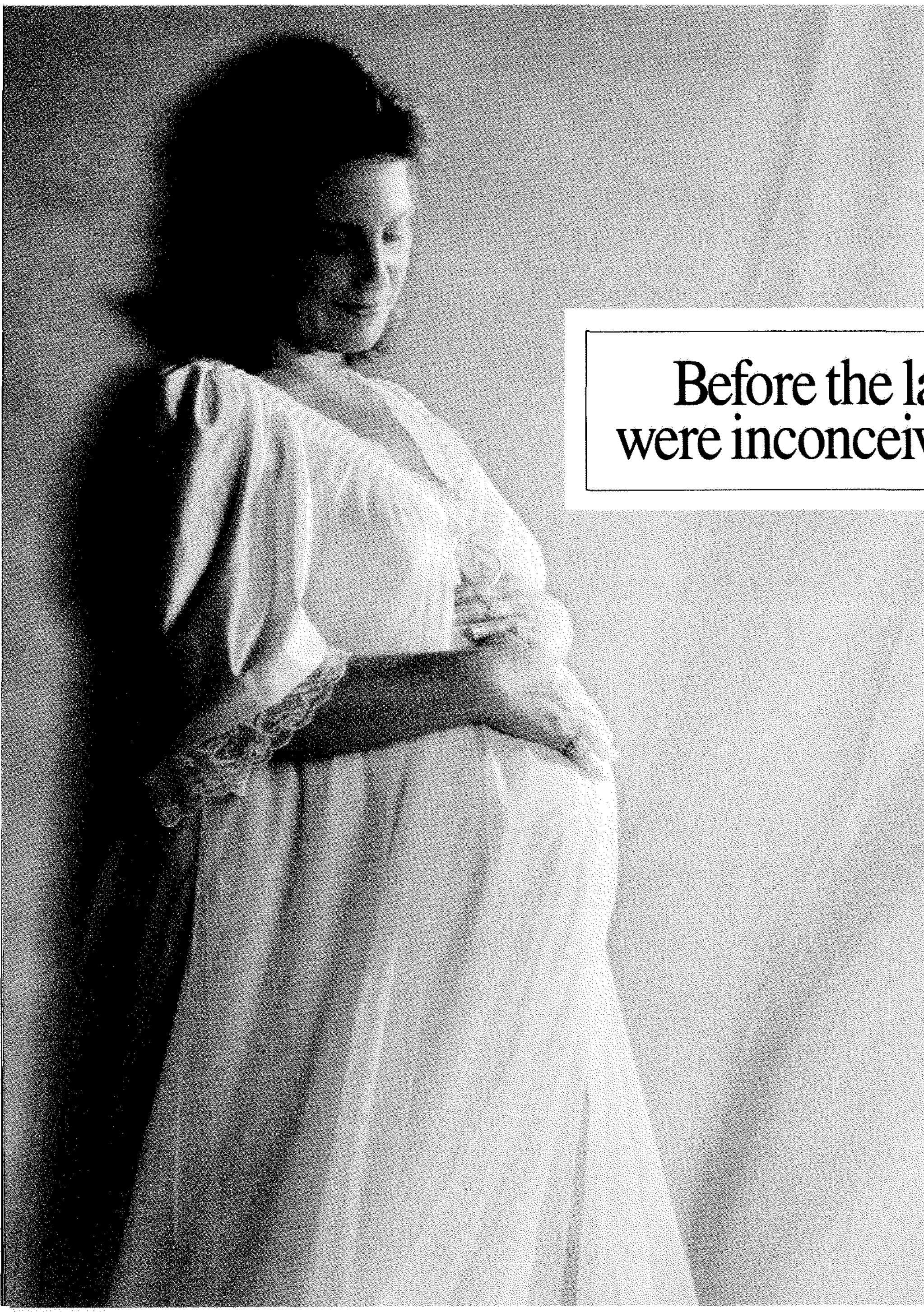
Yesteryear's baseballs

Tomorrow's computers





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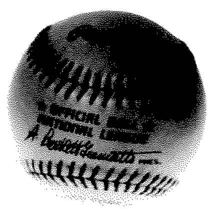

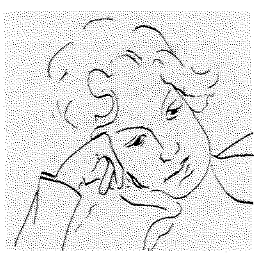

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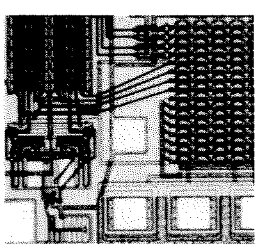

On the Cover: Lasers were not really used to probe the 1986 (left) and 1987 baseballs, but Ron Scott, in his story beginning on page 2, describes everything else he almost tried. The optics for this shot are courtesy of Robert Lang (BS '82, PhD '86) of the Photonics Group, Advanced Electronic Materials and Devices Section at JPL.

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How We Almost Solved the Problem of Why the 1987 Baseball Went Farther (If It Did) Than the 1986 Baseball

by Ronald F. Scott

Pedro Guerrero, then of the Los Angeles Dodgers, trots around the bases after a home run in 1987.

During the summer before last, everyone was talking and writing about the remarkable rate at which home runs were being batted in baseball's major leagues.

I saw a TV news feature one evening about the topic. Apparently, home runs were being hit at a rate 20 percent higher than a standard I didn't catch (last year? average rate?). Players, coaches, and fans were all ascribing the increase to a ball that was different in 1987. The manufacturer (there is only one, Rawlings, which makes balls for both major leagues) denied it, and two sectioned baseballs, that year's and the previous year's models, were produced to show that there was no evident difference.

What the TV broadcaster and I didn't know was that I would get involved, if you can call it that, in the controversy. At about this time, Caltech's public relations office had already been approached by a major West Coast newspaper, whose reporter wanted to know if anyone at Caltech might be interested in testing that year's and yesteryear's baseballs. The inquiry filtered down to me. In my life I have been more devoted to the flight of golf balls than baseballs, but the mechanics are the same (see Rabindra D. Mehta, "Aerodynamics of Sports Balls," *Annual Review of Fluid Mechanics*, 1985: 151-89), so I thought I'd take the question a stage further.

Hall Daily, Caltech's assistant director of public relations, filled me in on the story. This newspaper would 'support' some 'research' on the reason for the longer ball.

"When do they want the results?"

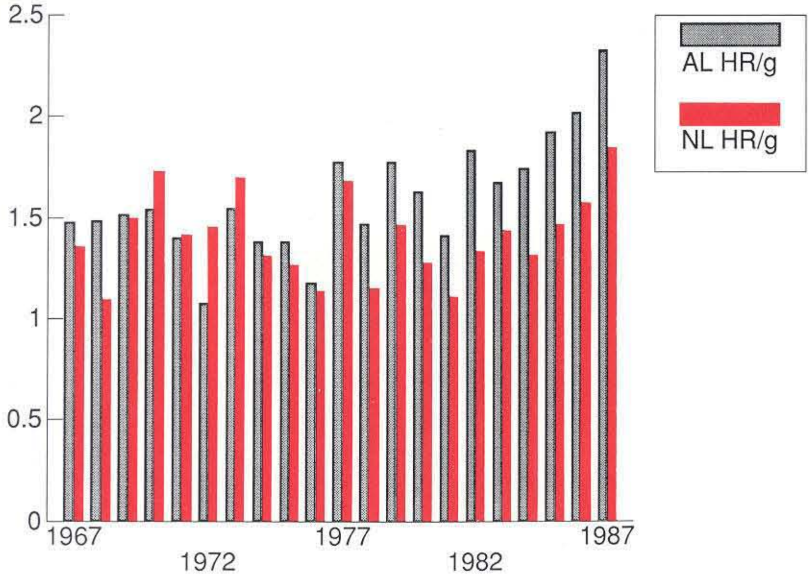
"Two weeks."

It was obviously not possible to do any reasonable amount of testing in such a short period, but I asked how many balls were available for testing.

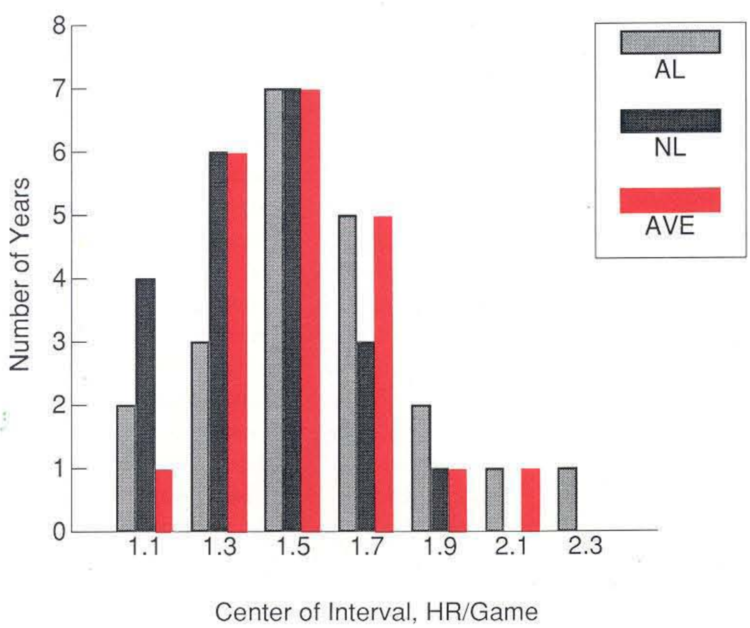
"Two: one 1986, and one 1987, but the reporter's looking for more."

It all seemed impossible at that stage (not enough balls), but I suggested some work the newspaper could do without using any baseballs at all. It was not clear, you see, whether the increased number of home runs was real or perceived; that is, whether 1987 was a particularly fruitful home run year, or 1986 had been one of especially low production. Since baseball is quintessentially a game of statistics (when our sons were younger, I had done several tours of duty at baseball parks, and had also listened to Vin Scully and his colleague, the numerically inclined Ross Porter), I concluded that it ought to be possible to make use of such data as the number of home runs hit per game, per league or both leagues, in the first two months of the season (this was about June 15) for, say, the last 20 seasons. Then we could establish a mean, standard deviation,* etc., and decide if 1987 was actually abnormal. I pointed this out to Daily. He thought it might be possible, but said the difficulty would lie in obtaining data on a

**I should point out that the manipulation of the statistics is not all that simple either. It could be assumed that the number of home runs, say, per year is a random sample, independent of time-classical statistics. Or we could postulate a correlation with some evolving factor, say, the weight of batters (like that of Rams linemen), in which case we should mess with time series analysis and Bayesian statistics.*



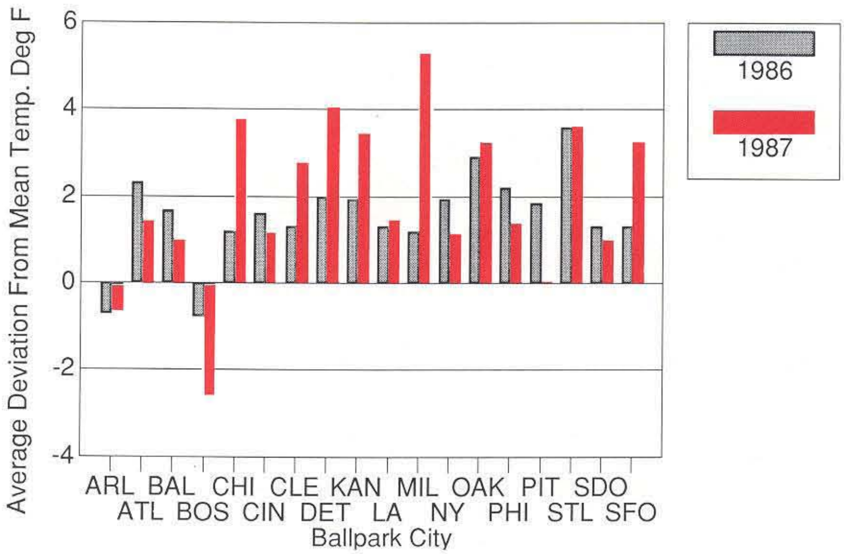
The top graph shows home run averages per game by year for 1967 to 1987; gray is the American League, red the National League. In the graph below the same data are arranged to display the number of years in each league (and the average of the two), in which the home runs per game shown on the horizontal scale were hit. The average is shown in red.



Baseball is quintessentially a game of statistics.

monthly basis. It did. Still, maybe this computation of annual home-run rates would solve the whole problem; 1987 could be within the standard deviation. Warming to the task, I then suggested weather statistics; balls will travel farther on a hot day. I said I'd settle for the average temperature at the major league parks over the two month period for the last 20 years. Daily indicated that park temperatures were not one of the usual statistics, so I compromised on the appropriate city temperatures for the period. That shouldn't be hard to get. A few questions came up—such as Candlestick Park, where we probably should use San Francisco airport temperature, not that of San Francisco itself; or correct for the Astrodome over the Houston city temperature. However, in general, city temperature was probably obtainable. Then the home-run fluctuation could be compared with the mean two-monthly city temperature over the period of interest. It might be an interesting correlation. Or it might not.

Hall arranged for lunch with the newspaper reporter. I talked to a graduate student who was interested in a possible research problem and could use the money, not necessarily in that order. I almost forgot the lunch, but was only 45 minutes late on the day. After introductions, we began serious negotiations. The lack of baseballs ("once is not enough") had been ameliorated. The reporter reported that a dozen 1986 balls had been located, and, of course, the 1987 balls were freely available. I asked where the 86s had come from, and he said an assistant coach had found a dozen in a box on the top of



This graph shows the average departures from the long-term mean monthly temperatures for April, May, and June for the major league baseball cities; the gray bars are 1986 and the red, 1987. The temperatures were measured at airports in or near the cities and the data obtained from the NOAA Climatological Data Annual Summary. (I strongly doubt the Boston figures, since they differ in trend from all other East Coast data.) Evidently 1987 was half a degree Fahrenheit warmer than 1986 for those three months, but I don't think this is significant for the home-run problem.

the lockers in the Dodgers locker room. I was curious enough to inquire how you could tell 1986 balls from 1987 balls. I thought they'd be dated. Not so.

"They have to be National League balls, otherwise we can't tell," he remarked.

He explained. The balls of both leagues have the names of the league and its president stamped on them. Charles Feeney, president of the National League for 13 years, had retired at the end of the 1986 season and was replaced by Bart Giamatti. So the former's name appears on the 1973-86 balls and the latter's on the 87s.

"Could I summarize the provenance of your dozen 1986 baseballs?" I asked. "I understand from you that your PR friend at the Dodgers got them from an assistant coach who found them in the locker room in a box."

"That's right," said the reporter.

"But how does the assistant coach know they are 1986s and not some previous year?"

"He *knows*. But it probably doesn't matter anyway, since it is the 87s that are different. Any demonstration that the 87s go farther than a previous year's ball would satisfy a lot of people."

I wasn't sure it would satisfy me. Hall had told him about my statistical notions. He cleared that issue up right away.

"What the fans want to find out is what's different about the *ball*; that's what everybody is asking. Just take it for granted that it's going farther."

"But if it really isn't going farther, as a *ball*, and it's just due to the warm weather this year, or the fact that there are no new pitchers this year (are there?) and everybody's got books on the old ones and practiced more, or there are more new young hitters this year than usual, or at several parks they've moved the fences in (have they?) since last year, what's the point of testing the baseballs?" I protested.

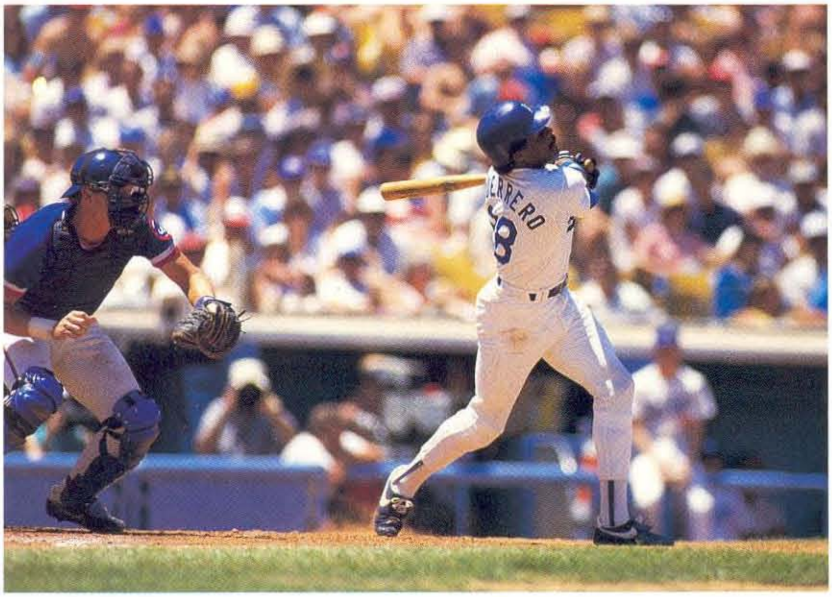
"It would still make a good story," the reporter said.

OK, back to the number of 1986 (?) balls.

"Couldn't you get more?"

"Well, that's a tricky point."

He didn't want people to know what he was working on, so he didn't want to call up all the major league teams and ask if they had any 86s left. Naturally, any communication with the manufacturer was out. Where did all the left-over balls go at the end of a season? I asked. Did Rawlings sell its surplus (we assumed it had one) to the minor leagues, Mexican leagues, Central American leagues? How were the balls dealt with anyway? Apparently they are delivered to each major league team, but umpires, who keep them in a locked room, are actually in charge of the balls and prior to each game they rub down a sufficient number to last through the game. Since a team wouldn't want to run out, they must have plenty left over; so what did they do with them at the end of each season? No one knew. Maybe they were kept for spring training. Did they use any 1987 balls in spring training? Obviously, all these questions could have been answered by a few calls to the Dodgers and the umpires, but we had to keep everything under wraps.



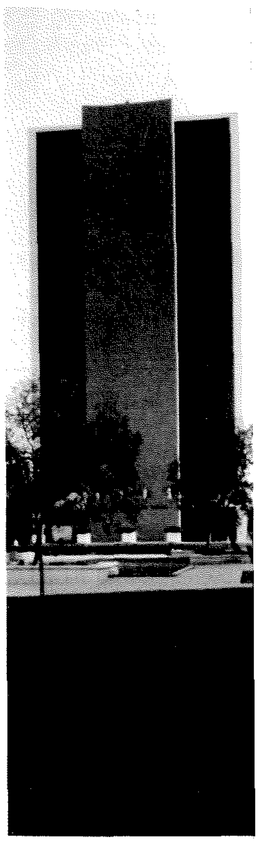
Then the fun began. Research is research. There's equipment, sensors, data acquisition, statistics, theory, and analysis. . .

Guerrero points out the destination of a home-run ball (June 1987).

Next, we discussed money. I needed support for a graduate student through the summer—a few thousand dollars if the newspaper paid him directly—and another couple of thousand to build some test equipment, since the reporter was serious about actually testing balls. It looked as though we'd have to build something to hit them with. He indicated that the total amount of money was acceptable to the newspaper. We agreed I'd put together a proposal after some more thinking and contact him in a couple of days.

Then the fun began. Research is research. There's equipment, sensors, data acquisition, statistics, theory, and analysis, whether you are bashing a baseball, running a rat, shaking a structure, or deifying DNA. I got together with a graduate student to run through a research plan. What could we do in the way of testing a baseball for impact and flight characteristics? Obviously, one or two 86 and 87 balls could be sliced open ("sacrificed" is what the biologists call it) and examined. There is a core (what's it made of?) wound with string (composition?) under some tension, and covered with a hand-stitched leather cover. The overall weight and diameter were regulated, but was some variation allowed in the two properties? If so, how much? Could the *average* weight or diameter of 10,000 1987 baseballs have differed slightly from that of the 86s? Presumably, we could tell if the core and string were made of different materials in each year, but it might take quite sophisticated equipment to tell whether, say, the core or string materials varied in chemical content if man-made, or whether their sources were different if the materials were natural. How could we tell if the string *tension* varied from one year to the next? I didn't see how we could *un*wind the string and measure the tension. We might unwind each ball and measure the length (under a small tension) and weight of the resulting pile of string. But we could only do this for one or two balls, unless another 1000 or so arrived from a miraculous donor. The results would likely be meaningless, for two balls. Needless to say, the dissected balls were of no value for further tests, say, impact tests, so our 1986/1987 stock would be depleted by one or two for each season.

Leaving the ball autopsy aside for the moment, we considered static compression tests. It would be easy to put a ball in a compression test machine and measure the amount of its compression at various force levels. We would have to see if there were any Edgerton pictures of a baseball at impact, showing how much it was deformed, so that we could estimate how much static compression of the ball would be representative of a home-run impact. This might turn up something, but it was a test that would have to be done on a *lot* of balls. However, we would have to assume that a massively crushed ball would not be a valid candidate for later impact tests, and our supply would thereby be diminished by another ball or two. There might be quite subtle reasons why one season's balls would fly farther than another's. A typical baseball in a ball game might be hit a few times, but probably not very many; a slight scuff



The first thought that came to mind was to drop a baseball from a high tower and measure the height of its rebound.

leads to its retirement. (Where does a retired baseball go? The umpire gives it to a ball boy, who normally adds it to the fungo bag for the next practice.) It was possible that each season's balls might have the same compressibility initially, but that one set might have a greater resilience after one or two hits, and thereby go a bit farther. It would be necessary to test each ball a few times.

After a lot of thought about *static* tests, it became clear that, no matter how they might be performed, the tests would be unlikely to resolve questions of the dynamic performance of a baseball. In addition, there was the consideration of the audience for the test results. We ought to perform fairly simple tests that the average fan could relate to, and whose results would be credible.

The first thought that came to mind was to drop a baseball from a high tower (say, Millikan Library), and measure the height of its rebound. That raised a number of questions. A brief calculation indicated that the terminal velocity of a baseball in free fall in air of about sea level density is about 130 feet per second; a second estimate showed that Millikan, at about 110 feet, was not high enough for the ball to reach terminal velocity. Now, at ball/bat impact in a baseball game, the relative velocity of baseball and bat appears to be about 200 feet per second, so we are faced with a problem. During contact, a baseball is certainly severely deformed; the impact process is what we call in mechanics "nonlinear." In simple terms that means that a ball arriving at a stationary bat at 200 feet per second does not leave the bat at twice the velocity of a ball striking the bat at 100 feet per second. If we want to find out, in as straightforward a fashion as possible, what happens when a baseball in a real-life ball game strikes a bat at 200 feet per second, then we have to have a baseball strike something resembling a bat at 200 feet per second.

That was not all with regard to drop tests, though. They are not all that easy to do. What should the ball hit? Presumably, it should be a piece of hardwood (rather than a steel plate, say), but the wood would have to be flat, as we could not guarantee an impact point to the required precision if we actually had a bat there. In fact, how much scatter (wind, etc.) would we get for a succession of balls dropped from the same place? How big would the piece of wood have to be?

The next consideration was the measurement to be made. The obvious thing was the height of bounce, which might be 50 feet plus. How would we record that accurately? We'd need to drop a few store-bought balls just to establish the general bounce height range, since we could not use up our precious supply of test balls. Then we could put up a marked (feet, inches) board at that elevation, and film each test with a movie camera, although where to locate the camera was not all that obvious either. A better method would be to record the velocity of the ball just before and after impact, but some thinking was needed to figure out how to do that, too, with an uncertain impact point each time. Could we drop the ball down a tube (it would have to be perforated, or consist of guides only, to let the air get out of the way) with photoelectric sensors at the bottom? Would it bounce straight up the tube? All in all, it didn't seem too good an idea after all to drop the balls off a building. More thought had to be given to another test.

What it boiled down to, eventually, was another dynamic test, preferably simulating as closely as possible the contact of bat with ball that occurs in the baseball field. In golf, there is a machine designed to perform such a test. It rotates a golf club, in a reasonable simulation of a real golf swing, to make contact with a golf ball on a tee, and drive it into a typical flight. The machine is called an "Iron Byron" because the motions are said to be a mechanical representation of the swing of Byron Nelson, formerly an eminent professional golfer. It is seldom used in golf ball commercials presumably because it could determine *accurately* whose ball went farther. The situation in golf is quite different

from that in baseball. There are many golf ball and club manufacturers, who must make the equipment in conformity with specifications required by the Professional Golfers Association (PGA). Many people, including professionals, play golf, and fairness requires that all use balls and clubs meeting the PGA specifications. Weight and diameter of the ball can be easily controlled, but the impact and flight characteristics demand a dynamic test for completeness.

In baseball, the ball is, of course, moving.

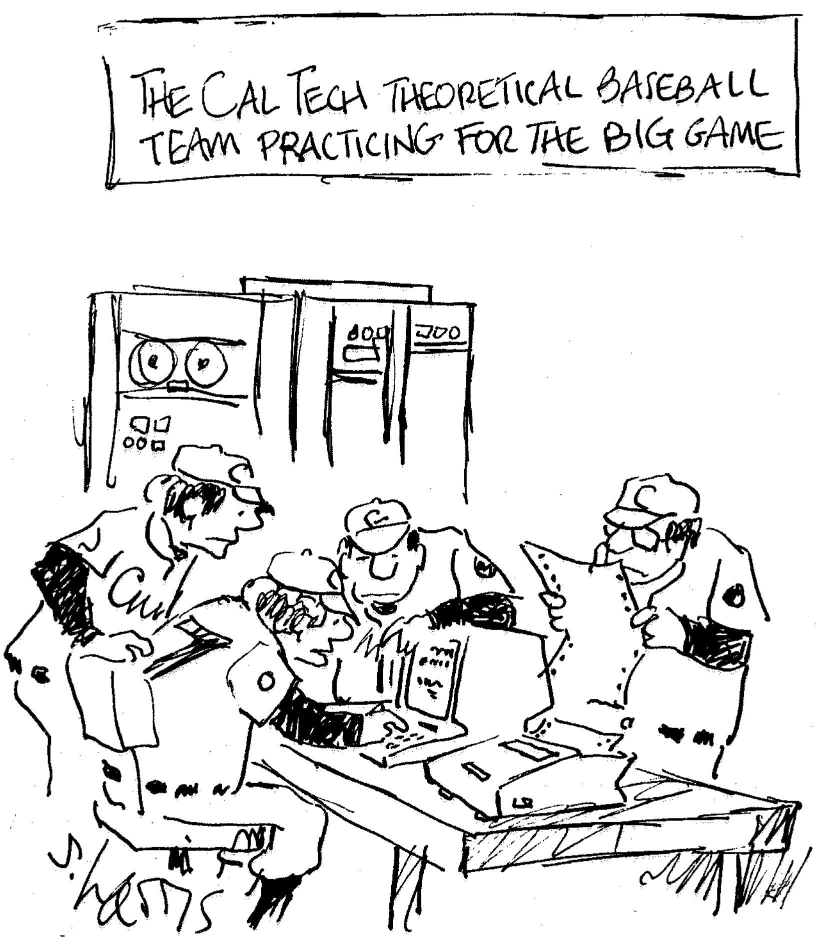
In baseball, there is essentially only one market for professionally used baseballs—the major leagues; and there is only one class of user—professional players. Here there is only one manufacturer. Batters of both teams in a game hit balls from the same batch; whatever ball is produced, everyone uses it. Perhaps it is for this reason that a dynamic baseball testing device does not appear to exist. (Or *is* there one, hidden in the hills of Haiti?) Anyway, we were not permitted to inquire. It appeared, therefore, that we would have to devise our own machine. Since we originally envisioned completing the research in a relatively short period, we decided to design this device first, so that the lengthy period of construction could take place while we undertook other tests.

Complicated though Iron Byron is, at least all the club-head has to do is hit a stationary ball. In baseball, the ball is, of course, moving. Maybe this is why we couldn't find any stroboscopic flash pictures of contact between a bat and a baseball; you don't know where precisely to aim the camera. After a short period of consideration, we decided we could not hit a moving ball either, and came to the conclusion that we would have to knock a ball off a tee, which meant that the bat would have to move a bit faster.

That brings us to the question of the bat. We should incorporate a real bat in the device, to which the bat would have to be clamped. That would be easier to do with a metal bat, although major league bats are required to be wooden. A metal bat, in fact, simplifies a number of problems (consistency, reliability, no break), so we considered using one. A baseball bat swing is, more or less, in a horizontal plane, so our first thoughts were to have a vertical axis and horizontal arm, arranged to clamp a baseball bat so that it would make contact precisely and repeatably with a ball on a tee at a radius of about four ft. from the axis (about the distance of the impact point on a held bat from the axis of the hitter). Initially, we considered that a spring (coil or torsional) mechanism would be appropriate, so that the bat arm could be rotated away from the ball—winding up the spring—and cocked. We never got as far as sketching out a trigger mechanism, obviously no trivial component. The whole system would have to be pretty rigid, not in the interest of realism but of repeatability. The spring would store a fair amount of energy, so safety was involved. We did not want to measure the distance a graduate student would travel.

After impact, the arm and bat would have to be stopped, which would require a shock-absorber mechanism, arranged so as not to break the bat or arm while stopping it. It would still

We ought to perform fairly simple tests that the average fan could relate to, and whose results would be credible.

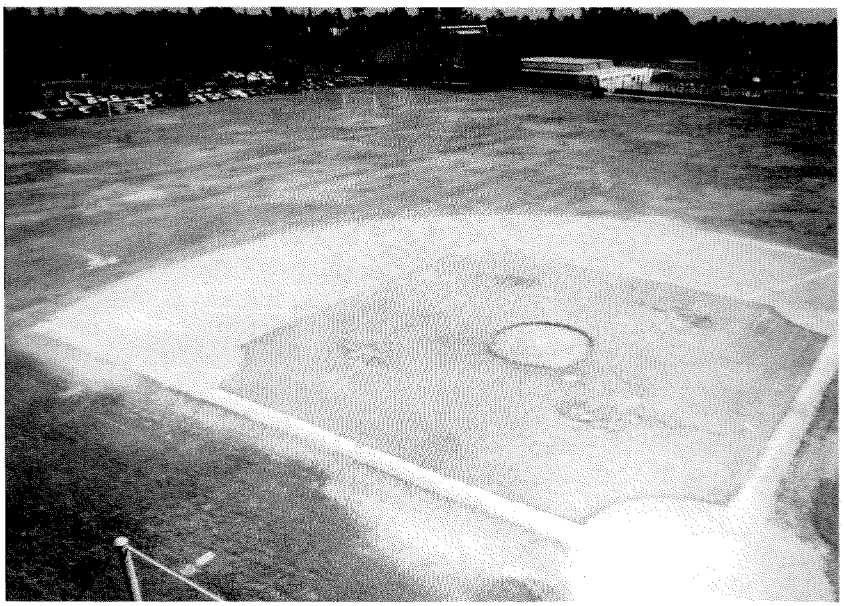


be necessary to measure the initial velocity of the bat at impact, so that differences in the range of travel could be related to small variations in the impetus. The mechanism required to do all this was so violent that brief consideration was given to the design of a system in which the arm holding the bat was rotated by a motor, with rotation speed measured precisely, so that the impact velocity would be controlled. However, the rotation speed for an impact-point velocity of 200 ft. per second would have to be about 500 rpm, and the time interval between successive passages of the bat over the ball point would be about 1/8 second, in which time the ball would have to be introduced in such a way as to be stationary at impact. We also worried about the way the bat might be held. Ideally, the suspension should have the resilience or compliance and damping of a human bat handler, but how would you measure that, let alone reproduce it in a piece of machinery (apart from spinning up a volunteer)? I decided to leave that problem for later. This approach, therefore, seemed to possess the disadvantages of complexity. In addition, both these devices, spring-loaded and rotation, would be costly to manufacture and debug. These difficulties caused us to turn our attention to a different scheme, one in which an actual bat would not be used, but which might be simpler to construct and operate.

This device, which we never got so far as to design, was merely conceptual; it was to consist of a gun. An impacting piston, incorporating a piece of baseball bat, and energized by a spring or compressed air, would travel up a tube to make contact with a stationary baseball at about 200 ft. per second, propelling the baseball a horizontal distance of, say, 400 ft., roughly typical of a home run's travel. Contact would be arranged to impart some back spin to the ball about a horizontal axis, since this spin modifies the lift and drag to which the ball is subjected in flight, and has a substantial effect on the range achieved. The device could be relatively safe, with suitable arming and triggering precautions, and could be precisely aimed and locked at suitable azimuth and elevation angles. A certain amount of practice with store-bought balls should produce the initial calibration data we would require.

As part of the calibration process, we would have to measure the emitted ball's velocity each time (and spin rate, too) since we could not hope to make all impacts identical, and we would have to allow for variations in range resulting from slight changes in the initial conditions. It didn't seem that this would be too hard to implement.

This brought up the question of the actual tests, however: Where and how were they to be performed? The ball would travel in a typical home run trajectory, horizontally about 400 ft.



The deserted Caltech baseball field at night might have been the site of the baseball tests—but wasn't.

and perhaps to 150 ft. in height at a maximum. Since we would be striking 20 or 30 balls about ten or more times each, with each shot requiring some initial preparation and subsequent measurement, several minutes would be needed for each test, and the whole process would consume hours. Consequently, wind and temperature would be factors. Ideally, the tests should be done indoors, in a large air-conditioned building; a closed baseball stadium (Astrodome, King Dome) would be ideal, but obviously we could not conduct tests in such a partial (as opposed to impartial) location. How about a large aircraft or airship hangar? What size are the largest ones? I didn't know. The easiest thing to do was to conduct the tests outdoors in a suitable playing field, for example, at Caltech. For obvious reasons, we would want to work in private. It would not be suitable to have a number of people milling about, possibly prone to beaning by the batted ball, and harassing the distance measurement team. There are significant extra hazards to performing outdoor experiments at Caltech, too—audience suggestions, for example: "Why don't you make the launch tube of neodymium?" "Is gravity the same here as in Kansas City?" —and the usual $\ddot{x}$, F^*_{ij} quantum electrodynamics stuff. From all these considerations (time, temperature, throng) the only reasonable way of testing *al fresco* would be to work from, say, 2 to 5 a.m. There would be some other problems associated with working in the dark, of course, but with a relatively constant trajectory providing a landing ellipse not greater than 20 ft. in major axis length and less in the minor, plus radio-communications between mission control and target personnel, flashlights, hard hats, and maybe body armor, we should be able to get by.

The landing area was another puzzle. The best solution seemed to be to use a large shallow box, filled with sand of just the right density to bring the terminal baseball to a stop without its bouncing, burial, or damage—the ideal sand trap of the other sport, or of the long jump. Box edge markings and strings would facilitate the measurement of distance. To eliminate possible bias among the experimenters it would be necessary for the shooters to be ignorant of which ball (1986 or 1987) they were projecting, but on the other hand, the balls would have to be marked in code so we could keep track of the number of times each was struck. The code would ensure that the terminal team didn't know what the ball was either, when they measured its range. Then, before a test, the marked balls would be scrambled in a box and picked out blind for loading.

This process, imperfect as it was, took a few hours of thought; then we were ready to communicate with the reporter. I told Hall Daily where we were in the experiment design and he said he'd call the newspaper. Word came back that they weren't interested anymore. I presumed that the whole thing had been a figment of the reporter's imagination, and that when he told his editor about the project, the latter had told him he was deranged if he thought the newspaper would finance such a dubious research project.

As everyone now knows, home-run production fell off markedly in the second half of the season; no one surpassed Babe Ruth's record or Roger Maris's asterisked total, and discussion of a spiked baseball fell to zero. And in the 1988 season, home-run production dropped to a pretty low rate.

All the groundwork has been done, however, if the American League or National League wants to dig into the dynamics of a bonked baseball in the future. But count me out. □

Ronald Scott, the Dotty and Dick Hayman Professor of Engineering, has been a member of the Caltech faculty since 1958. He grew up in Scotland (his BSc is from Glasgow University) where he played cricket, not baseball. He continued playing cricket as a graduate student at M.I.T. (with a West Indian team) and even played a few times on a Caltech team. He has always played golf.

It's as Simple as One, Two, Three...

by Richard P. Feynman
as told to Ralph Leighton

Richard Feynman, Nobel laureate and the Richard Chace Tolman Professor of Theoretical Physics, was a bestselling author as well. When he died last February 15, the sequel to the 1985 "Surely You're Joking, Mr. Feynman," Adventures of a Curious Character *was already finished. Entitled* "What Do *You* Care What Other People Think?" Further Adventures of a Curious Character, *the book recounts more Feynman tales, including this one, as told to his friend and drumming colleague, Ralph Leighton. The book, which also contains an account of Feynman's service on the Challenger commission (expanded somewhat over the article that appeared in* E&S *Fall 1987) will be available at the end of this month.*

Feynman's curiosity also extended to drawing. He was a persistent artist, according to Leighton. "He was always amazed by the fact that a good drawing seemed to come out randomly," said Leighton. "Since he could never predict when a good drawing would appear, he drew a lot." The drawings that accompany this article also appear in the book and were part of a collection exhibited at Caltech last March at the time of Feynman's memorial service.

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When I was a kid growing up in Far Rockaway, I had a friend named Bernie Walker. We both had "labs" at home, and we would do various "experiments." One time, we were discussing something—we must have been 11 or 12 at the time—and I said, "But thinking is nothing but talking to yourself inside."

"Oh yeah?" Bernie said. "Do you know the crazy shape of the crankshaft in a car?"

"Yeah, what of it?"

"Good. Now tell me: how did you describe it when you were talking to yourself?"

So I learned from Bernie that thoughts can be visual as well as verbal.

Later on, in college, I became interested in dreams. I wondered how things could look so real, just as if light were hitting the retina of the eye, while the eyes are closed. Are the nerve cells on the retina actually being stimulated in some other way—by the brain itself perhaps—or does the brain have a "judgment department" that gets slopped up during dreaming? I never got satisfactory answers to such questions from psychology, even though I became very interested in how the brain works. Instead, there was all this business about interpreting dreams, and so on.

When I was in graduate school at Princeton a kind of dumb psychology paper came out that stirred up a lot of discussion. The author had decided that the thing controlling the "time sense" in the brain is a chemical reaction involving iron. I thought to myself, "Now, how the hell could he figure that?"

Well, the way he did it was, his wife had

But it was an interesting question: what does *determine the 'time sense'? When you're trying to count at an even rate, what does that rate depend on? And what could you do to yourself to change it?*

a chronic fever which went up and down a lot. Somehow he got the idea to test her sense of time. He had her count seconds to herself (without looking at a clock), and checked how long it took her to count up to 60. He had her counting—the poor woman—all during the day; when her fever went up, he found she counted quicker; when her fever went down, she counted slower. Therefore, he thought, the thing that governed the "time sense" in the brain must be running faster when she's got fever than when she hasn't got fever.

Being a very "scientific" guy, the psychologist knew that the rate of a chemical reaction varies with the surrounding temperature by a certain formula that depends on the energy of the reaction. He measured the differences in speed of his wife's counting, and determined how much the temperature changed the speed. Then he tried to find a chemical reaction whose rates varied with temperature in the same amounts as his wife's counting did. He found that iron reactions fit the pattern best. So he deduced that his wife's sense of time was governed by a chemical reaction in her body involving iron.

Well, it all seemed like a lot of baloney to me—there were so many things that could go wrong in his long chain of reasoning. But it *was* an interesting question: what *does* determine the "time sense"? When you're trying to count at an even rate, what does that rate depend on? And what could you do to yourself to change it?

I decided to investigate. I started by counting seconds—without looking at a clock, of course—up to 60 in a slow, steady rhythm: 1, 2, 3, 4, 5. . . . When I got to 60, only 48 seconds had gone by, but that didn't bother me; the problem was not to count for exactly one minute, but to count at a standard rate. The next time I counted to 60, 49 seconds had passed. The next time, 48. Then 47, 48, 49, 48, 48. . . . So I found I could count at a pretty standard rate.

Now, if I just sat there, without counting, and waited until I thought a minute had gone by, it was very irregular—complete variations. So I found it's very poor to estimate a minute by sheer guessing. But by counting, I could get very accurate.

Now that I knew I could count at a standard rate, the next question was—what affects the rate?

Maybe it has something to do with the heart rate. So I began to run up and down the stairs, up and down, to get my heart beating fast. Then I'd run into my room, throw myself down on the bed, and count up to 60.

I also tried running up and down the stairs and counting to myself *while* I was running up and down.

The other guys saw me running up and down the stairs, laughed, and asked, "What are you doing?"

I couldn't answer them—which made me realize I couldn't talk while I was counting to myself—and kept right on running up and down the stairs, looking like an idiot.

(The other guys at the graduate college were used to me looking like an idiot. On another occasion, for example, a guy came into my

I found I could arrange them in geometrical patterns—like a square, for example: a pair of socks in this corner, a pair in that one; a pair over here, and a pair over there—eight socks.

room—I had forgotten to lock the door during the "experiment"— and found me in a chair wearing my heavy sheepskin coat, leaning out of the wide-open window in the dead of winter, holding a pot in one hand and stirring with the other. "Don't bother me! Don't bother me!" I said. I was stirring Jell-O and watching it closely; I had gotten curious as to whether Jell-O would coagulate in the cold if you kept it moving all the time.)

Anyway, after trying every combination of running up and down the stairs and lying on the bed, surprise—the heart rate had no effect. And since I got very hot running up and down the stairs, I figured temperature had nothing to do with it either (although I must have known that your temperature doesn't really go up when you exercise). I fact, I couldn't find anything that affected my rate of counting.

Running up and down stairs got pretty boring, so I started counting while I did things I had to do anyway. For instance, when I put out the laundry, I had to fill out a form saying how many shirts I had, how many pants, and so on. I found I could write down "3" in front of "pants" or "4" in front of "shirts," while I was counting to myself but I couldn't count my socks. There were too many of them: I'm already using my "counting machine"—36, 37, 38—and here are all these socks in front of me—39, 40, 41. . . . How do I count the socks?

I found I could arrange them in geometrical patterns—like a square, for example: a pair of socks in this corner, a pair in that one; a pair over here, and a pair over there—eight socks.

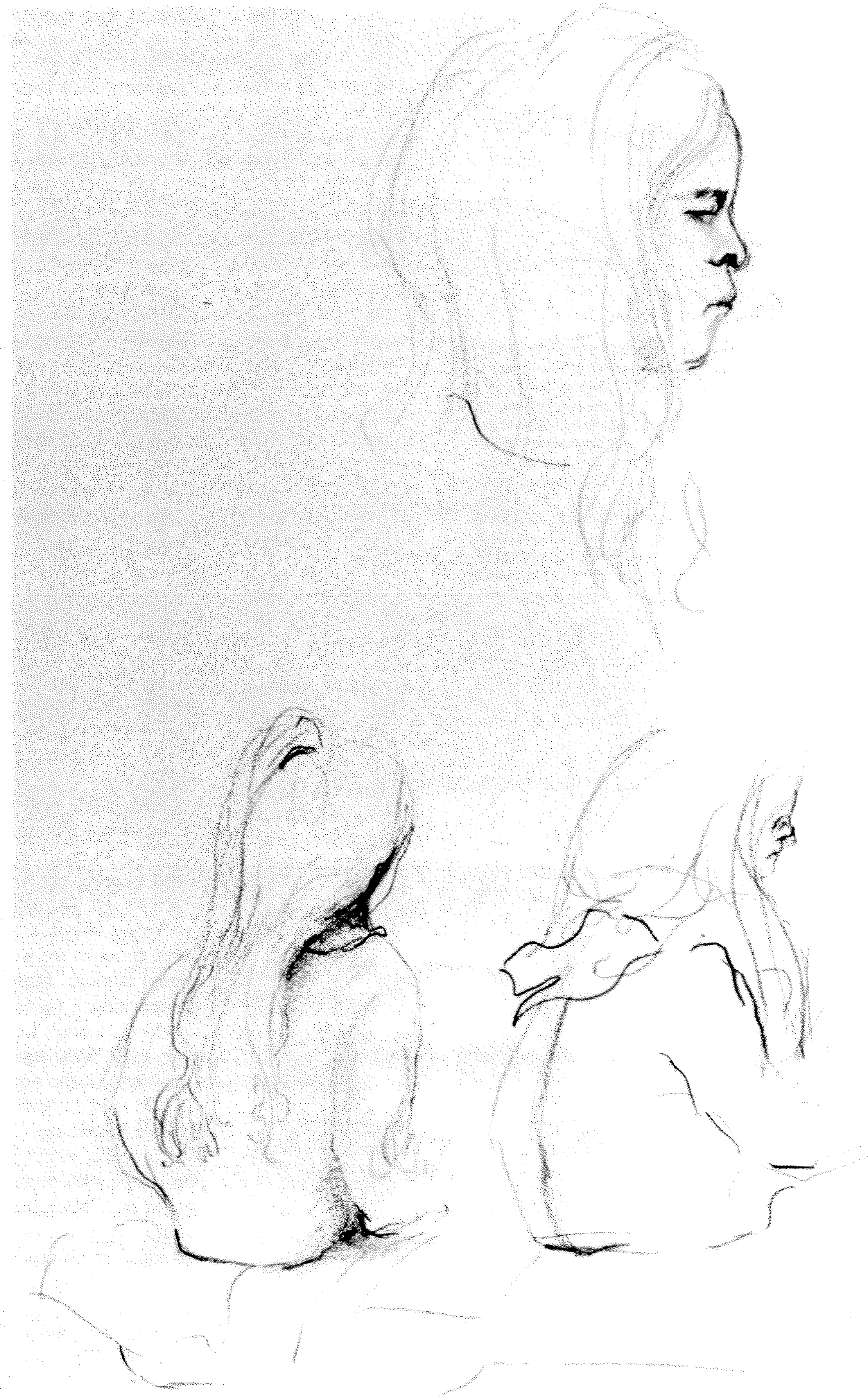
I continued this game of counting by patterns and found I could count the lines in a newspaper article by grouping the lines into patterns of 3, 3, 3, and 1 to get 10; then 3 of those patterns, 3 of those patterns, 3 of those patterns, and 1 of those patterns made 100. I went right down the newspaper like that. After I had finished counting up to 60, I knew where I was in the patterns and could say, "I'm up to 60, and there are 113 lines." I found that I could even read the articles while I counted to 60, and it didn't affect the rate! In fact, I could do anything while counting to myself—except talk out loud, of course.

What about typing—copying words out of a book? I found that I could do that, too, but here my time was affected. I was excited: finally, I've found something that appears to affect my counting rate! I investigated it more.

I would go along, typing the simple words rather fast, counting to myself 19, 20, 21, typing along, until—What the hell is that word?—Oh, yeah—and then continue counting 30, 31, 32, and so on. When I'd get to 60, I'd be late.

After some introspection and further observation, I realized what must have happened: I would interrupt my counting when I got to a difficult word that "needed more brains," so to speak. My counting rate wasn't slowing down; rather, the counting itself was being held up temporarily from time to time. Counting to 60 had become so automatic that I didn't even notice the interruptions at first.

The next morning over breakfast, I reported



Well, now it was clear: he's 'looking' at his tape going by so he can't read, and I'm 'talking' to myself when I'm counting, so I can't speak.

the results of all these experiments to the other guys at the table. I told them all the things I could do while counting to myself, and said the only thing I absolutely could not do while counting to myself was talk.

One of the guys, a fella named John Tukey, said, "I don't believe you can read, and I don't see why you can't talk. I'll bet you I can talk while counting to myself, and I'll bet you you can't read."

So I gave a demonstration: they gave me a book and I read it for a while, counting to myself. When I reached 60 I said, "Now!" —48 seconds, my regular time. Then I told them what I had read.

Tukey was amazed. After we checked him a few times to see what his regular time was, he started talking: "Mary had a little lamb; I can say anything I want to, it doesn't make any difference; I don't know what's bothering you"— blah, blah, blah, and finally, "Okay!" He hit his time right on the nose! I couldn't believe it!

We talked about it a while, and we discovered something. It turned out that Tukey was counting in a different way: he was visualizing a tape with numbers on it going by. He would say, "Mary had a little lamb," and he would *watch* it! Well, now it was clear: he's "looking" at his tape going by so he can't read, and I'm "talking" to myself when I'm counting, so I can't speak.

After that discovery, I tried to figure out a way of reading out loud while counting— something neither of us could do. I figured I'd have to use a part of my brain that wouldn't interfere with the seeing or speaking departments, so I decided to use fingers, since that involved the sense of touch.

I soon succeeded in counting with my fingers and reading out loud. But I wanted the whole process to be mental, and not rely on any physical activity. So I tried to imagine the feeling of my fingers moving while I was reading out loud.

I never succeeded. I figured that was because I hadn't practiced enough, but it might be impossible; I've never met anybody who can do it.

By that experience Tukey and I discovered that what goes on in different people's heads when they *think* they're doing the same thing—something as simple as *counting*—is different for different people. And we discovered that you can externally and objectively test how the brain works: you don't have to ask a person how he counts and rely on his own observations of himself; instead, you observe what he can and can't do while he counts. The test is absolute. There's no way to beat it; no way to fake it.

It's natural to explain an idea in terms of what you already have in your head. Concepts are piled on top of each other; this idea is taught in terms of that idea, and that idea is taught in terms of another idea, which comes from counting, which can be so different for different people!

I often think about that, especially when I'm teaching some esoteric technique such as integrating Bessel functions. When I see equations, I see the letters in colors—I don't know why. As I'm talking, I see vague pictures of Bessel functions from Jahnke and Emde's book, with light-tan *j*'s, slightly violet-bluish *n*'s, and dark brown *x*'s flying around. And I wonder what the hell it must look like to the students. □

"Safecracker Suite," a tape of Feynman recounting one of his most infamous exploits (interspersed with Feynman and Leighton on drums) may be ordered by writing to Box 70021, Pasadena, California 91107. Cassette tape is $10; CD, $15. All proceeds go to cancer research.

"What Do You *Care What Other People Think?"* will be available in bookstores at the end of October, or it can be ordered directly from the publisher.

Please send me _______ copies of *"What Do* You *Care What Other People Think?"* at $17.95 per copy.

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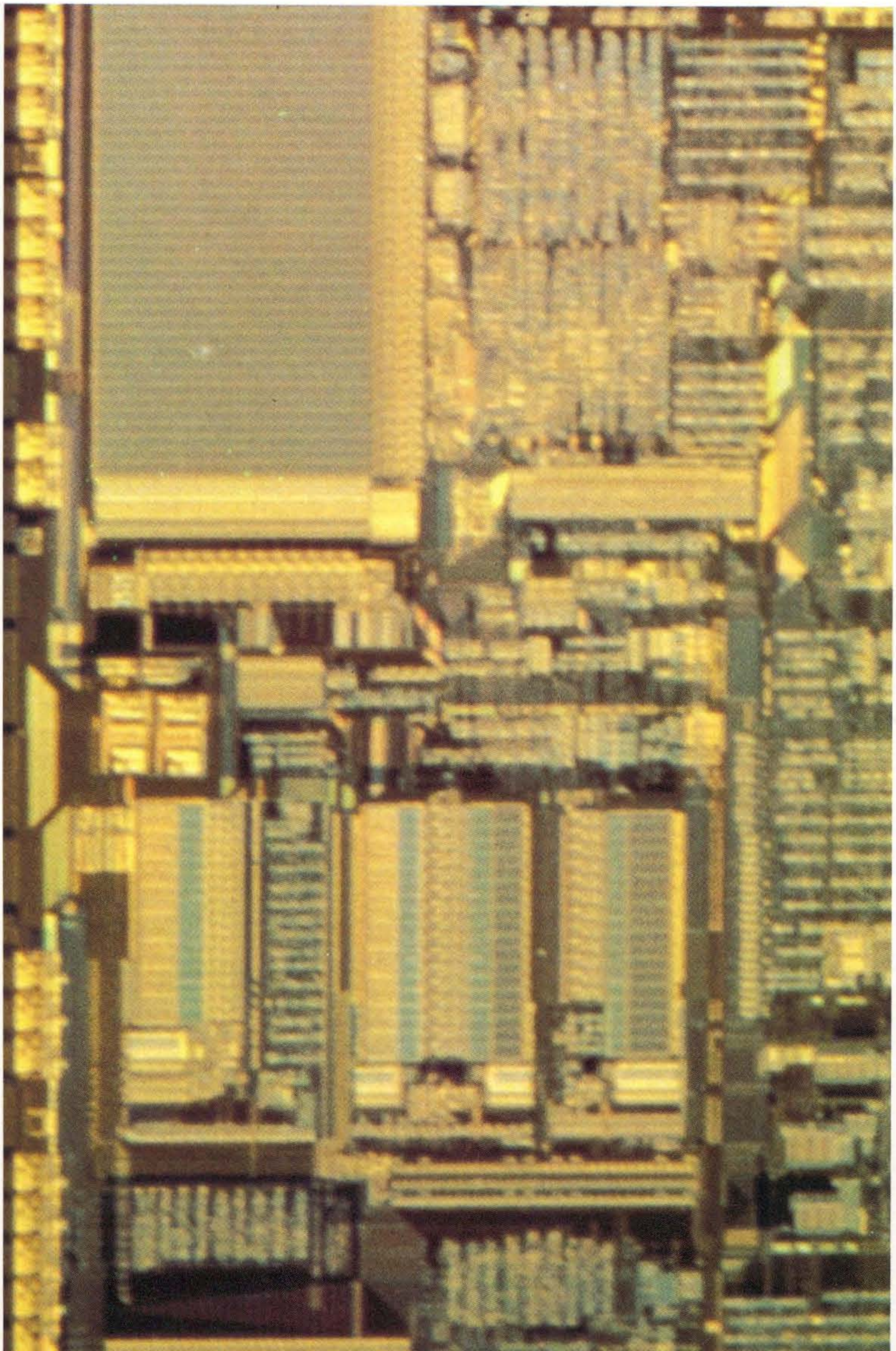


Have You Used Your 4 Million Transistors Yet This Year?

Future trends in microcomputing

At left, detail from the Intel 80386 microprocessor, one of the most wanted chips today. The image is blurry because a photographer's lens is not as good as the quarter-million-dollar ones used to print the pattern on the chip.

More electronics are manufactured each year than existed in the world at the beginning of that year—10^{15} transistors this year, according to Gordon Moore, chairman of Intel Corporation. That translates into a million transistors per person in the developed world, or 3-4 million per family. So your family will have to consume 4 million transistors this year alone, says Moore. Next year it will be 8 million, and 16 million the following year. Although some of your transistors have found homes in your automobile, microwave oven, and TV set, most have been gobbled up by the microcomputing industry, which has mushroomed from nonexistence a decade ago into a $20-billion business today.

What the future holds for a field of such phenomenal growth was the subject of four public lectures at Caltech last spring, "Future Trends in Microcomputing," a series that the Institute hopes to continue next year. Organized by Barry Simon, Caltech's IBM Professor of Mathematics and Theoretical Physics, and Professor of Theoretical Physics Geoffrey Fox (who is also associate provost for computing), the lectures brought world leaders in the fields of microcomputing, as well as overflow audiences, to Beckman Auditorium on campus.

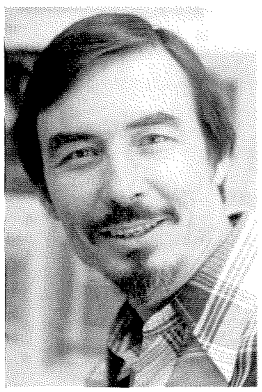
Besides Moore, the speakers included Benjamin Rosen, Carver Mead, and Philippe Kahn. In 1957 Moore co-founded Fairchild Semiconductor, which built the first integrated circuit; he and Bob Noyce then went on to found Intel, which invented the microprocessor in 1971. Moore and Rosen are both members of the Caltech Board of Trustees, and they, as well as Mead, hold Caltech degrees. Rosen (BS '54) remembers that when he was a freshman, Moore (PhD '54) was his chemistry teaching assistant. "He gave me a D," Rosen said.

Rosen co-founded Sevin Rosen Management Company, a venture capital firm with large investments in the microcomputer industry. He is also chairman of the Compaq Computer Corporation, a corporation that grew to have annual gross sales greater than a billion dollars, faster than any other in history; he was a founding director of the Lotus Development Corporation and is, as well, a director of Borland International, Inc., Bestinfo, and Quarterdeck Office Systems.

Mead (BS '56, MS '57, PhD '60), now the Gordon and Betty Moore Professor of Computer Science at Caltech, built the first workable gallium arsenide transistor, and his contributions to the theory of quantum tunneling were essential to the invention of the integrated circuit and the microprocessor. He is a well-known innovator (and textbook author) in VLSI and is currently doing pioneering work in neural networks.

Kahn, an immigrant from France, founded (and is currently president of) Borland International, Inc., which produces the popular software programs SideKick, Quattro, and Paradox. His innovative software, sold at discount prices, is challenging the industry giants. Kahn founded Borland in 1983 with $5,000 out of his own pocket, because all the venture capitalists ("including me," Rosen admits) turned him down. Rosen describes him today as "the most outspoken person in the industry."

Benjamin Rosen



Carver Mead

Gordon Moore

Philippe Kahn

"If the same kind of progress had been made in the auto industry over the past seven years, you'd go a million miles per hour and get half-a-million miles to a gallon of gas."

In the beginning of his talk, Mead traced the development of the microcomputer industry from the introduction of the printed circuit board ("the first big step") through the invention of the transistor in 1947, and then the first integrated circuit, made by Fairchild in 1959. "None of us saw this as the beginning of a revolution," noted Mead. "It is characteristic of great inventions that most people—even those working in the field—notice them only when they are adopted."

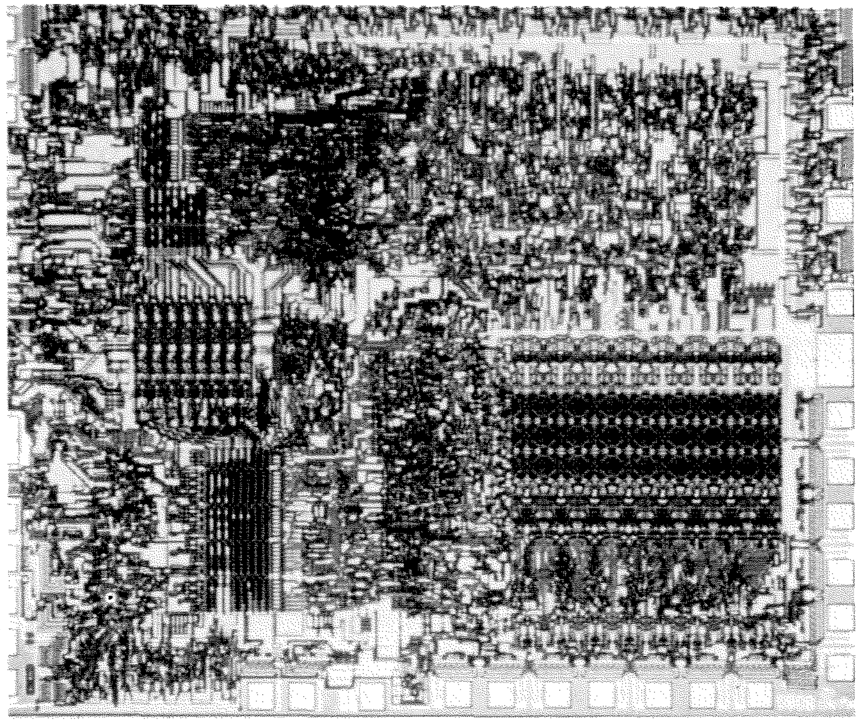
Rosen, in his talk, also described the "hypergrowth" of the personal computer industry as a revolution. That revolution was created out of three ingredients, he claimed—"technology, entrepreneurs, and money." But, as successful as this revolution has been, the microcomputer industry has still penetrated only 20 percent of its potential market, according to Rosen. Apparently we haven't all consumed our 4 million transistors this year.

Several of the speakers offered comparisons of "then" and "now" to illustrate just how far and how fast the microcomputing industry has ballooned. Moore compared "IBM's top-of-the line personal computer for 1987 to a big mainframe computer like the IBM 370, model 168, top of the line in 1975. The PC has four mips (million instructions per second, a measure of computing power) instead of two—twice the power at 1/34th of the price. If the same kind of progress had been made in the auto industry over the past seven years, you'd go a million miles per hour and get half-a-million miles to a gallon of gas. It would be cheaper to throw your Rolls away than park it downtown in the evening. I thought that was a neat analogy until someone pointed out that the Rolls would be only six inches long and two inches wide."

"Look at the six years from mid-'81 to mid-'87," said Rosen. "Memory chips went from 16,000 bits (the fundamental information unit —a binary 1 or 0) to 1,000,000 bits, a 64-times increase. The microprocessor word length (the number of bits it can handle per computing step) has doubled; floppy disk storage densities have gone from 160,000 to almost 1,500,000 bytes (a group of bits, usually six to eight, that represents a text character or a processing instruction), an eight-fold improvement. The Winchester hard disk, introduced in '83, held 10 megabytes (million bytes). Now you can get 300, a 30-times improvement. And microprocessor speeds have quadrupled. If you multiply all these factors together you get an absolutely specious figure of merit, so I can categorically say the personal computer is 122,880 times better than it was 6 years ago."

Mead provided another comparison: "The cost of a chip today is about the same as the cost of one of those individual transistors we used to solder onto circuit boards. Yet the capability represented by that chip has gone up by a factor of more than a million. The Industrial Revolution, which substituted fossil fuels for human and animal power—and gave us smog and urban waste and all the other good things about modern society—gave us, in terms of getting from the East Coast to the West, or printing a book—an increase of a factor of about 100."

Taking examples from his own experience,



The first commercial microprocessor, Intel's 4004 chip, built in 1971, contained about 2,200 transistors. It addressed 9.2 K of memory designed for arithmetic applications or control functions.

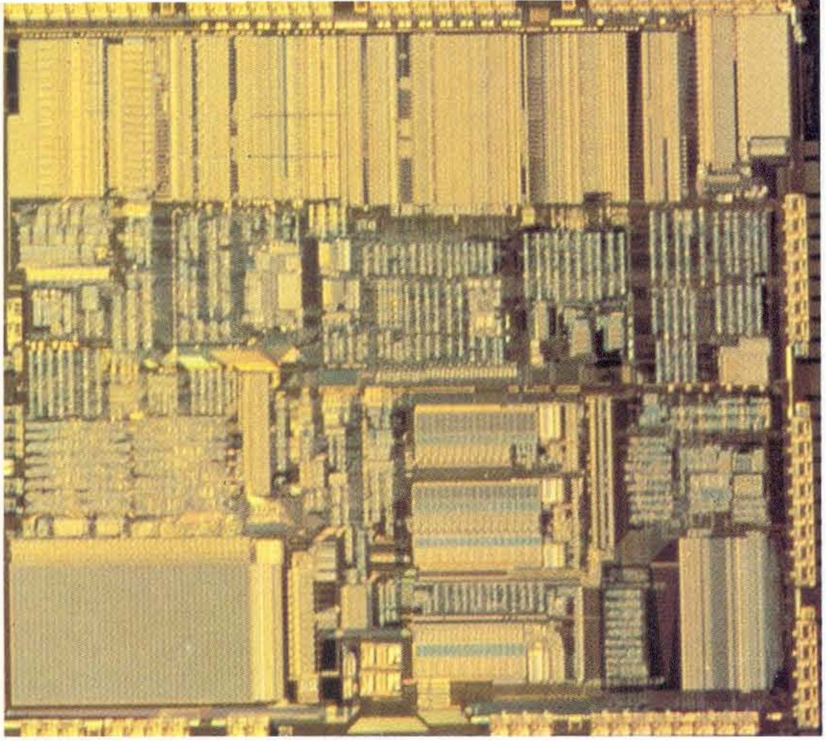
Moore contributed some insight into how this explosion happened. "As we've learned to pack more and more electronics on a given area of silicon, the standard chip becomes increasingly complex. The technology may exist to make something even more complex, but if the design costs dwarf the manufacturing costs, it will be cheaper to build your system from simpler products. That's why Intel got its start making memory chips—it's a universal function.

"This leads to the idea of a transistor budget—the maximum number of transistors you can put on a chip and still manufacture it economically. So how do you use the budget to make practical devices? Early in Intel's history, a Japanese company that wanted to make a family of calculators came to us. (There were hardly any Japanese semiconductor companies then.) They had designed some 13 logic chips, all quite complex and far beyond our ability to undertake. One of our engineers, Ted Hoff, suggested that he could get all their functions using a general-purpose computer architecture and some stored programs, and went on to point out that the same chip could be used for elevator control, traffic-light control, and a whole bunch of dedicated logic operations. And that was the origin of the microprocessor.

"That was in 1971. The 4004 chip had about 2,200 transistors, right against the limit of our transistor budget then. It was a 4-bit microprocessor addressing 9.2 K (thousand bits) of memory (on another chip), designed for arithmetic applications or control functions. Since it had a 4-bit word length, there were 16 potential instructions you could give it. As the technology developed, we added the 8080, which had about 8,000 transistors. It was an 8-bit microprocessor, alphanumeric-oriented, aimed at data-processing applications; it addressed 64 K of memory and was actually the basis of the first personal computer, as far as I know. There was a machine called the Altair that came as an 8080 and a bunch of stuff in a kit, and you assembled it at home. But the 8080 was still mostly used as a dedicated controller—it wasn't big enough to be really reprogrammable, like a stand-alone computer. I'm talking about Intel because I know the data, but the trend is true for the other manufacturers as well.

"And the budget kept growing. Two years later, the 8086 had about 30,000 transistors—over ten times what the 4004 had. The 8086 had a 16-bit word length, and addressed one megabit. It was big enough to separate the data interface from the central processing unit, so it could walk and chew gum at the same time. With 16 bits, it could receive some 64,000 instructions, plenty for high-level programming languages. It was fully reprogrammable, in other words. In fact, the 8088—essentially the same chip—was the processor IBM chose for their first PC.

"Next came the 80286, with about 125,000 transistors. It addressed 4 billion bits, I think, enough to use the high-capacity hard disks that were just coming out. Plus it had multi-user capability—different programs could run simultaneously, and its hardware kept the data for each program separate. It's the basis for the



The 80386 is a full 32-bit processor, with 64 trillion bits addressable, designed to use multiple operating systems simultaneously.

"We think the 80386 will pass the 80286 by 1990, and by 1992, it will be dominant. It'll be the chip for all seasons."

IBM PC-AT and all its clones. A lot of the budget had to be used for compatibility. The 80286 had to run all the software written for the 8086 and 8088. So the increased budget went for compatibility, performance, and memory management. The current step, the 80386, is a full 32-bit processor, with 64 trillion bits addressable, designed to use multiple operating systems simultaneously. The rest of the budget went to increased ease of use and compatibility."

The 80286 was introduced in the IBM PC-AT at a four percent market share. "In three years' time," Rosen pointed out, "the 80286, with no operating system or applications programs designed specifically to take advantage of it, but simply by being faster, took over 53 percent of the market. And the 80386 has had a much faster start. We think the 80386 will pass the 80286 by 1990, and by 1992, it will be dominant. It'll be the chip for all seasons."

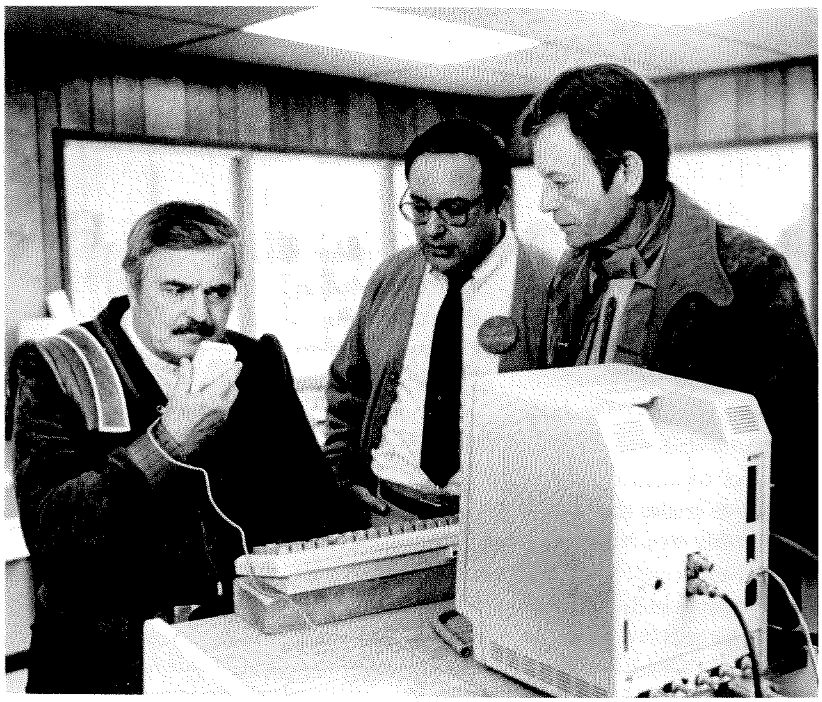
But the chip designers are racing on ahead. Said Moore, "If we do a linear extrapolation, in 1990 we'll have 2 million transistors per chip—about 7 times the 80386, and in the year 2000 we'll have about 50 million. What features might we put on a 2-million-transistor chip? Faster execution. And you can add a lot of memory on-chip, so the machine isn't always waiting to get information from memory chips. You could add a floating-point arithmetic processor, which consumes some 70 or 80,000 transistors—only a couple percent of the budget. We could add a variety of other dedicated processors. It will have a lot of parallel processing capability, and hardware fault tolerance—redundant circuits built into the chip. And a lot of the 2 million will go to compatibility—why abandon $10 billion worth of existing software?

"What benefits will the user see? Simplified networking, improved graphics, and I hope they'll be a lot easier to use. Or we could put a whole simple computer system on one chip, greatly reducing the cost of a run-of-the-mill microprocessor."

Rosen also had some predictions for the near term: "Fortunately, John Adams didn't close down the patent office in 1799, although he wanted to, feeling that everything to be invented already had been. In the next five years or so, I don't think there will be an increase in word length; 32 bits is absolutely adequate to meet all our needs in memory addressability and in the speed you need to communicate with disk drives, printers, video displays, and so forth. You will, however, see this basic 32-bit architecture, whether it's Intel's or Motorola's, go up in performance at lower cost. You will also see lots more co-processors—graphics co-processors, more advanced math chips, better input/output processors. Chip memory and disk storage will continue to grow, all at a much lower cost per bit, of course, and with faster access as well. Displays are going to higher resolution, and I think you'll see flat, low-power, color displays for portables in a few years. Further miniaturization—in a few years, a ten-pound portable with more functionality than any PC today."

For the year 2000, according to Moore, predictions are much tougher. "What do you do with 50 million transistors? That's two hundred

"Can you imagine an office environment where you have 50 people talking to computers?"



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Talking to computers is a matter of course in the 23rd century, but it's not so easy in the 20th, as Chief Engineer Scott discovers. In this scene from *Star Trek IV: The Voyage Home,* **he is unsuccessfully trying to speak to a Macintosh, using its mouse as a microphone.**

80386 chips in one. It's a mind-boggling amount of electronics. We could put every function we've ever built to date on one chip. We'll definitely put a lot of software on the chip and increase its parallelism.

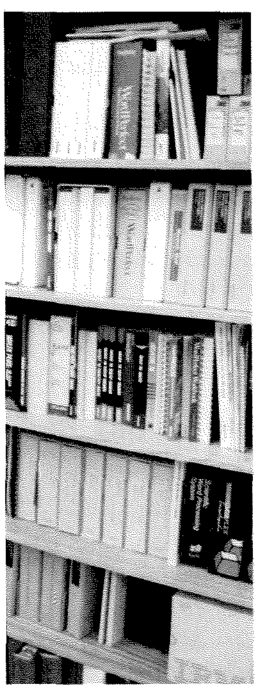
"The user's benefits will include speed: desktop computers that execute billions of instructions per second. Systems interconnection will be very easy, resulting in local and global networks and instant access to data at a level we can hardly conceive of today. A lot of the budget will go to the human interface. I hope to never open a manual again after the late '90s. And I hope a lot of the artificial intelligence functions really come into play."

Artificial intelligence has been slow coming to PCs because they haven't had enough horsepower, Rosen claimed in his lecture. "AI is a hog. It requires a lot of speed, lots of chip memory, and lots of disk storage, but now with the third generation of personal computers we finally have the hardware to go with the software's requirements. AI includes pattern recognition, handwriting recognition, expert systems, machine intelligence, natural language use, and speech recognition. When we get a system that recognizes continuous human speech, regardless of who's talking, we'll be able to dispense with the keyboard. The keyboard is a big impediment to anyone who doesn't use it frequently."

Kahn and Moore were less sanguine about the imminence of voice input. "The user interface—how the user gets information into and out of a system—is a surface," said Kahn, "and the depth of what's available in the computer lies underneath. There's an evolution going on from DOS-type (computer prompt and command input by keyboard) to graphical, a set of pictures on the screen and a pointing device (a light pen or a 'mouse') to select the function wanted. Some people want to get to a natural-language interface, so you can talk to the computer the way we're talking now. Can you imagine an office environment where you have 50 people talking to computers? And what if someone calls you on the phone while you're talking to it? It's like a videophone—do you really *want* people to see you on the phone? (I've got a phone in my bathroom.) So it's an interesting proposition, and the technology will exist to do it, but do you want it all the time?"

"I think it will be well into the next century before we're really comfortable with voice input instead of the keyboard," Moore said. "The keyboard is really pretty efficient—if you know how to type. Maybe we'll have to teach

"People love their favorite software, and why shouldn't they—they spent nights *learning it, and the last thing they want to do is to have to learn something else."*



typing in computer science classes."

Rosen saw user-friendliness as an important factor in the industry's future growth. "The 20 percent of the market we have now are the easy sales—the people who want to be first on the block to have one," he said. "How do we get the other 80 percent? It seems contradictory, but we need high-performance computers to attract low-performance users. You've got to make the machine less complicated on the outside by doing more work inside. You need software that's more intuitive and easier to learn—and with consistent interfaces between user and machine, graphics-based, so you can dispense with the manual. People don't read manuals anyway, so you might as well get rid of them. The Macintosh has done a lot in this direction."

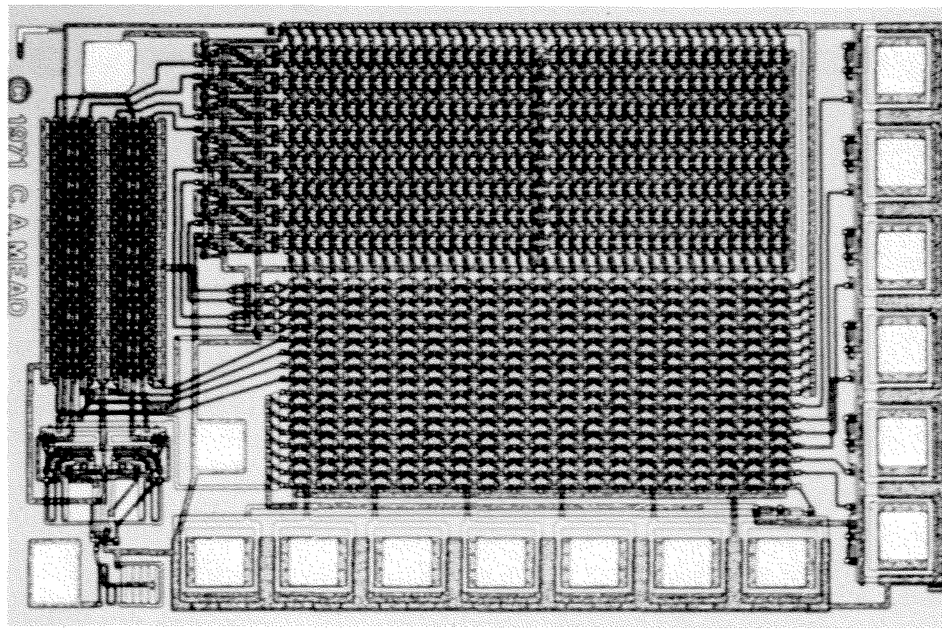
Kahn had a different viewpoint: "Graphic interfaces are not necessarily easier to use. It is easier to get into something, but running it may not be trivial. There are some Macintosh programs now where you have to press SHIFT, COMMAND, SPACEBAR, and move the mouse down to make something happen on screen."

As for future software, Kahn predicted that the main categories—word processing, spreadsheets, database managers, and communications packages—will not change much, "but there will be all these new tools, like AI and parallel processing, to do them with."

"The next thing in word processors will be to make them 'habit-compatible,'" forecast Kahn. "If you like to do things one way, why should you have to learn another way just because some software publisher thinks it's better? The machine should learn to work the way the user works, not the other way round. It's called ergonomicity—the human factor in design.

"People love their favorite software, and why shouldn't they—they spent *nights* learning it, and the last thing they want to do is to have to learn something else. Commands have to be logical and intuitive so people can remember them or figure out how to use them. People were screaming at a product like WordStar, saying it was difficult to use, but at least it had logical ways to remember things. Some of the more 'modern' word processors don't—there is no logical way to remember that pressing SHIFT ALT F4 does whatever it does.

"Software will have to get faster. People *hate* slow software. We all know we're going to die, and we have better ways to spend the time we have than sitting in front of a screen reading, 'Please wait while I process this command.' But the wait is going to get worse if we're not careful, because the processors and architectures we'll be using in PCs for the next several years are single-processor, single-memory-bank architectures managed by multi-tasking operating systems. Which means that instead of one program having all the hardware's resources, you'll be running several applications at once, swapping them all in and out. Accessing rotating (hard) disk storage is the PC's slowest function, because it's mechanical. And you're sharing the processor's time, too. One big, slow application, like sorting a massive mailing list, will penalize the whole multi-tasking system. So software engineers will have to write smaller, faster code.



The first chip designed by a silicon compiler was created by Carver Mead in 1971. As the complexity of chips increased, designing them became more and more difficult. When the silicon compiler became commercially available, complex chips could be designed in days instead of years.

"We all know we're going to die, and we have better ways to spend the time we have than sitting in front of a screen reading, 'Please wait while I process this command.'"

Craftsmanship will be even more important than it is now, perhaps crucial."

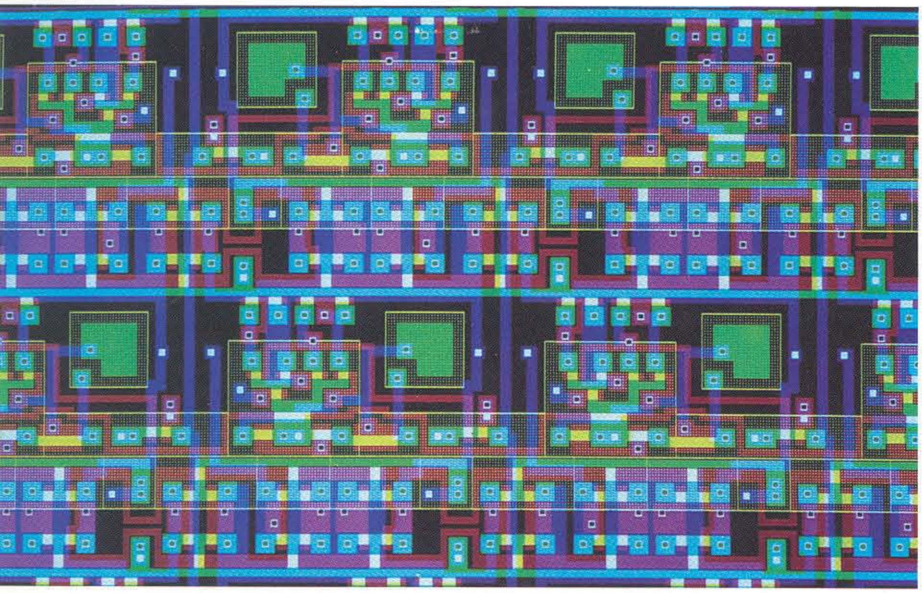
All the speakers noted the disparity between hardware and software development. Moore is convinced that the semiconductor engine will grow as long as the market holds, "so go use your 4 million transistors this year. But will the software to fuel that engine in the year 2000 be ready? Look at the PC-AT, which is basically the 80286. It has all the functions for multiprocessing, but none of the PC software uses it. It's just baggage—unused six years after the chip came out and probably eight years after the software people knew it would be there. I don't see the software catching up. Chips are growing exponentially in complexity and improvements in software are nowhere near the same rate."

Even Kahn agreed that software has to catch up. "Technology will help us build better software," he said. "Software people will have to master technology like AI and apply it where needed. The last thing we want is for software to become sloppy because we're just playing catch-up. There's no point in having more memory and faster processors if it just goes to support software that could be, and should have been, streamlined."

"New software stimulates hardware growth," said Rosen. "Look at the 8-bit, 16-bit, and 32-bit microprocessors. The 8-bit, starting with, say, the Apple-II in 1977, didn't take root until the first applications software came along that business professionals could use: Visicalc, two years later. The same with 16-bits. IBM introduced the PC in 1981. It grew moderately in '82, then explosively in '83. Lotus 1-2-3 was introduced in January of '83, and that was the first application that took full advantage of the 16-bit architecture. Now we have the same situation with 32-bits. Compaq led this generation with the DeskPro 386. But we haven't seen the application yet that will make them really take off. Nothing takes full advantage of the hardware. What is the new 1-2-3, the new Visicalc? If I knew, I'd invest in it.

"It's interesting that most software comes from smaller, more entrepreneurial companies. Compare this to the hardware situation, where IBM, Apple, and Compaq together have a 77-percent market share, and about 100 companies are fighting for the other 23 percent. It's like a supermarket shelf—there is only room for so many brands of toothpaste, and those that can't get on the shelf die. Not too long ago there were so many PC companies going out of business that it was said the industry was entering a new chapter, Chapter 11. To start a new PC company today is impossible. No one will fund a company to compete for unavailable shelf space. . . . But the software companies, such as Microsoft, Lotus, Ashton-Tate, Borland, WordPerfect, Autodesk, have all started off very small and have stayed largely software-only. The big hardware manufacturers have either not tried, or have been very unsuccessful at creating PC software. I think creating software is a discipline that lends itself better to a small, dedicated company than to an appendage of a large manufacturer of iron. And software companies can still get started today, they're still fundable by the venture-capital community."

The emergence of standards has been critical



Mead's silicon retina, which contains a 48 x 48 array of these cells on a tiny chip, mimics the processes of "slippery, gooey neurons." The colors in this computer represent the semilayers, which perform the neural processes of the retina's layers of cells.

to developing software as an industry in its own right. According to Mead, "Standardization has unleashed a wave of innovation in software—many bright, innovative people were able to concentrate on individual applications. That development has had an immeasurable effect on the economy. Although it doesn't show up in any of the standard measures of productivity, it has allowed us to do things we couldn't have imagined doing previously."

But it wasn't always that way. "In 1980," said Rosen, "there was a Tower of Babel in operating systems. There was Apple DOS, which didn't talk to TRS-80 DOS, which didn't talk to Commodore PEP/DOS, which didn't talk to IBM. Then, when Microsoft's MS/DOS came out in late '81, we had a *de facto* standard, at least for business users, for quite a while. This had a galvanic effect. . . .

"That's changing now. Microsoft will release the last version of DOS, Version 3.4 this year. DOS is going to dominate the business market for at least another two years, until OS/2, released in the last two months, kicks in. OS/2 is an IBM/Microsoft joint release. There are over a thousand new OS/2 applications that will be on the market by 1989. OS/2 will probably pass DOS in 1991. In the meantime, Macintosh has become a force in the business market with its operating system, another standard. But even though there's no longer a single standard, each is large enough now to attract software developers and other support companies to it, ensuring they'll all have strong growth in the coming years.

"I'd like to show you what happens when you take the long view of technology. Almost without fail, when technology changes, the leadership changes too. If you look at calculators, the big names in electro-mechanical calculators were Frieden, Marchant, Victor, Monroe—where are they now? They aren't. We have Hewlett-Packard, Casio, Texas Instruments—a new set of players. Components—vacuum tubes were led by RCA, Raytheon, GE, and Sylvania—they're barely participants in the semiconductor industry. Or computers, as we've gone from mainframes to minis to PCs. Look how sleepy almost all the mainframe companies, with the exception of IBM, were as we went to minis. Look how sleepy the minis have been as we've moved to PCs. Or in software—look what happened to Visicalc when Lotus 1-2-3 came along; WordStar once had almost 100 percent of the word-processing market, and then WordPerfect and 20 others passed it by. There's a lot of inertia in business, and I think it behooves all of us to remember that today's complacency could well become tomorrow's obituary. It's not that some domestic or foreign competitor obsoletes us, we obsolete ourselves. Only those companies that keep innovating, keep pushing the state of the art, survive."

Mead looked at the reason behind the inertia. "Breakthrough technologies come from a direction not foreseen by the existing industry or predicted by the analysts. This may sound like an amateur taking a potshot at the professionals, but that isn't my intent. A breakthrough technology, by definition, is not part of the existing

"There's a lot of inertia in business, and I think it behooves all of us to remember that today's complacency could well become tomorrow's obituary."

culture that's established in companies; great inventions come out of left field. And there's a corollary to that observation: New technologies are adopted last by the companies that need them most. That's because they're not part of the culture that drives the company. Therefore, they won't be seen because they're contrary to what was successful in the past. Once you've built a successful culture, it is difficult to see your environment in a new way.

"Fortunately, we have an entrepreneurial system. We depend on the innovations of the citizens of a free economy to keep ahead of the bureaucrats, ahead of the people who make a living by controlling and planning. In the long term it is the element of surprise that gives us the edge over much more controlled economies. I think this must be true in any industry that is driven by the intellectual insights that make possible entirely new ways of doing things."

Innovation is alive and well in universities as well as industry. In his lecture, Mead described some of his work at Caltech on custom chips designed for specific applications. Using a VAX for the electronic synthesis of high-quality music, the simulation takes 600 times longer than real time. "So we designed a chip whose architecture was specifically crafted for this task. One such chip simulated the instrument in real time. That chip had the effective power of about 600 VAXes."

Mead has also built a chip that simulates the neurons in an animal retina (*E&S*, June 1987). "It does a fantastic amount of computation at a level that can't be done by a supercomputer. (Those slippery, gooey neurons are at least a billion-fold more powerful than our biggest supercomputers.) I believe that building silicon chips that compute analogously to our carbon-based nervous system will be the next fundamental step in electronics," Mead predicted.

As for the outlook for U.S. competitiveness in the microcomputer industry, the speakers were optimistic to varying degrees. Rosen concluded his talk with four observations. "First, the microcomputer industry was created in the U.S. because of our unique entrepreneurial technology sector—our tradition of individualism going back to the first homesteaders, of people willing to take chances. There is no stigma attached to failure here; you can always pull up stakes and try something else. That's less true in Europe, and in Japan it's very hard to fail with honor. Second, after 10 years, the U.S. still leads the microcomputer world, and by a wide margin, if I may be chauvinistic. I think we're likely to continue, both in hardware and in software, for many years to come. Third, microcomputers are the fastest growth industry ever, with lots of room still to grow; and finally, I think that microcomputers are going to become the dominant part of the entire computer industry in the 1990s."

From the vantage point of a chip manufacturer, Moore seemed a bit less enthusiastic. "It's of significant concern that most of our memory chips are now built overseas," he said, "as our systems manufacturers found out in the current shortage. The U.S. now produces only a couple of percent of the world's D-RAMs (dynamic random access memory chips). The dynamic RAM was the product that got Intel going, and we dropped out several years ago because we couldn't see a return on investment there, with the Japanese in particular just pouring money into market share. Once you lose an industry like that, it doesn't come back. It's not just a case of getting incrementally cheaper—I think right now we could probably make D-RAMs as cheaply as the Japanese can. But it has to get to the point where you can see that lasting for a long period of time, and I don't see that. The Japanese are reinvesting in vast amounts of capacity because of the present shortage, and next year they'll probably catch up with demand again, prices will plummet again, and we'll be very glad we're not in the D-RAM business. So we're going to have to get used to our D-RAMS and a lot of other components coming from overseas. A tremendous interdependence is developing. We can expect the Japanese to be major competitors, and we'll continue to see some loss of our chip market, especially as Japan is now a larger market for semiconductors than the U.S., and the Japanese have a tremendous advantage serving that sector. But they also have a very significant disadvantage serving the U.S. market. We have made some progress in trying to get the competition to be more fair than in the past. If we could get free trade, we'd be happy—it's never been free trade. But I don't believe they're going to put us out of business."

Mead, reporting from the thick of the creative end of the business, claims that "there is still plenty of innovation in the electronics industry. We don't need the feds to bail us out. We're doing just fine. There is as much innovation and creativity in this business now as I ever have seen, and there are numerous directions for us to travel in the future."

"The future trend in microcomputing, I think, is eliminating the 'micro,'" said Moore. "Increasingly, microcomputing *is* computing." □

The Answer Is Not Necessarily the Solution

by Robert L. Sinsheimer

Robert Sinsheimer pinch-hit as the commencement speaker for Willy Fowler, Nobel laureate and Institute Professor of Physics, whose wife's death compelled him to cancel his speech. Sinsheimer had been a faculty member at Caltech for 20 years and was professor of biophysics and chairman of the Division of Biology, when he left in 1977 to become chancellor of UC Santa Cruz. Now chancellor emeritus, he returned to Caltech last year as a visiting associate. He is now a professor in the Department of Biological Sciences at UC Santa Barbara.

I want to talk today primarily to the graduates. If faculty and others wish to draw inferences, they are encouraged to do so. Graduates—this is your day—a celebration for you and your families.

What I want to say reminds me of a bit of humor that was current a few years ago. It concerns the later stages of the French Revolution, when paranoia had become rampant and the intellectuals were being systematically executed.

One morning there were three intellectuals who were to be taken to the guillotine—a surgeon, a lawyer, and an engineer. The surgeon was led up first and it was explained that he had his choice—he could be face down or face up. Being a macho type, he chose face up. The executioner then pulled the rope—but the guillotine jammed and the blade did not fall.

Well, under the rules, if your life was thus spared by Divine Providence, you were allowed to go free—so the surgeon was released.

Next came the lawyer, who had to show that he was just as brave as the surgeon so he also lay face up. And the guillotine jammed again. So he was set free.

Then came the engineer. He also lay face up. But then immediately he pointed up and said, "I see the problem. The third bolt is loose and . . . "

The moral is: It's fine to make use of your technical expertise, but you should always be aware of the context.

You have received at Caltech a superb technical education provided by some of the finest scientists and engineers on the planet. This

In a democratic society, in a technological age, every sector must have the opportunity to participate in the creation of the future.

knowledge will serve you all of your life as an invaluable foundation. You are, deservedly, fortunate. You will have the privilege of a life on the frontier of knowledge—a life ever enriched by new vistas of new worlds. You will have the opportunity to participate all of your life in the ongoing, enduring process of scientific discovery and technical invention.

This is a privilege—one that we critically need to extend to representatives of all the diverse segments of our American society. In a democratic society, in a technological age, every sector must have the opportunity to participate in the creation of the future.

This is a golden age for science. Building upon the cumulative discoveries of the past, using ever more powerful instruments, the rates of discovery and invention continue to accelerate. You will not—you cannot—ever cease to learn. The one certainty for the future is change. I have no doubt that more will be learned in the next 20 or 30 years in most areas of science and technology than in all previous times.

When I graduated from the East Coast version of Caltech—back in the late Stone Age—computers and lasers and nuclear power did not exist; quarks and leptons and hadrons were unknown, as were quasars and pulsars and black holes. No one knew the chemical structure of a protein; the nature of the gene was as mysterious as was the surface of Mars. You can be sure that—when you return to Caltech for your 50th reunion in the year 2038—comparably great discoveries and inventions will have been made. The deepest mysteries of matter, of the cosmos, of the mind still await your inquiry.

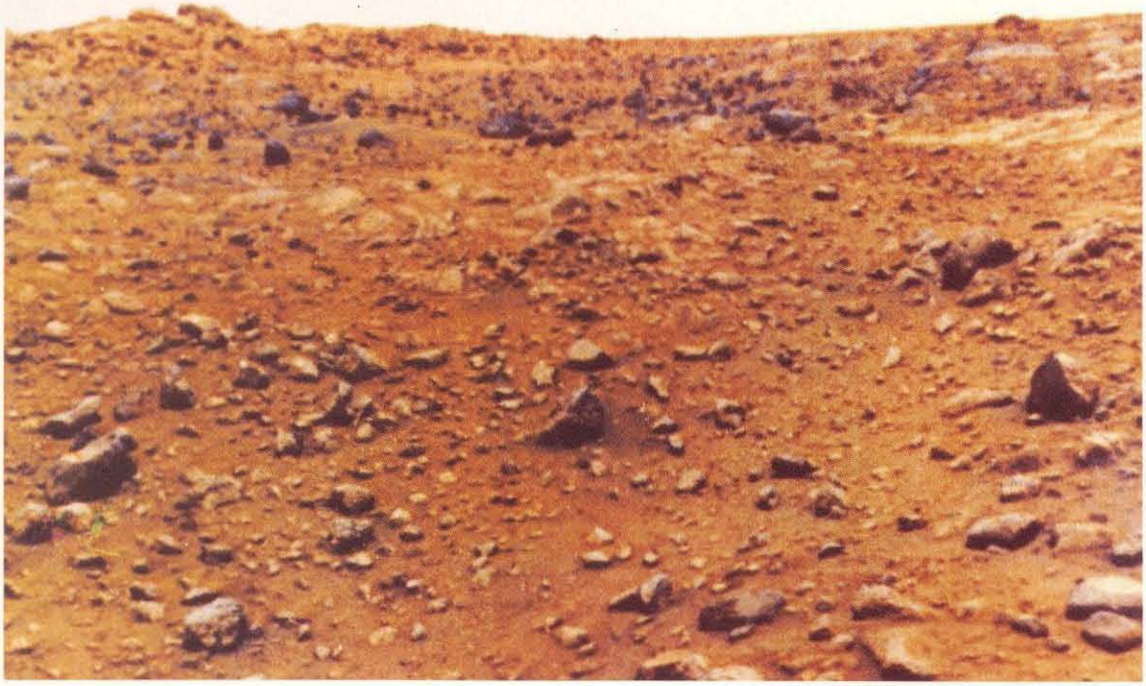
Caltech has many extraordinary strengths: the exceptional quality of its students and faculty; its small size, which minimizes bureaucratic delay and incomprehension; the resources available to it; and, I would mention particularly, its relative homogeneity. Primarily devoted to science and engineering, the Caltech community largely shares a common outlook, a common perception of the world, which greatly facilitates agreement and action. This homogeneity has its manifest benefit—but it may also have its cost. You may not be fully prepared for the tumultuous and diverse world outside these cloistered, cerebral quarters.

I was a member of the Caltech faculty for 20 years and then I served for 10 years as the chancellor of a campus of the University of California, which likes to refer to itself— probably, correctly—as the greatest public university in the world. And so I bring perhaps an unusual perspective in which to view Caltech—and the larger world into which many of you will now enter.

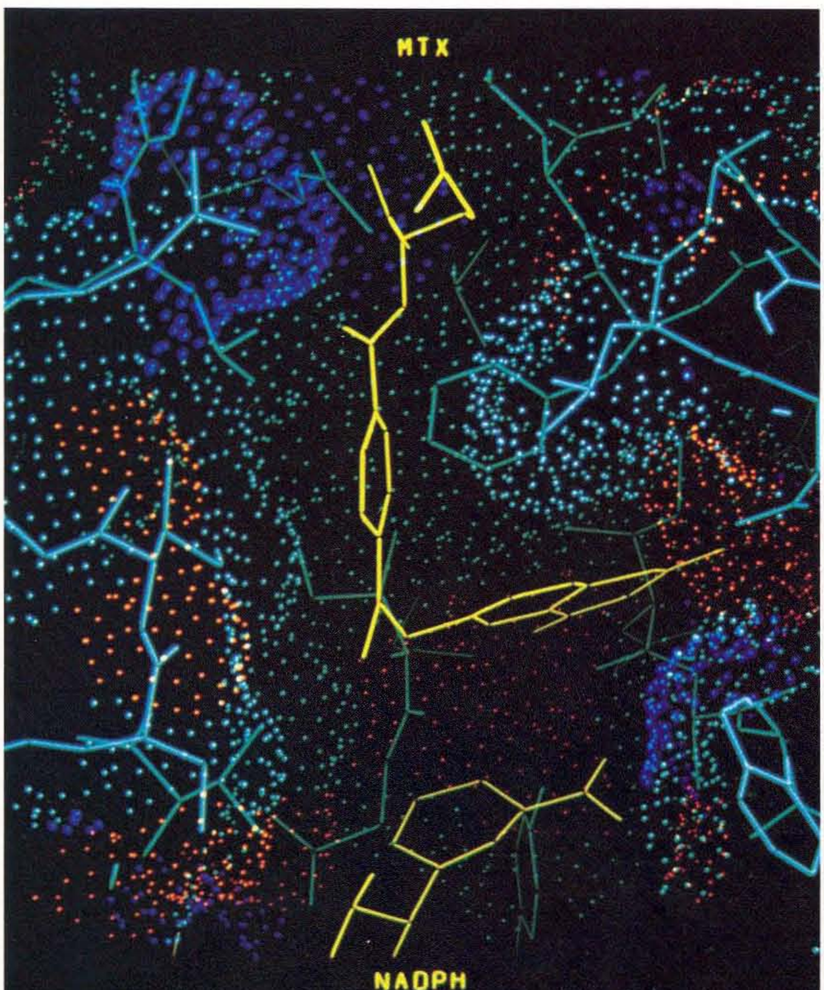
As chancellor of UC Santa Cruz, one of my more interesting but perplexing tasks was to interact intellectually with representatives of disciplines that maintain very different—and very diverse—views of the world and of the value structures appropriate thereto: with artists, who value above all the creative act—on canvas, in stone, on stage, in film; with humanists, who celebrate grace of expression and depth of understanding of the human condition and of the preconditions of human knowledge; with the social scientists, who view science and technology through a very different lens—who wonder about our societal antecedents and about the consequences of our perturbations of the social order.

These varied academic disciplines do at least share a common faith in rationality. Such a belief is by no means universal, as witness the dedicated acolytes of the modern religions, who couch issues not in terms of how or can, but in terms of should or should not, of hubris and humility, of good and evil, of Faust and Pandora. Or witness the animal rights people who presume, on moral grounds, to set animal welfare above the alleviation of human misery; or the reflexive environmentalists who at times can elevate the preservation of an obscure species above manifest human need; or the fundamentalists who firmly believe in an ordained eternal order which we dare not perturb; and so forth.

Bertrand Russell said, "Sin is geographical." Today we might well add, "Sin is occupational."



Before the "golden age of science," no one had seen the surface of Mars (shown here in the first color images from Viking 1) or the structure of a protein. The crystal structure here (depicted and modeled using BIOGRAF simulation tools) shows the active site of the enzyme dihydrofolate reductase, which binds an inhibitor (methotrexate) and a cofactor (NADPH), both shown in yellow. Studying these interactions was the work of Adel Naylor, grad student in chemistry.



I cite these to suggest that in addition to continuing to keep pace with your science or technology you will also, to be effective, need to learn to comprehend—and to match wits with—the advocates of worldviews very distant from your own. For these causes do each have a germ of truth. We should have compassion for animals; we should not heedlessly diminish the diversity of species, for each (as are we) is the inheritor of 3 billion years of evolution; and we should not tamper thoughtlessly and grossly with established tradition. But when these germs of truth sprout into obsession, conflicts arise.

To comprehend other perspectives it is important to recognize your own preconceptions, often unspoken but shared by most scientists and engineers. The credo of scientists is—indeed must be—that knowledge is good and that more knowledge is better and that the quest for knowledge itself is one of the highest forms of human endeavor. You should know that others are not so sure—especially in a world in the thrall of an ethic that strongly favors the swift application of new knowledge to practical purpose. They think of Hiroshima and Chernobyl and Love Canal—and they fear. As Robert Penn Warren wrote, "The end of man is knowledge, but there is one thing he can't know. He can't know whether knowledge will kill him or save him."

Scientists believe firmly in a physical causality even if, in certain circumstances, probabilistic in nature. The initial state determines the secondary state. Things are as they are because they were as they were. Much of the world does not

Scientists have a peculiar and distinct conception of the nature of truth and its relation to falsifiability—which is also not widely shared.

share this belief. Witness the daily horoscopes, Las Vegas, the cults of Nostradamus, and so on. Scientists believe there is a truth that can be found in nature. Others, in their frames, confounded by the distortions of image and the biases of preconception, are not so sure a truth even exists, much less that it is accessible. Scientists have a peculiar and distinct conception of the nature of truth and its relation to falsifiability—which is also not widely shared. And scientists know of the impermanence of the world—the long history which preceded the emergence of our species—the course of stellar evolution which produced the very elements of which we are made, the evolution of the planet as seen in the geological record and the ongoing movements of the tectonic plates, the evolutionary chain of life as recorded in the fossils and even more evident in our very genes. Yet much of the world recognizes no history before the written record and no order beyond that currently accessible to our senses.

The advances in science and technology have released or even engendered vast forces in our society. As one consequence, all of the major problems of our time have a significant scientific or technological component. Consider the following:

- Defense—Star Wars, verification of arms reduction.
- Industry—high tech (while education is distressingly low tech; we have been brilliantly successful in the use of our powerful means of communication for entertainment but we have been woefully unsuccessful in their use for education, thereby creating a grievous imbalance.)
- Health—merely consider the challenges of AIDS and drug addiction and cancer and mental disorder.
- The environment—pollution, the greenhouse effect, the depletion of the ozone layer.
- Ethics—how best shall we manage our new ability to intervene at the genetic level in the living world, which includes *us*.

Those of our persuasion are sure that these problems can only be solved by more knowledge, by better science and engineering. But others, who would somehow selectively retreat from today's reality, will argue that we would merely compound the evil. Thus, all of these problems have other nontechnological components as well, other important, very human dimensions—economic, ethical, ethnic, racial, religious, political, the thrust of ego, the lust for power—which are often of great importance.

Choices will be made; priorities will be set. Good or evil will, indeed, be served. As the custodians of the cumulative knowledge of science, it must be your responsibility in our society to provide the voice of that knowledge—the voice of quantitative projection, of reasoned wisdom—into the din of special pleadings and often fanatic views so abundant in our society. To do so, with any effect, you must understand, you cannot dismiss, the other perspectives. In a conversation I had with David Gardner, the astute president of the University of California once remarked, "The problem with you scientists is that you don't realize the answer is not necessarily the solution."

That sounds paradoxical, but it isn't. As a scientist or engineer you may derive the optimal, analytically effective answer to a problem. But in reality, the answer may not be optimal because it may simply not be politically or socially feasible in our time. And then one must fashion a solution—an alternative answer to the problem—that is attainable. And that requires that you comprehend the other perspectives—and their points of divergence from your own.

Finally, in conclusion, in accord with the spirit of the time, I will cast your horoscope. Your stars have risen in the house of Millikan and Feynman, on the cusp of Everhart, under the sign of the Beaver. You are about to enter the Constellation of Prometheus where you will grow in knowledge and blaze in the firmament of science to guide us into the new millennium. Beware the black holes of ignorance and intolerance. Strive to spread the warmth of compassion and understanding to all in your corner of the universe. □

The Methane-Eaters

"Once we know how these bugs work, we can really *use them to degrade toxic compounds"*

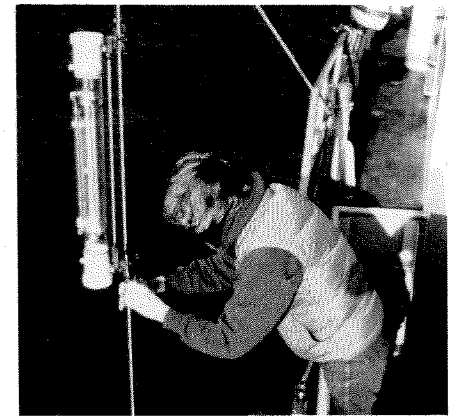
"People always ask me how a lab that's known for doing genetics fits into the Environmental Engineering Science Department," says Associate Professor of Applied Microbiology Mary Lidstrom. "To me, it's crystal-clear. You use the most sophisticated tools at your disposal to solve the problem at hand. It's unusual to combine environmental science and molecular biology, but people are beginning to realize that that's where the solutions to many environmental problems are going to lie."

Consider the methylotrophs, an obscure tribe of inoffensive bacteria who live on methane gas (CH_4). This little-studied family contributes to the natural order of things by removing methane from the atmosphere and converting it into multi-carbon compounds that go back into the food chain. (All animals produce methane as a waste, humans more than their fair share by burning fossil fuels.) But although one-carbon compounds are the main course, it's been found that the methylotrophs can down a side order of chlorinated hydrocarbons simultaneously. Since chlorinated hydrocarbons such as trichloroethylene (TCE) are showing up in numerous cases of groundwater and soil pollution countrywide, researchers are looking to the methylotrophs for a biological solution to a chemical problem.

While the little guys have a limited diet, they aren't nearly as fussy about their lodgings. "You find them in soil, in lakes, and floating around in the ocean—just about anywhere," according to Lidstrom. "Other people have found that there is a small natural population in groundwater, and that its growth can be stimulated simply by injecting methane and air. But that's about as far as you can go with a black-box approach. We're taking a mechanistic approach, looking for the biochemical mechanisms and genetic regulators. Once we know how these bugs work, we can *really* use them to degrade toxic compounds in contaminated aquifers and soils."

There are two parts to discovering a mechanism: the first is determining which genes are involved, and where they lie in the array of chromosomes; the second is finding out what each gene actually does.

To find out which genes are involved in a given process, take a sample of bacteria, irradiate it to induce random mutations in the DNA of individual bugs, clone each bug into a colony, find the colonies where that process has gone haywire, and analyze those colonies' DNA to determine where the mutations occurred. But messing with the meth-



Far left: Dr. Lidstrom counting bacterial colonies in a Petri dish. Left: In the incubator room, where the colonies are grown. Right: Collecting wild bacteria in Framvaren Fjord, Norway.

ylotrophs' digestion turns out to be a tricky business. The methane-eaters are so specialized they can't survive on anything else—they starve on standard culture-dish fare. So mutations that interfered with methane metabolism promptly killed the bacteria, making them tough to study. Fortunately, their first cousins, who live on methanol (methyl alcohol, CH_3OH), can also get by on sugar, so work focused on them.

But now that you have a methanol-eater that can take it or leave it alone, how do you know whether your mutant has a defective one-carbon metabolic system? Methanol dehydrogenase, a crucial enzyme in one-carbon metabolism, also converts allyl alcohol (innocuous to these bugs) to allyl aldehyde (a toxin). Thus any mutants that survive a healthy dose of allyl alcohol have defective systems.

To find out where the mutations were, the researchers go to a "clone bank"—the entire genetic complement of a normal methylotroph chopped into random fragments and cloned. One fragment contains the original version of the gene that was mutated in the bacterium. Each fragment is inserted into a different sample of the mutant bug, using standard recombinant DNA techniques, and the bugs are put out to pasture in methanol. The sample that gets the original gene grows, and the fragment of DNA that went into that bug can be analyzed, the sequence of its amino acids determined, and its position in the set of chromosomes mapped.

Once a gene has been sequenced and mapped, there are several ways to figure out what it does, but that's another story.

Lidstrom helped develop the techniques used to study the methanol-eaters while at the University of Washington in Seattle, before coming to Caltech in May 1987. When she left Seattle, the group had found 10 genes. One gene codes for methanol dehydrogenase itself. Three are involved in attaching the enzyme to its "cofactor"—another molecule the enzyme needs to do its job. One helps stabilize the enzyme and transfer it to where it's needed. One encodes a protein, called cytochrome c, that transfers the energy provided by methanol dehydrogenation to the cell's other metabolic machinery. Four regulate the other genes. The group had made little progress with the methane-eaters, however.

Lidstrom's Caltech group has found three more methanol-eater genes. "One is a previously unknown subunit of the enzyme, which is very interesting. One seems to be involved in regulation, and we have no idea what the other one does. We're in the process of making a mutation of it right now, and we'll see if the mutant can still grow in methanol."

The Caltech group has also been able to crack the methane barrier. According to Lidstrom, "The genes are similar enough that once we get them from the methanol-users we can use them to identify that same DNA in the methane-users. We've looked at five of these genes in the methane-users now. There would be no other way to get those genes."

The genes can also be used to identify and count bacteria in the field. A soil or water sample is chemically treated to extract the DNA from any bacteria present. This DNA is matched against tagged DNA from the methylotroph genes by a process called hybridization. The tagged DNA can be counted in a detector, giving a number proportional to the number of methane-eaters in the original sample. The population data, when correlated with methane and TCE consumption studies at the same site, will show how the bugs behave in the wild.

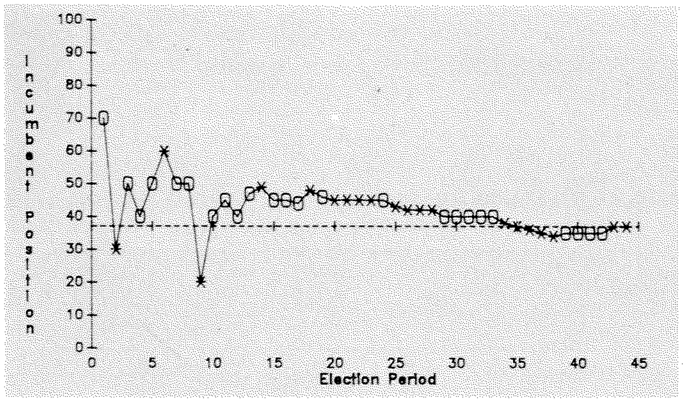
Says Lidstrom, "We've done some field studies already, just looking at population distributions in various environments. We should know enough about the mechanisms to be able to start field tests of methane and TCE consumption in about two years, and we'll have to see how closely our lab work fits with what we get in the field. But conservatively, we should see applications on-site in the next five years."□—*DS*

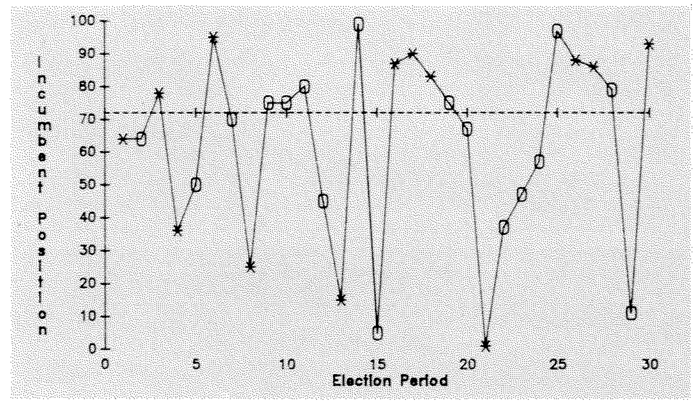
In the experiment's simplest form, voters have no information whatsoever.

Voting in the Dark

Who are your Representatives in the State Legislature? What are their positions on acid rain? If you haven't the foggiest, you're not alone. But a democratic society depends on well-informed voters making rational choices, doesn't it? Think of the Pilgrim Fathers electing William Bradford governor, or the ancient Greeks meeting in the agora to discuss the issues of the day. How does the system work when voters know little or nothing about the candidates and issues? Does it work at all?

Professors of Political Science Richard D. McKelvey and Peter C. Ordeshook are exploring the gap between the traditional civics-text theory of well-informed voters and the reality of a poorly informed public. They work in Caltech's Laboratory of Experimental Economics and Political Science, where researchers investigate aspects of economic and political behavior through simulations in controlled settings. Volunteers play the roles of the entities under study: voters, committee members, corporations, or what have you. As an incentive to play their parts to the hilt, the participants are paid cash according to how well their entities did. A network of personal computers doles out information to the participants, records decisions, and handles all the bookkeeping needed to





Left: Election record for a typical experiment. The dotted line indicates the median policy. The 0 and * indicate which candidate is the incumbent at each election. Thus Candidate 0 is in office before the first election, but is promptly ousted. Both candidates pick more or less random policies at first, but learn from their mistakes. By election 10, they start to converge to the median. Note how the voters "test the waters" every few elections by electing the challenger. This may help drive convergence by showing what the other candidate has to offer.

Right: An experiment that never converged. Although the voters tended to re-elect candidates who stayed close to the median (Candidate 0 in elections 10 – 12, for example), the candidates didn't seem to get the message.

track the experiment.

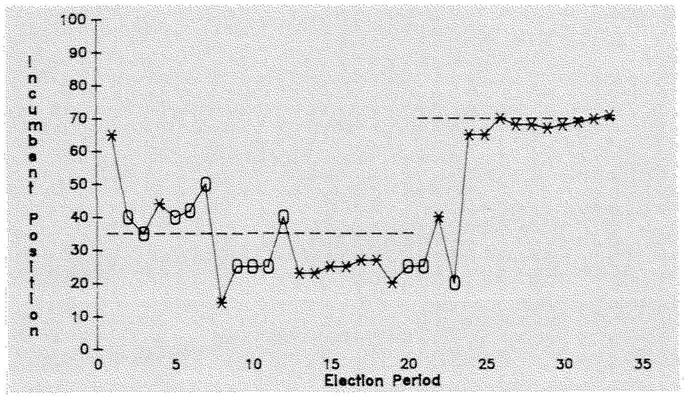
The electorate consists of up to 50 students. Two are candidates, one of whom is in office when the experiment begins. The incumbent selects a "policy" regarding an "issue." Neither has anything to do with the real world. The issue is a linear scale of, say, 0 to 100; choosing a policy amounts to picking a number in that range. Each voter is assigned a "payoff curve" (a plot of policy vs. payoff) that peaks at some random policy number; each curve is different. All voters are paid according to where the incumbent's policy falls on their individual curves: the closer the policy is to the peak, the larger the payoff. Once paid, each voter must decide whether to keep the incumbent in office or to vote for the challenger in the next election. At the same time, both incumbent and challenger select (but do not reveal) new policies. Then the election is held, and all voters are paid according to the winner's policy. The process repeats for 40 cycles or until time expires, when the voters get real money in proportion to the payoffs they have amassed. Candidates are paid in proportion to the number of elections they win.

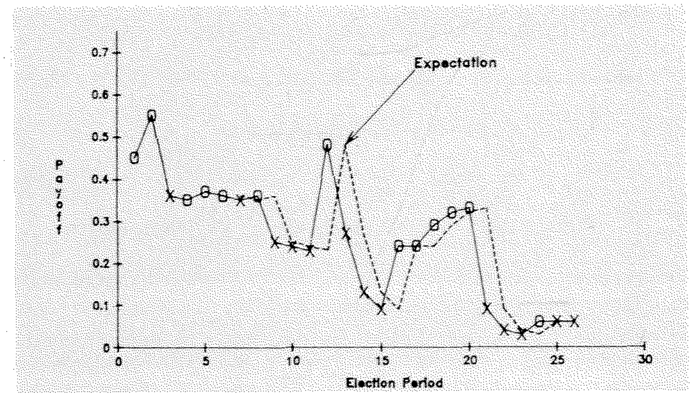
In the experiment's simplest form, voters have no information whatsoever about the candidates' policies, or where their own curve peaks. All they have is their personal history of payoffs under past administrations. Similarly, candidates know only their own policy selection, and who won the election.

The set of payoff curves has a median peak—the one where half of the curves peak to its right and half to its left. The median policy is the candidates' optimum position under majority rule. If a candidate should take a position to the right of the median, for example, all voters to the left of the median would prefer to vote for the median instead. If everyone were fully informed about policies and payoffs, the candidates would immediately adopt the median policy, or the electorate would quickly drive the candidates there by voting for whoever was closest. McKelvey and Ordeshook have found that, over time, candidates still move toward the median in the information-poor experiment described above. In other words, even though voters have incomplete information, the system is still able to function, albeit more slowly.

The experimental voters view their personal histories differently, McKelvey finds. "Some voters just go by the last period—am I better or worse off now? Others give the incumbent the benefit of the doubt. If the payoff drops a little bit, they'll still vote for the incumbent; they take a weighted average over the past few cycles, and only punish the incumbent if the payoff drops significantly. We are still working on a theoretical model for this."

Most runs converge to the median in 10 to 15 cycles. Some never converge, however, if candidates misread the voters' signals. "We also get deviations," McKelvey says, "because some individuals vote at random, or do crazy things. We think that in large electorates, these phenomena would disappear. Individual mis-





Left: When the median suddenly shifts during the experiment, the candidates flounder around their previously successful positions until Candidate * stumbles upon the new median.

Right: A single voter's history. A 0 represents voting for the incumbent, an X represents voting for the challenger. The dotted line shows the voter's payoff from the previous election—if the current administration gives a smaller payoff, this person votes for the challenger.

takes would tend to cancel out statistically, so the consistent behavior of a few people would tend to govern the whole system. Even ten percent of the electorate would be all you need if the rest voted randomly."

In some experiments the payoff curves are changed in the middle of the run, radically shifting the median. This generally throws the candidates for a loop, but only for a few cycles until one candidate stumbles upon the new median. Then both candidates rapidly converge to it.

Current experiments make additional information available to the participants. Voters may be told where their curve peaks, for example. Voters may buy information about the candidates' positions, or the experimenter may publicly announce which candidate's position is more extreme in one direction—equivalent to a special-interest group endorsing the candidate most in line with its position.

These runs also converge to the median. "Voters frequently know a lot more about interest groups and other voters than they know about the candidates," McKelvey notes. "So in real campaigns, you look at the endorsements. Take California. We have all these very complicated propositions on each ballot. Every voter gets a pamphlet with the full text of each measure. But very few voters actually take the time to read them and figure out what they mean, because in that same pamphlet are signed arguments for and against them. Who signs what tells you a lot about the proposition. Trying to dissect the propositions yourself is expensive, in terms of time invested, so you take the more cost-effective method. You read the endorsements, and ask your friends' opinions. And as long as some segment of the electorate opts to be informed, this works fine."

In the latest wrinkle, a candidate is in office for four "years" between elections. A policy is chosen each year, the voters are paid accordingly, and then a poll is taken: if the election were held immediately, would you vote for the incumbent? After four cycles of policy, payoff, and poll, the election is held in earnest.

This set is just getting under way, so it is too early to tell if the intermediate polls, by allowing incumbents to test several policy variations, help the candidates converge faster. "In the real world," Ordeshook remarks, "voters are continuously monitoring their own welfare, and the candidates are continuously polling the electorate. It would be much more realistic to have analog computers, with people turning policy knobs and approval knobs continuously, and then have an election after some period of knob-turning. But we're stuck in our digital age."

McKelvey concludes, "You have to be very careful in trying to extrapolate to the real world. These are very simple experiments. But we feel the convergences we have seen demonstrate that it is *possible* for electoral systems to work properly over the long term, even when individuals have access to very little information."□—*DS*

Books

The Business of Science

Winning and Losing in the High-Tech Age

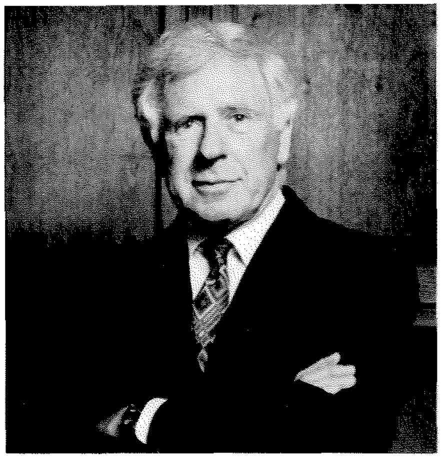
by Simon Ramo

Hill and Wang, 1988
$19.95
289 pages

Anyone who has had the pleasure of being in Si Ramo's company learns to expect his unique combination of wit and astute analysis. Those who haven't met him have a treat in store in reading *The Business of Science*. It is a hard book to categorize. The author informs us in the prologue that it is not an autobiography, but in the course of the book he tells a good deal about himself. It is about business, but a very special kind of business—that of high-technology military research and development, in which Dr. Ramo does not lack for experience. And he is prepared to apply this experience, without hesitation, to a host of new entrepreneurial endeavors. The book concerns technology much more than it concerns science, and it provides a realistic view of America's technological slip with respect to Japan and, to a lesser extent, to western Europe. Ramo is, nevertheless, optimistic about the possibilities for the utilization of our great scientific strength to regain primacy in technology and to contribute to a better life for all of humankind. There is an explicit blueprint for the next generation of scientifically trained entrepreneurs to follow. And, of course, the book offers a view behind the scenes of an incredible period in world history that the author played an important role in shaping.

Of his early childhood and youth Ramo tells us nothing beyond the fact that he spent his first twenty years (including his four undergraduate years at the University of Utah) in a community of gentle, warm people, and that he had competent, responsible, and caring teachers. One wishes he had told more about what it was like to grow up as an academically and musically precocious boy in Mormon Salt Lake City. He doesn't tell us how he made the choice of where to go to graduate school, but he had the great good fortune to select the California Institute of Technology, where he earned his PhD in physics and electrical engineering in 1936. From there he went to the General Electric research laboratory in Schenectady, and he alleges that he was hired as much for his talents as a violinist as for those he had demonstrated as a scientist. He left there after World War II to join Hughes Aircraft, having properly foreseen what would be a great growth in military technology related to aircraft and air defense. The description of Si and Dean Wooldridge's separation from Hughes to start Ramo-Wooldridge, and in particular Si's interaction with Howard Hughes, is one of the most fascinating parts of the book.

The new company played a singular role in the creation of the U.S. intercontinental ballistic missile force: it was given the task of system engineering and technical direction of the entire system—not a bad start for a fledgling company. Si gives much credit for the success of the program to Air Force General Bernard Schriever, who had overall responsibility, but this credit must be shared with the remarkable group of scientists and engineers who were attracted by Ramo and Wooldridge. It was almost inevitable that the company would play an important role in the



"Murph" Goldberger, former president of Caltech.

evolving NASA space program. Ramo expresses regret over the circumstances that prevented him from voicing his concerns about the manned space program and, at a later time but for similar reasons, the Space Shuttle. He is highly critical of the present lack of a coherent plan for space science and planetary exploration and the failure to provide an adequate expendable launcher capability to back up the Shuttle.

Si has long been involved in trying to rebuild the White House science advisory apparatus, which was destroyed by Mr. Nixon. To my surprise, he identifies Nelson Rockefeller as a principal ally in this effort. (In my own dealings with him, Rockefeller seemed to think Edward Teller was the only scientist worth talking to.) Si's efforts with Rockefeller and with then-President Ford resulted in Congress establishing the Office of Science and Technology Policy in the Executive Office of the President, thus protecting it from the wrath of some future president. Si was offered the position of science adviser to the president by Gerald Ford but declined; he felt that his long association with TRW would worry the bureaucracy and the Congress. Si continued his efforts to improve science advising at the start of the Carter administration and played an important role in the appointment of Frank Press. He struggled heroically with the incoming Reagan administration, trying to motivate them to increase the role of the OSTP and the science adviser so that someone of genuine stature could be attracted to the job. He failed, as we all know, and over the past eight years the influence of the science adviser has steadily declined.

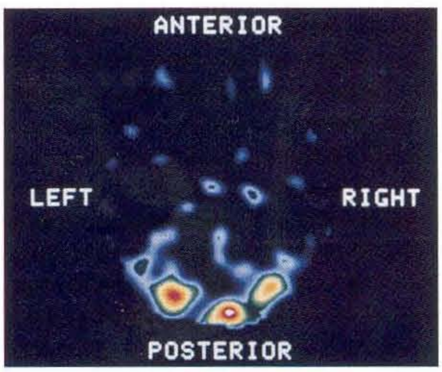
Nowhere was this more evident than in connection with Mr. Reagan's famous speech of March 23, 1983, which initiated the Strategic Defense Initiative, a concept that had received virtually no critical analysis by even the minuscule advisory apparatus then in existence. The science adviser and the principal technical people in the Pentagon had no input until the last moment. Si has spent a great deal of time trying to put some order into the ill-conceived and chaotic program that developed after the Reagan speech, a program that is hopelessly far away from demonstrating any promise of being able to achieve the president's dream of "rendering nuclear weapons impotent and obsolete." As might be expected, Si understands perfectly well and explains that the deployment of a ballistic missile defense makes no sense, given the current levels of strategic nuclear missiles, or in an environment where the offensive forces are not constrained by serious verifiable treaties. This portion of the book should be carefully read by both of the current presidential candidates.

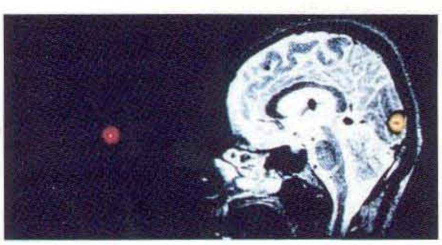
The candidates would also be well advised to study the analysis of the decline of U.S. leadership in technology, and of course, with Si Ramo speaking, there is a cure for the malady. This is Si at his best. Even though you may quarrel over some points, you cannot help being swept up by his imagination and creativity. Those of you who have had the privilege of knowing him can hear his voice in these pages. I have to confess that at a certain point I became very uneasy about the technical fix that was being offered for all the world's problems. But on the last page he says what was for me critically missing from what had preceded. I can do no better than to use his own words: "The business of science and technology is to discover the secrets of the universe and apply scientific and engineering skill to yield us security, prosperity, health, and happiness. Yet science and technology can never be more than tools. Poverty, disease, starvation, crime, overpopulation, ignorance, wars, and the impairment of the environment cannot be cured by science alone. That requires parallel social advance. The world's most serious unresolved issues are not science-technology ones; they are social, economic and political. . . . Whatever we ultimately are able to do to elevate the society will occur earlier and with greater success if our science-and-technology tools are many, sharp, versatile, and effective. Wise application of science and technology should offer a life that is steadily better as we progress, more slowly than we would like, toward one that is best." Well said, Si!

Marvin L. Goldberger
Director, Institute for Advanced S
Princeton, New Jersey

"Virtually any cognitive process can be studied by this technique."

Top: A typical horizontal PET slice. Red areas are most active. Allman is facing the top of the page. Middle: X-ray of Allman's head (facing left) in the MRI format, showing the planes of the 7 horizontal PET slices. Bottom: An unsubtracted PET scan, converted to sagittal format and superimposed on the MRI image.





Head Games

Every student knows the straightest path to success is to get inside your adviser's head, but few do it quite as directly as Bassem Mora. Mora, now a senior in biology (premed), took a peek inside Professor of Biology John M. Allman's brain as his SURF project last summer.

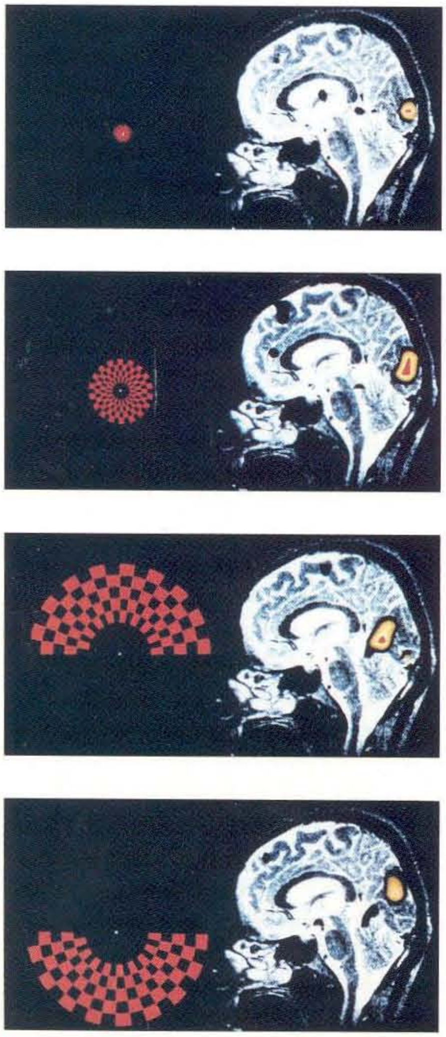
The brain's complex anatomy is a mirror for the complex functions it performs. Specific areas of the brain perform specific tasks. The visual cortex, which converts nerve impulses from the eyes into what we see in our mind, lies at the rear of the brain, in the occipital lobe. The brain has been mapped in broad outline, based on decades of individual medical histories—a tumor here, and the patient no longer recognized faces, but could identify people when they spoke; a lesion there, and the patient lost the use of the left hand. Electrodes have charted the ebb and flow of the brain's electricity. But, short of taking the top off someone's skull for a direct look, how can one relate a burst of electrical activity to a specific lump of tissue in a living person?

Mora developed a computer program that matches activity to anatomy. The program combines two images made by different techniques. Both techniques, Magnetic Resonance Imaging (MRI) and Positron Emission Tomography (PET), are standard diagnostic tools for noninvasive looks inside the body—in this case into Allman's brain.

MRI produces detailed three-dimensional anatomical images much in the same way that a CAT scan does. MRI, however, does not bombard the body with potentially harmful x-rays, but instead immerses the body in a magnetic field and records its response to radio waves. MRI scans are clearer and more detailed than CAT scans. Bones register very strongly in CAT scans, obscuring softer tissue, but bones do not generate strong MRI images—a critical consideration for brain imaging. The MRI scans were made by Dr. William G. Bradley, Jr., (BS '70, and a pioneer in MRI for medical uses) at the Huntington Medical Research Institute in Pasadena.

PET scans depict cellular activity but show few recognizable anatomical features. Active regions register as bright areas, while less active regions are proportionately dimmer. PET actually measures variations in blood flow. About 20 milliliters (roughly four teaspoons) of water labeled with oxygen-15, a short-lived tracer with a half-life of 123 seconds, is injected into the bloodstream. Greater blood flow to active regions brings more ^{15}O there, just in time for it to emit a positron and decay to ordinary nitrogen-15. The PET scans were made by Dr. Mark Raichle and his team at the Washington University Medical School in St. Louis, Missouri. "It takes a small army of people to make a PET scan," Allman remarked. "^{15}O decays so fast that they have to make it on the spot. So they make it in a cyclotron down in the basement, and then shoot it upstairs to the imaging lab through

Subtracted PET scans superimposed on the MRI scan. Allman is facing left, toward the corresponding visual stimulus. Note how the bottom half-checkerboard registers higher in the brain than the top half-checkerboard.



a pneumatic tube."

Since the brain is active even when the body is at rest, a single PET scan is a blurry, uninformative thing. But by making two of them, and subtracting one from the other in the computer, subtle changes become obvious. A background scan was made while Allman was lying quietly, eyes closed and wearing earplugs. Then Allman opened his eyes and looked at a series of flashing patterns on a video screen. Another scan was made for each pattern. (A special headrest keeps the head stationary, eliminating movement errors between scans. "It was a very comfortable couch, actually," Allman recalls. "They take great pains to make sure you're perfectly relaxed. Once you've settled into position, the whole series of scans takes two to three hours, and if you start to fidget, you ruin them.") When the background scan was subtracted from a pattern scan, only a small region of the visual cortex remained. Other researchers had done similar experiments, and had been able to show that various patterns stimulated different bits of the cortex, but without precise anatomical landmarks it was impossible to tell exactly what patch of gray matter had lit up.

Superimposing MRI and PET images is a bit more complicated than just aligning two pieces of film on top of each other and holding them up to a strong light. Each complete image is actually a series of slices through the brain, a fixed distance apart, taken simultaneously. The MRI scan has 10 slices taken sagittally (in vertical planes running front to back) spaced 2.7 millimeters (mm) apart, while the PET scan has 7 horizontal slices spaced at 14.4 mm intervals. Furthermore, each MRI slice consists of a matrix of 256 × 256 pixels ("picture elements"), while the PET slice contains 100 × 100 pixels. Each pixel in the MRI image represents a cube of tissue measuring 0.95 mm front to back, 0.95 mm top to bottom, and 2.7 mm left to right. Each PET pixel is 2.7 mm × 14.4 mm × 2.7 mm.

Mora's program had to transform 7 horizontal PET slices into 10 sagittal ones to match the MRI scans, taking into account the size differences between their pixels. A conventional x-ray, taken while Allman was still in the PET headrest, showed the exact orientation of his head during the scans, and provided anatomical details that Mora could match to the MRI scans. The MRI data arrived at Caltech on magnetic tapes in a format that Mora's computer couldn't read. No one knew how that format worked, so Mora spent several days figuring it out for himself. According to Allman, "It was a remarkable accomplishment. Most people—professionals included—would have thrown in the towel right there. There were other hurdles, too—although the idea was straightforward, this was not an easy project."

The resulting superpositions clearly show the specific lumps and strands of cortical tissue that responded to each stimulus. As more of the retina was stimulated, larger areas of cortical tissue responded. Furthermore, the areas were inverted—a stimulus in the lower half of the visual field lit up an area closer to the top of the skull than did the mirror image stimulus in the upper half of the field. (The latter phenomenon had been inferred from patients recovering from gunshot wounds—small-caliber bullets destroy a very narrow column of tissue along their immediate path, which can usually be determined with great accuracy.)

"Virtually any cognitive process could be studied by this technique," Allman says. "The St. Louis group is using it right now to study speech, and we expect a lot of other people to start using it." In the meantime, Mora is continuing with the project this summer with a variant of MRI that tracks the adenosine triphosphate (ATP) that fuels the brain's electrical activity. Besides being a more direct measure of brain function than PET, this strategy uses no radioactive tracers, and the whole thing can be done in one machine—perhaps even at one time.□—*DS*

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The first concrete of the Beckman Institute's foundations was ceremoniously poured on September 7. Caltech president Thomas E. Everhart lobbed a bottle of champagne into the hole, then the other participants tossed silver dollars into the wet concrete. Among those attending were (from left) Don Toy of A. C. Martin & Associates (the architects), Arnold and Mabel Beckman, and Everhart. The facility, due to open in 1989, will be devoted to interdisciplinary research in chemistry and biology.

Honors and Awards

George W. Housner, the Carl F Braun Professor of Engineering, Emeritus, received the National Medal of Science from President Reagan in a ceremony at the White House on July 15. Arnold O. Beckman, PhD '28 and a life trustee of Caltech's Board of Trustees, was honored with the National Medal of Technology at the same ceremony. The awards noted Housner's contributions to earthquake engineering and Beckman's to analytical instrument design.

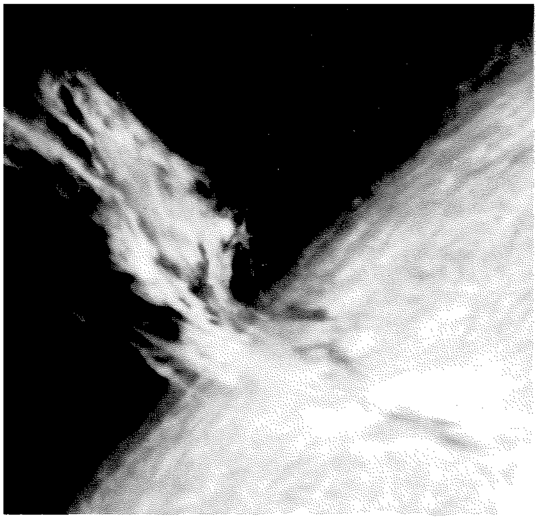
Assistant Professors of Biology Howard Lipshitz and Paul Sternberg have been named Searle Scholars by the Chicago Community Trust. The two are studying various aspects of gene activity in embryos.

Rudolph Marcus, the Arthur Amos Noyes Professor of Chemistry, was given the Peter Debye Award in Physical Chemistry at a symposium held in his honor in June. The American Chemical Society (ACS) presented the award as part of the Third Chemical Conference of North America, held in Toronto, Canada. At the same conference, Professor of Chemistry Robert Grubbs was given the ACS Award in Organometallic Chemistry, and William Goddard, the Charles and Mary Ferkel Professor of Chemistry and Applied Physics, received the ACS Award for Computers in Chemistry.

Gordon E. Moore, PhD '54 and a member of Caltech's Board of Trustees, has been given the 1988 Founders Award of the National Academy of Engineering for his role in developing large-scale integrated memory and the microprocessor.

The Office of Naval Research (ONR) has selected four Caltech faculty to be ONR Young Investigators for 1988. They are Assistant Professor of Chemical Engineering Frances Arnold, Associate Professor of Electrical Engineering John Doyle, Assistant Professor of Computational and Neural Systems Christof Koch, and Assistant Professor of Applied Physics Kerry Vahala. Only 15 investigators were chosen from a nationwide pool of 332 applicants.

The Associated Students of the California Institute of Technology (ASCIT) has honored six members of the Caltech faculty for their teaching excellence. They are Bruce Cain, professor of political science; Paul Patterson, professor of biology; Charles Peck, professor of physics; Thayer Scudder, professor of anthropology; Charles Seitz, professor of computer science; and Kerry Vahala, assistant professor of applied physics.



An 80,000-mile-high solar flare erupted at 1:37 PM PDT on July 25. The flare, 10 times the earth's diameter in length, was the largest in 4 years and lasted for nearly 2 hours. Solar flares are explosive releases of luminous gas, charged particles, and x-rays. Their effects on earth include the aurora borealis and disrupted radio communications. The photograph was taken at Caltech's Big Bear Solar Observatory by Harold Zirin, professor of astrophysics and director of the observatory.

What's Shaking?

Caltech plans to install a network of 10 high-tech, digital seismometers that will convert some 62,000 square miles of southern California into the world's largest scientific instrument. Dubbed the "Terrascope" by Don Anderson, professor of geophysics and director of the seismological laboratory, the network will stretch from San Luis Obispo to the Mexican border, and from the Channel Islands to the Nevada state line. The first unit has already been installed in Pasadena's San Rafael Hills. These seismometers will have a dynamic range 10,000 times that of conventional ones, allowing them to record big quakes without going off scale, while still being sensititve enough to pick up the 20 to 30 tiny temblors that jiggle California every day. The instruments will also be able to detect long-period vibrations outside the ken of ordinary ones, an advance Anderson likens to the onset of radio astronomy in the scope of new phenomena that are likely to be revealed. The units will be linked by satellite to high-speed computers on campus, and via the Global Positioning Satellites, will be able to track L. A.'s journey to Alaska. Network data can also be used to construct detailed three-dimensional pictures of the earth's interior ("Interesting Times in Geophysics" *E&S*, Spring 1988). The network will cost about $4.2 million.

Obituaries

W. Duncan Rannie (PhD '51), the Robert H. Goddard Professor of Jet Propulsion and professor of mechanical engineering, emeritus, died on August 13. Rannie first came to Caltech in 1938 to study under Theodore von Kármán. He joined Caltech's Jet Propulsion Laboratory in 1946 as chief of the Ramjet Section. He was appointed assistant professor of mechanical engineering in 1947, and became a full professor in 1955. He became emeritus in 1981. Rannie was known for his work in several branches of fluid mechanics, in particular the aerodynamics of turbomachines and heat exchangers. Rannie was 74.

William R. Smythe, professor of physics, emeritus, died July 6 at age 95. Smythe came to Caltech as a research fellow in 1923. He was named professor of physics in 1940 and became emeritus in 1964. He was the Head of the Special Ballistics Section of the Caltech Rocket Project from 1942 to 1945, where he developed a solar yaw camera to stabilize spinning rockets in flight. Smythe invented a method for separating isotopes of an element electromagnetically, and also solved various problems in eddy currents and electromagnetic theory.

David F. Welch, professor of engineering design, emeritus, died on July 2. Welch was 70. He worked for several large industrial firms before earning a professional degree in industrial design from Caltech in 1943. He joined the faculty as an instructor in industrial design and engineering drafting in 1947. By 1961, he was a full professor of engineering design, and became professor emeritus in 1987. During 1964-65, Welch went to Kanpur, India, with six other Caltech faculty and staff to help develop the curriculum for the Indian Institute of Technology campus there.

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