California Institute of Technology

E&S
Engineering and Science

Volume LXV,
Number 2,
2002

IN THIS ISSUE

The shadows cast by this year's graduating class may some day equal that of the multifaceted Richard Feynman, who was portrayed on stage by this year's commencement speaker, Alan Alda. Just as Feynman never made it to Tuva, Alda never met the late Nobel laureate; but in both cases the quest itself became a great adventure.

Alda's speech begins on page 17.

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ENGINEERING AND SCIENCE

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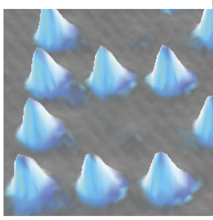

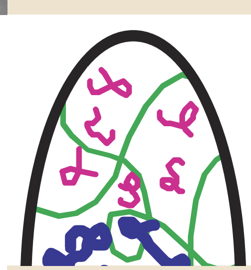

On the cover: Cows laze under the watchful eye of this Orma herder in northern Kenya. On page 6, Jean Ensminger relates how anthropologists have taken experimental economics out of the university laboratory and into different societies around the world, and what the results can tell us about why some economies function better than others.

Engineering & Science (ISSN 0013-7812) is published quarterly at the California Institute of Technology, 1200 East California Boulevard, Pasadena, CA 91125. Annual subscription $10.00 domestic, $20.00 foreign air mail; single copies $3.00. Send subscriptions to Caltech 1-71, Pasadena, CA 91125. Third class postage paid at Pasadena, CA. All rights reserved. Reproduction of material contained herein forbidden without authorization. © 2002, California Institute of Technology. Published by Caltech and the Alumni Association. Telephone: 626-395-3630.

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The Sturtevant Memorial Spa was formally dedicated in a short ceremony on May 2. The spa was built in memory of the late Bradford Sturtevant (MS '56, PhD '60), the Hans Liepmann Professor of Aeronautics, who died in October 2000. A legendary swimmer, Sturtevant was long active on the faculty athletic committee and was a key figure in the planning and construction of the Braun Athletic Center. His widow, Carol, thanked all who had contributed to the unique memorial, which will "enhance the Caltech experience for students and the Caltech family."

"It will offer respite from the academic pressures of Caltech," said athletic director Tim Downes. "Brad Sturtevant will always have a spot on this pool deck."

As the guests of honor at Dodger Stadium on Saturday, June 1, some 1,600 members of the Caltech/JPL community watched the Battlin' Beavers' battery of pitcher Isaac Gremmer (freshman) and catcher Eric Peters (sophomore, chemistry) handle the ceremonial first pitch. From left: Gremmer; Peters; and honorary coaches David Baltimore, president of Caltech; and Charles Elachi (MS '69, PhD '71), director of JPL. The Dodgers beat the Arizona Diamondbacks, 2–0.

What Bleach Can Teach About Ozone Leach

Results from a JPL/Caltech collaboration have unraveled a mystery that may permit better global measurements of some ozone-depleting gases. Scientists have long known that the HO_x radicals—hydroxyl (OH) and hydroperoxyl (HO_2)—destroy ozone in Earth's stratosphere, allowing more ultraviolet radiation to reach Earth's surface. The HO_x radicals can't be measured easily, but the hydrogen peroxide (H_2O_2) produced when they react with each other can be.

So atmospheric scientists would like to use peroxide as a proxy to map HO_x distributions, but there has always been a large, nagging discrepancy between the actual peroxide measurements and the levels predicted by global computer models of the atmosphere, suggesting that the chemistry has not been completely understood. The new study has resolved much of the disparity.

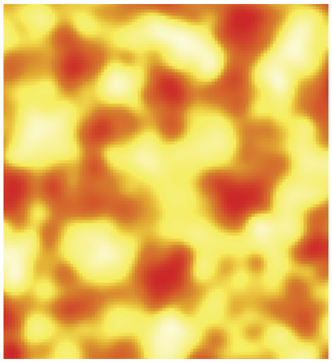
In the May 7 issue of *Geophysical Research Letters*, the scientists report discovering an error in the calculations for the rate at which two hydroperoxyl radicals form hydrogen peroxide, which were thought to be well known. Lance Christensen, a Caltech grad student in chemistry working at JPL and the paper's lead author, showed that at the low temperatures relevant to the stratosphere, the rate is slower than had been previously measured. In lab studies, hydroperoxyl radicals are typically formed from methanol, but Christensen discovered that the trace amounts of methanol present accelerated the rate of hydrogen peroxide formation.

"The importance is not so much the hydrogen peroxide itself, but that it opens the possibility for remotely measuring hydrogen peroxide to infer the HO_x radicals" from space or the ground, says Mitchio Okumura, an associate professor of chemistry and a coauthor of the study. "The HO_x radicals are central to the chemistry of the stratosphere and upper troposphere in understanding ozone depletion."

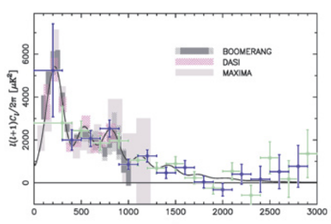
"We're trying to improve our understanding of the atmosphere well enough to be able to model ozone depletion and climate change in general," says JPL's Stan Sander (MS '75, PhD '80), another coauthor. "This work provides an important tool."

In addition to Okumura, Sander, and Christensen, the other authors are Ross Salawitch, Geoffrey Toon, Bhaswar Sen, and Jean-Francois Blavier, all of JPL; and K.W. Jucks of the Harvard-Smithsonian Center for Astrophysics. The study was funded by NASA. □—*RT*

An Ultrasound of the Infant Universe



In this view from the Cosmic Background Imager, the hotter, denser regions are the "seeds" from which galaxy clusters will eventually grow. The image covers a 2° by 2° field—an area about 16 times that of the full moon—and shows details about 1 percent the size of the moon.



The fluctuations in the microwave background can be sorted by area the way the graphic equalizer on your stereo sorts sound waves by frequency. Seen this way, the CBI data (green and blue crosses) extend to much higher "frequencies" than the other experiments. The solid line shows the predicted size distribution; the CBI has verified "overtones" beyond the other instruments' hearing.

Caltech's Cosmic Background Imager (CBI), a radio telescope set high in the Chilean Andes, has uncovered the finest detail seen so far in the cosmic microwave background radiation, which originates from the era just 300,000 years after the Big Bang. The new images are essentially photographs of the infant universe from before stars and galaxies existed, and reveal, for the first time, the seeds from which clusters of galaxies grew.

The cosmic microwave background radiation was emitted some 14 billion years ago, when matter first got cool enough for electrons and protons to combine and form atoms. Minuscule fluctuations in the universe's density at that point imprinted themselves on the radiation as subtle temperature differences—about one part in 100,000. The CBI makes fine-detailed, high-precision pictures of these temperature differences in order to measure the geometry of space-time and other fundamental cosmological quantities (*E&S*, 1996, No. 4). Tony Readhead, the Rawn Professor of Astronomy, is the project's principal investigator.

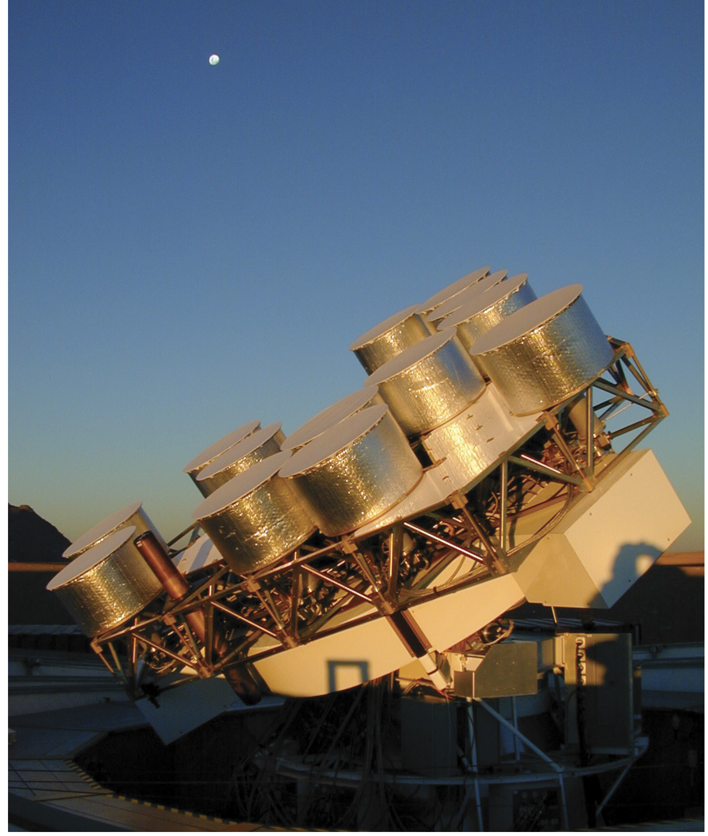
Because it sees finer details, the CBI goes beyond the recent successes of the BOOMERANG and MAXIMA balloon-borne experiments and the DASI interferometer experiment at the South Pole. The BOOMERANG experiment, led by Andrew Lange, the Goldberger Professor of Physics, demonstrated two years ago that the universe is "flat" (*E&S*, 2000, No. 3). The CBI results verify this, and confirm that most of the matter in the universe is exotic "dark matter" and that "dark energy" plays an important role in the evolution of the universe. The flat universe and the existence of dark energy lend additional empirical credence to the so-called inflation theory, which states that the universe grew from a tiny subatomic region during a period of violent expansion a split second after the Big Bang.

The CBI and BOOMERANG observations, combined with the MAXIMA and DASI data, cover a range of angular scales from about one-tenth of a moon diameter to about one hundred moon diameters. Each instrument uses different methods and different frequencies and looks at a different part of the sky, yet all agree, giving great confidence in the combined results.

The CBI is an array of 13 separate antennas, operated in concert so that the entire machine acts as an interferometer. Sited on the Llano de Chajnantor, a 16,700-foot plateau, it is by far the most sophisticated scientific instrument ever used at such an altitude. The telescope is so high, in fact, that team members must carry bottled oxygen. The CBI hardware was designed primarily by Stephen Padin, the chief scientist, assisted by senior mechanical engineer Walter Schaal (BS '58) and research engineer John Yamasaki. The software was designed and implemented by senior research associate Timothy Pearson and staff scientist Martin Shepherd. Postdoc Brian Mason and grad students John Cartwright, Jonathan Sievers, and Patricia Udomprasert also played critical roles. The telescope was built on campus and hauled from Pasadena to the Andes in August, 1999.

In five separate papers submitted to the *Astrophysical Journal*, Readhead and his Caltech colleagues, together with collaborators from the Canadian Institute for Theoretical Astrophysics, the National Radio Astronomy Observatory, the University of Chicago, the Universidad de Chile, the University of Alberta, the University of California at Berkeley, and the Marshall Space Flight Center, report on observations collected since the CBI began operation in January 2000. The images cover three patches of sky, each about 70 times the size of the moon.

The CBI team will next look for polarization in the cosmic microwave background's photons as part of a two-pronged attack with DASI. An upgrade to the CBI required for the polarization measurements was generously underwritten by the Kavli Institute. □—*RT*



The CBI at twilight. Each metal canister contains a 90-centimeter dish antenna. The array maps a patch of sky slightly larger than the moon every night.

Sorting Signals for Storage

Quick! Memorize this sentence: The temporoammonic (TA) pathway is a entorhinal cortex (EC) input that consists of axons from layer III EC neurons that make synaptic contacts on the distal dendrites of CA1 neurons. If by chance you can't, say grad student Miguel Remondes and Erin Schuman, associate professor of biology, it may be due to this very TA pathway.

In another clue toward understanding how memories form, Remondes and Schuman (who is also an assistant investigator of the Howard Hughes Medical Institute), have found that this pathway may be part of the brain's decision-making process about whether to save a particular input. The research was reported in the April 18 issue of *Nature*.

Input from the senses—an odor, say—follows a well-known path. The signals are first received by the brain's cortex. From there, they are sent to the dentate gyrus, and then to the hippocampus, both of which are known to be involved in saving and retrieving long-term memories. Scientists divide the seahorse-shaped hippocampus into four regions, named CA1 to CA4. The signals are processed first in CA3 and then in CA1 before the hippocampus sends its output back to the cortex, probably for long-term storage. This pathway is called the trisynaptic circuit.

Scientists had also mapped the TA pathway, but did not know its function. Remondes and Schuman report that it may serve as a gatekeeper that enhances or diminishes the signals of each specific set of neurons that attempts to form a memory. Further, this pathway may also provide the hippocampus with the information it needs to form so-called place-selective cells; that is, cells that help animals to know where they are in their environments.

The TA pathway's input comes from a different part of the cortex, and goes directly to CA1. Remondes and Schuman found that the TA pathway's effect depends on the time lag between when the hippocampus receives the inputs and when it sends its own signals back to the cortex. If the timing is close, within 40 milliseconds, the TA pathway acts as a signal (and memory) enhancer; that is, it stimulates stronger signals from the hippocampus. If the delay is more than 400 milliseconds, it inhibits the signals.

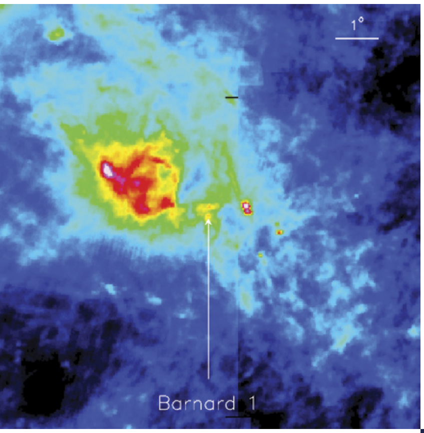
"So the brain sends the information to the hippocampus," says Remondes, "and instead of just collecting the results, the hippocampus may very well perform 'quality control' on the potential memory. And it may be doing this by using the direct cortical input from the TA pathway."

Although the scientists have not done any specific spatial-memory experiments, the work may also shed light on how the brain forms place-selective cells. Since other studies have established that the trisynaptic circuit is not needed for spatial memory, some of the information entering the hippocampus may actually be provided by the TA pathway.

"The TA pathway has been briefly described in the past, but not really acknowledged as a 'player' in the memory debate," says Remondes. "Hopefully, these findings will bring new insight into how we form, or don't form, memories." □—*MW*

CSO Wins the Lottery

This IRAS image (below) shows the galactic neighborhood of Barnard 1, the region of the Milky Way where the Caltech Submillimeter Observatory (below, right) discovered triply deuterated ammonia.



A rare type of ammonia containing three atoms of deuterium has been found in a molecular cloud about 1,000 light-years away, in the direction of the constellation Perseus. The comparative ease with which the molecules were detected means that there are more of them than previously thought. The observations were done by an international team of astronomers using the Caltech Submillimeter Observatory atop Mauna Kea in Hawaii, and were reported in the May 20 issue of the *Astrophysical Journal Letters*.

Deuterium, or "heavy hydrogen," has a neutron in its nucleus in addition to the single proton that ordinary hydrogen has. Ammonia contains one nitrogen and three hydrogen atoms per molecule.

Triply deuterated ammonia was thought to be so rare in deep space as to be undetectable from Earth, says Professor of Physics Tom Phillips, director of the Caltech Submillimeter Observatory and leader of the Caltech team. No other molecules containing three deuterium atoms have ever been found in interstellar space. "From simple statistics alone, the chances for all three hydrogen atoms in an ammonia molecule to be replaced by the very rare deuterium atoms are one in a million billion," Phillips explains. "This is like buying a $1 state lottery ticket two weeks in a row and winning a $30 million jackpot both weeks. Astronomical odds indeed!"

Both hydrogen and deuterium are present in the interstellar medium, says Dariusz Lis, a senior research associate in physics and lead author of the paper, and at higher temperatures they freely trade places with their counterparts in the ammonia molecules. But at the frosty 10 to 20 degrees above absolute zero that prevails in the clouds, the deuterium atoms prefer to settle into the ammonia molecules and stay there.

The study furthers our understanding of the chemistry of the cold, dense interstellar medium and the way that molecules transfer from dust grains to the gas phase, Phillips explains. The researchers think the triply deuterated ammonia was returned to the gas state, and thus rendered observable, when it was kicked off dust grains by energy from a young star forming nearby.

The Caltech Submillimeter Observatory, funded by the National Science Foundation, has the world's most sensitive submillimeter detectors, making it ideal for seeking out the diffused gases and molecules crucial to understanding star formation. The observing team also included members from France's Observatoire de Paris and the Max-Planck-Institut für Radio-astronomie in Germany. □—*RT*

Are people in some societies inherently more cooperative? . . . Does the lack of development in many countries have to do with the belief systems in peoples' heads? . . . Our experimental data speak directly to this question and yield some rather surprising results.

Left: In a grass hut in an Orma village in Kenya, the games master, right, recaps the rules of the Ultimatum game to the player on the left, who's been given 100 Kenyan shillings (about $2) and now has to decide how much of that he's prepared to offer to another, anonymous player. If his offer is accepted, he gets to keep what's left, but if it's refused, he'll lose everything. This game was played in 16 societies around the world, including Hamilton, Missouri, above, and Papua New Guinea, right.

From Nomads in Kenya to Small-Town America:

Experimental Economics in the Bush

by Jean Ensminger

Why are some countries rich and others poor? It's something that's still not well understood. We know there's a relationship between economic performance and the way a country is governed, and we know it has something to do with the way governments enforce the law and set incentives for production and exchange, but we also think that informal social institutions at the local level play a role. And this gets us into the fuzzy domain of "social capital": Are people in some societies inherently more cooperative? Do they have richer social networks that oil the wheels of trade? Are they more trusting of one another? Do notions of what constitutes fair dealing and sharing with others vary? If so, how and why? Presumably all of these characteristics have a bearing on economic exchange, but it's very difficult to measure such things precisely across different societies. I've been interested in these issues for some time, but it was only in the last couple of years that I began using a methodology that I think has tremendous promise for getting at them in a much more rigorous way. So when I was given the opportunity to join a project applying experimental economic methods to these social characteristics in a lot of different cultures around the world, I happily agreed to participate.

PICTURE CREDITS:
6, 10, 11, 16 – Jean Ensminger; 6, 13 – Kathleen Cook; 7 – David Tracer; 13 – St. Louis Convention and Visitors Commission

For a cultural anthropologist like me, this is a rather unusual form of research, and not just because it's about economics. Most of us are engaged in some variety of descriptive case study, and much of our research is qualitative, whereas the research I'm talking about here is quantitative, and even experimental. It's not what anthropologists usually do. But let me add, it's also unusual from an economic standpoint, because the way I do experiments is not the way they're usually done at Caltech, a world center for experimental economics. The experiments are pitched as games, because they usually involve some sort of bargaining situation between the individuals taking part. When Caltech economists do experiments, they often do them in laboratories down in the basement of Baxter Hall with undergraduates. But Caltech undergraduates are not your average Americans—at least we certainly hope not! I'm interested in the economic behavior of *average* people, people of all age ranges and all socioeconomic brackets, so I want to play economic games with a more representative sample of the population. I want to use these games to study the norms of altruism, trust, and cooperation of people

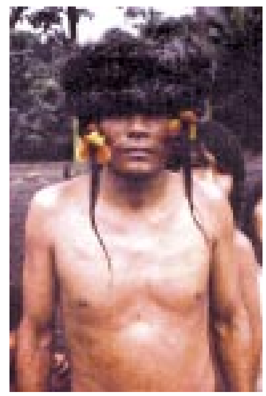
in places like New Guinea, the Amazonian rain forest, Kenya, and rural and urban Missouri. Among other things, studying a diversity of societies, from isolated family groups to complex urban communities, may shed light on the evolution of social norms. As anthropologists, we have often lived in a society for years, and have gotten to know people well—their family relationships, traditions and beliefs, relative prosperity, social standing, and much more.

There were several motivations for this project. We already knew from laboratory experiments in the United States that subjects often did not behave according to narrow economic assumptions of self-interest. Instead of playing in a totally selfish way, players often offered their partners substantial portions of the pot. A couple of years ago, a young anthropologist named Joseph Henrich decided to find out if the people in the Amazon behaved in the same way as American university students when they played these games, and lo and behold, they didn't. So the MacArthur Foundation sent people out to look at a variety of small-scale, close-knit societies around the world to see if the results from the Amazon would be replicated in other places, or if they were just a fluke. Anthropologists who already had considerable field experience were asked to return to their field sites to play some simple economic games for real money. We looked at 16 societies in all: one in New Guinea, one in Indonesia, one in Mongolia, five in Latin America, six in Africa, and two in the United States. They included hunter-gatherers, slash-and-burn subsistence farmers, nomadic herders, cash-crop farmers, and wage workers in an industrial society. I'm going to tell you about three of the games that we played, and what insights the results have given us about the relationship between social norms and economic development.

There's a debate raging among social scientists right now about the role of culture in economic development. Does the lack of development in many countries have to do with the belief systems in peoples' heads? Do some cultural beliefs constrain development? For example, many small-scale societies, such as those of hunter-gatherers and subsistence farmers, require food to be shared among family and close neighbors. Some scholars maintain that this "taxation" is a disincentive to production—why work harder if you have to share your profits with the lazy ones?—and that this could explain why such economies don't develop. Our experimental data speak directly to this question and yield some rather surprising results that support an entirely different perspective.

A second debate concerns the role of trust in the economy. Most scholars agree that trust is important for economic growth. Without trust there would be no credit cards and no checking accounts; can you imagine if every transaction had to be a face-to-face meeting where each person had

The Machiguenga live in isolated family groups along rivers in the Peruvian rain forest as slash-and-burn farmers, growing crops like manioc, bananas, and maize, supplemented by hunting and fishing. When Joseph Henrich took the Ultimatum game to them, they played more as game theory predicts—making very low offers and rejecting none—than university students did. This surprising result inspired the present study.



Two subsistence-farming societies of the Ecuadorian Amazon studied by John Patton often intermarry, but play the Ultimatum game differently. The Achuar (near right) have one of the highest murder rates in the world—the men kill each other for status—but were more generous players than their less murderous Quichua neighbors (far right).

Francisco Gil-White studied nomadic herders of cattle, sheep, and goats on the steppes of Mongolia (shearing goats, above, and racing horses, below). Neighboring Mongols and Kazakhs have deep cultural and historical differences, but were more generous when they played the Ultimatum game against each other than against their own people, perhaps due to higher fear that their offers would be rejected.

Like many other Amazonian subsistence farmers, the Tsimané of eastern Bolivia grow plantains, maize, rice, sweet potatoes, and papayas—and sweet manioc for beer—in small gardens cleared out of the forest, supplemented by hunting, fishing, and foraging. Households of related families often pool food, but there's little cooperation with unrelated family groups except for occasional home-brew parties and group fishing expeditions. Introduced to the Ultimatum game by Michael Gurven, they made low offers and rejected none.

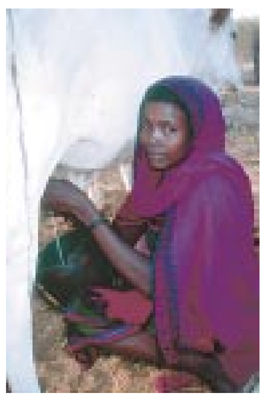
one hand on the goods and one hand on the money? In the United States, we have very strong institutions of legal enforcement, but there's still an awful lot of business transacted without every single contingency being written into the contract. We have certain norms of acceptable behavior, and trust is a lubricant that lets a lot of economic behavior move along. A number of scholars argue that trust is a "cultural primitive"—some cultures are very trusting, and some are not—and a culture that is very trusting is likely to do better economically. The natural conclusion of this line of thinking is that if you happen to reside in an untrusting culture, you are out of luck when it comes to economic development. The data from this project allow us to explore alternative explanations.

The Au of Papua New Guinea grow taro and other crops in small clearings, keep pigs, and forage and hunt in the forest. They place a premium on generosity. In this society, accepting a gift puts the recipient under obligation to the giver, which may explain why game players often made illogically generous offers, which were often (again, illogically) refused. A mother (above) is being taught the Ultimatum game by David Tracer. A girl (left) is carrying her kid brother.

One of the greatest challenges of any cross-cultural experimental project is to keep the controls tight, so that all the results are really comparable. Given the vast diversity of the societies in which we are working, it's impossible to achieve this perfectly. We put considerable effort into thinking through this problem before we headed out to our respective sites, but in many respects we fell short of the level of controlled experimental design we would have liked. Though most of us had considerable field experience in our areas, we truthfully had no idea if this was even going to work, much less any idea about the specific nature of the logistical complications that we would each encounter. Needless to say, in this first phase of the project we all had to do some creative improvising on the spot. We have learned a great deal from this first round, however, that will allow us to tighten up the controls considerably in the next phase of the project now under way, funded by the National Science Foundation and the Russell Sage Foundation.

We did make efforts to control a number of obvious issues across all the sites. We all set out with the same game scripts to be translated into the local dialect. No deception was used in the games—we were careful to do exactly what we told people we were going to do. All games were played for real money, and the stakes in the games were controlled across sites to be equal to one day's minimum wage in the local currency (thus, 100 shillings, or $2, in Kenya; $50 in rural Missouri; and $100 in urban Missouri)—a fairly substantial sum in each location. All of the games were also played as one-shot games, and players did not play more than one game. The games were anonymous, in that no players ever knew the exact partner with whom they were paired, although people knew they were playing with fellow members of their community. It was essential to replicate this characteristic in the United States as well, where we used a rural community with a total population not that much greater than some of our developing societies; in urban Missouri we used coworkers.

There is a reason that we played games for real money. You can ask people hypothetically, "Imagine that I gave you $100, how much of it would you share with an anonymous partner from your community?" You may or may not get an answer that corresponds with real behavior. Instead, put $100 on the table and say, "Here's $100, now tell me how you want to split that with an anonymous partner. You get to take home whatever you don't offer to the other player." Playing with real money is a much better measure of people's real behavior than asking a hypothetical question.



Jean Ensminger had to do quite a bit of driving across the grasslands of northern Kenya to take the games to the nomadic Orma herders, who move their cattle from pasture to pasture (above), far from towns, markets, and shops. They take their few belongings with them (below), and live off their cattle, especially the milk (right).

My first experimental subjects were the Orma, whose economy and society I've been studying for nearly 25 years. They live in northern Kenya near the Somali border, and are traditionally nomadic, living off large herds of cattle, although many have now adopted a settled lifestyle. Orma territory is still largely inaccessible and undeveloped. Almost everyone lives in a grass house, and there is no running water, no electricity, and few possessions other than clothing and cooking pots. Nevertheless, there is surprising differentiation among the population in terms of their degree of involvement in the market economy. The nomads tend to have a more subsistence-based lifestyle, their diet consisting mainly of milk and other cattle byproducts—and are a long way from towns and trade. The settled populations, on the other hand, send their sons, and occasionally daughters, to primary school for a few years. Though almost all are still tied to the cattle economy, many do wage work, others are tradesmen, and some grow a few food crops in rainy years. All are strongly tied to the market economy.

When I first started planning these games, I thought there might be resistance, but that wasn't the case—the Orma loved them, and wanted me to come back soon to play more. Many found it both fun and intellectually amusing, along the lines of "I'll be spending years trying to figure out what this all meant." Many of my concerns about logistics were also ill-founded. The grass houses, which I thought would be too permeable to keep the proceedings inside away from prying eyes and ears, turned out to be the perfect size for isolating the player from the rest of the group who were waiting their turn outside. And when I explained they could not talk about the game during play, they complied with remarkable discipline.

Before beginning the experiments, I held a large public meeting to explain that I would be playing fun games with real money, and that these games were going to be played all around the world. This led to a lot of amusement at the "insanity" of

Western ways, and how Westerners "had money to throw away on such foolishness." I might add that the reactions were not much different in Missouri!

Every household in each of five villages was asked to take part voluntarily in a detailed household demographic and economic survey, and I promised to invite at least one adult from each household to play a game. In addition to the money they might win for the games, each player was paid a show-up fee of one-third of a day's wages at the start of the games, to make them appreciate they were playing with real money, and to compensate those who would not win much in the games.

A wedding house under construction (left), a nomad's house (center), and the more substantial house of a settled family (right).

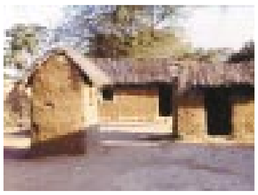
The first game I will discuss is the Ultimatum game. Here's how it worked. Approximately 20 people from a village were gathered together. All of them then learned the rules of the game. One by one, they were called to play the game, at which point I told them what their role was: I randomly assigned half to the role of Player One and half to Player Two. Each Player One was given a day's casual labor wage (about $2 in Kenya), then had to decide how to divide that money between himself or herself and an anonymous partner, Player Two. Each Player Two was informed how much he or she had been offered by Player One, and could either accept or refuse. But if Player Two refused, *neither* player got anything. So those assigned to the role of Player One, if they wanted to be greedy and keep as much of the money for themselves as they could, had to decide how low an offer they could make that would not be refused. They were told on a later occasion whether their offer had been accepted (in which case they got some money) or refused (in which case they got nothing), but neither player ever knew whom they had played against.

The game theoretic prediction based on the standard assumption that people act strictly in their own narrow economic self-interest is that Player One should offer the smallest amount possible, because it would be completely irrational for Player Two to refuse even a penny. After all, Player Two still comes away a penny richer, and has nothing to gain by refusing the offer. But in the United States, real people (well, students) don't play the game that way, and they don't play the game that way elsewhere in the world, either.

The way the Orma played didn't depend on gender, age, education, or wealth of household—the only variable that predicted the result was whether or not they were involved in the market economy. This variable is closely correlated with whether they were involved in wage labor or trade. The people involved in the market economy made

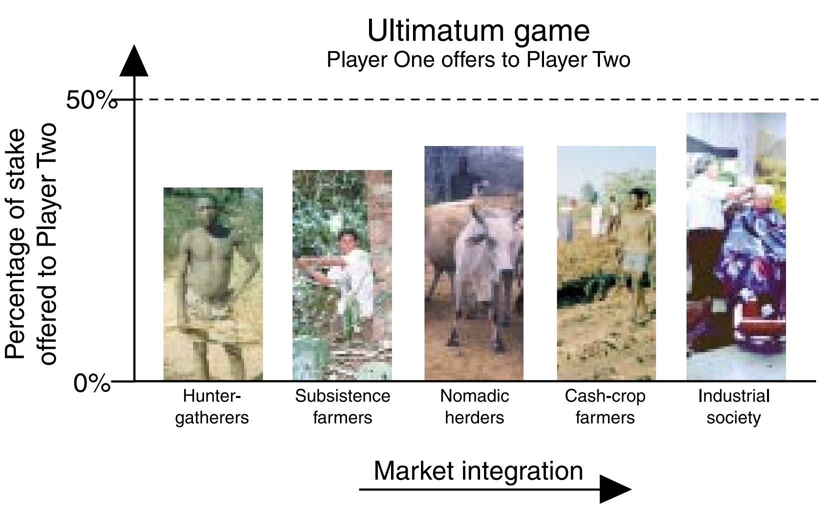
Settled Orma live in villages or towns with rows of shops like these on the right. Inside (far right), they're stocked with the basics of a settled life: clothing and cooking gear.

The Sangu of Tanzania were studied by Richard McElreath. Some are settled farmers (growing mainly maize), while others are nomadic cattle herders, much like the Orma. They are at the same level of market integration, and the herders and farmers made the same mean offers in the Ultimatum game.



more generous offers, with 80 percent of the players offering 50 percent of their stake to Player Two. The nonmarket people were evenly split between 30, 40, and 50 percent offers. There appeared to be no norm among the nonmarket people, but clearly a very prominent norm for the people involved in the market. Interestingly, no one made an offer below 30 percent. Were they afraid that a low offer would be refused? Perhaps they were, although out of 13 players who received 30 percent offers, only two refused. In this game we cannot disentangle high offers that are strategic from those that are motivated by fairness.

What happens when we look at how other parts of the world played? To date we have comparable data on the Ultimatum game for all 16 societies. We also have a ranking of those societies by their degree of market integration, from a subsistence-oriented, nonmarket economy of pure hunters and gatherers at one end, to an industrialized but close-knit community in rural Missouri at the other. The results are counterintuitive, but they're consistent with what we found within the Orma: the lower the level of market integration, the less generous the offers. The most market-savvy society, rural Missouri, was also very generous, with players offering an average of 48 percent of their stake. While the results for those 16 societies are statistically highly significant, and higher market integration correlates with higher offers, some individual subsistence-farming societies diverged considerably from this pattern. It remains to be seen whether these exceptions are the result of differences in the way the games were played across sites, or whether they represent actual cultural differences. We hope Phase II of our project will shed light on this. But interestingly, those differences evaporate when we lump the societies by economic subsistence strategy, as in the graph on the left, and we see that again there is an increase in offer size from low to high market integration.



As noted above, one of the drawbacks of the Ultimatum game is that we can't separate strategy from fair-mindedness. Are people really being fair-minded, or are they just making a high offer because they think they're going to get rejected if they don't? Well, fortunately we have another game, an even simpler one that allows us to isolate fair-minded behavior, called the Dictator game. In this game, I again endow Player One with a day's wages, which can be split any way the player likes with his or her partner, who remains anonymous—and that's it. Player Two doesn't have to decide whether to accept or reject the offer—what Player Two is given by Player One is what Player Two takes home. This is the purest measure we have of altruistic behavior. Player One doesn't have to worry about being rejected and ending up with nothing at all, so any offer above a penny is sheer generosity. When we looked at how the

In Missouri, the good folks of Hamilton (the two photos on the far right) and St. Louis (near right), studied by Jean Ensminger and Kathleen Cook, were fair, trustworthy, and trusting in the way they played the games.

Orma played we found, not surprisingly, that the offers went down for both the nonmarket and the market people, although the market people were still considerably more generous than the nonmarket. Now only 50 rather than 80 percent of the market people were dividing their stake 50/50, and the rest were simply all over the map. We still have no particular pattern among the nonmarket Orma, though there was a nice spike at the 20 percent offer, meaning that many of them chose to keep 80 percent.

I don't have the results of this game for all 16 societies, but I do have it for two others, the Hadza (a society of pure hunter-gatherers from Tanzania) and the rural Missourians. The results relative to market involvement look even more striking than they do for the Ultimatum game. The Hadza (at the lowest end of the market scale) kept 80 percent of their money and gave 20 percent, the Orma kept 66 percent and gave 34 percent, and the people of Missouri gave 48 percent, close to 50/50, even in the Dictator game where there was no worry about being rejected. Once again we find that market integration correlates with higher offers.

One of the last true hunter-gatherer societies, the Hadza, studied by Frank Marlowe, live in groups of 20–30 people in the Tanzanian savannah-woodland. Food, especially big game, has to be shared with the whole camp, otherwise the selfish person is gossiped about. Therefore it is handed over, unless it can be snuck into the family shelter under cover of darkness. The constant sharing and lack of privacy must get to them, because they were the least generous of all the Ultimatum game players: offers were low, and both high and low offers were often rejected.

We also played an interesting game designed to measure trust—that very elusive quality so important to economic development. In the Trust game, *both* players are given the same amount, let's say $40 in the rural United States. As before, Player One can give any percentage of the $40 he or she wants to Player Two. Whatever Player One doesn't send over, Player One keeps. But whatever Player One sends to Player Two will be tripled by me, and then Player Two has the option of sending something back. There's no confounding with fair-mindedness here—Player Two already has his or her stake, so Player One has no obligation to give anything to Player Two. So now the dilemma for Player One is that the more money he or she sends, the more money there is in the game, due to the tripling, but Player Two is the sole determiner of how that money is divided. If Player One trusts Player Two and sends all of the money over, and if Player Two is trustworthy and returns two-thirds of the tripling, they can both double their initial stake. But does Player One trust Player Two to do that? And how do the behaviors of both players vary cross-culturally? We've only played this game in a few societies so far, but the results indicate that in the small-scale societies of the developing world, there is less trust, and in the United States there is more (in both rural and urban Missouri).

What if Player One is trusting, but his trust is not repaid—he gets taken for a ride by Player Two? The amount Player Two gives back is a measure of his trustworthiness. As it turns out, trust and trustworthiness are highly correlated within societies, as we would expect them to be. The most trusting players, those from the United States, have their trust repaid. Thus in the United States, where trust is high, people (Players Two) are also very trustworthy. The Orma are the least trusting, and also the least trustworthy.

Could these findings correlate with the quality of a country's institutions? Could it be that if a government has very strong, effective institutions (such as well-enforced rules of law, and clear-cut

The Mapuche of southern Chile run small commercial farms and, like many small-scale farmers, distrust neighbors and don't welcome uninvited visitors. They believe that illness and bad luck are caused by the spite and envy of others. In the Ultimatum game run by Joseph Henrich, offers were a fairly good 34 percent—perhaps less out of fair-mindedness than fear of being rejected by a spiteful responder.

property rights), it pays people to be trustworthy, because they're unlikely to get away with cheating, or reneging on contracts? I was able to test this hypothesis using data provided by an organization called Transparency International, which compiles a corruption index that ranks countries by the quality of their institutions. Much of Africa is at one end, and the United States is pretty high up at the other end. (Though it's not the most uncorrupt country in the world. The Scandinavian countries have that honor, and soon I'm going to see how the people there play the games.) When I plotted the results of the Trust game for Player One offers for the people of three nations—Kenyan herders (the Orma), Zimbabwean cash-crop farmers (the Shona), and Americans in rural and urban Missouri—against the corruption index, they were dead on the line, that is, there was nearly a perfect correlation between higher trust and lower corruption. But so far we have only three cases, and these results need to be replicated at many other sites.

So what can we make of the results from these three games? Can they guide us toward formulating policies to promote economic development? As we've just seen, the trust data could have to do with the quality of a country's institutions. When there is little corruption—that is, the police and the courts are not bribable—it often does not pay to break contracts and cheat. Untrustworthy people are more likely to be caught and punished. When untrustworthy behavior does not pay, there is generally less of it, and thus the probability that an individual will find himself in an exchange with an untrustworthy person goes down. Under these circumstances we can hypothesize that people are more likely to be trusting because trust is often rewarded. The data are consistent with this hypothesis, though they cannot tell us whether trust is the *result* or the *cause* of good institutions. This is a puzzle we are about to attack through formal economic modeling.

It's much more problematical to try to figure out the correlation between fair-mindedness and the degree of market integration. We do know, however, that our data are quite inconsistent with the theory I mentioned earlier: that societies like hunter-gatherers and subsistence farmers (the ones

The Shona, Zimbabwean farmers growing cash crops such as maize, tobacco, and peanuts, and studied by Abigail Barr, divide into two societies—those who have always lived in the same village, and those resettled onto vacated commercial farms taken over by the government since independence. When they played the Trust game, the resettled farmers were less trusting than the unresettled.

Commercial whale hunters, the Lamalera of Flores (the third island east of Bali), studied by Michael Alvard, catch large whales in these small rowing boats, a very dangerous thing to do. Their close-knit, cooperative lifestyle was reflected in the way they played the Ultimatum game: offers were often overgenerous, and few were rejected.

that were very greedy in the Ultimatum and Dictator games) are locked into cultural patterns of sharing that prevent them from taking advantage of economic development. If that were the case, we would expect our small-scale societies to be the most generous, and the United States to be the least, which is exactly the opposite of what we found.

Many of the hunter-gatherers and subsistence farmers weren't very generous when they played the games, yet in other aspects, such as the way they share meat from a hunt, they appear to have a high sense of fair-mindedness. I would argue that the rules for sharing a kill among the people in a camp or village are highly specific to that activity. However, when we look at the development of various societies, we want to understand how their rules generate principles of behavior that eventually impact on *impersonal* exchange, which is what often goes on in the marketplace. And contrary to what one might think intuitively, I think these games actually mirror the real-life situation of what it's like to face a completely novel economic opportunity. This, I believe, gives us a better prediction of how people might respond to new economic opportunities than does extrapolation from a highly specific activity such as meat distribution. The latter is externally enforced (often very strictly: an Au villager in New Guinea who doesn't share his catch can be attacked or even killed), while what we appear to pick up from cross-cultural data is that norms of equity are internalized, or self-enforced, in more complex societies.

The Aché of Paraguay, studied by Kim Hill and Michael Gurven, are subsistence farmers who grow mainly manioc, but who often go off for several days on hunting expeditions. A hunter modestly leaves his kill at the edge of the village to be found by others, who divide it up fairly (right), without favoring the hunter's family in any way.

So why are some societies more fair-minded? Is it just a luxury, so that we find more fair-mindedness among wealthy societies? There are a couple of problems with this explanation: if that were the case, we might expect wealth within a society to predict fair-minded behavior. This doesn't happen, though we will continue to test for it. We also might expect to see a plateau in the data comparing the way the games were played in the different societies once a society rises above some minimum subsistence level, and we don't see any plateau—just a gradual incline across societies with offers rising in line with market integration.

Another possibility is that people in market-oriented societies learn that it's convenient to develop rules of thumb for dealing with anonymous exchange situations, and that a 50/50 split is a very nice convention for dealing with a lot of unknown situations.

A third, and related, possibility has to do with reputations. In a market economy, people have to think beyond making one quick killing, and they develop a set of behavior patterns based on the fact that they make a living by doing a lot of small deals. Fair-minded behavior is a very powerful

Jean Ensminger has lived with the same Orma family for a total of over four years since 1978 and watched much generational change. The chief (far right and below) was her first research assistant, and the baby girl in his wife's arms (right center) is now the young woman in the leftmost photo, who has daughters of her own.

signal to potential exchange partners that you're a good guy, a guy with whom one would want to engage in repeat business. Someone who gets a reputation for ripping people off may find that no one wants to trade with them.

The results of this project are highly counter-intuitive to most people. Highly market-oriented folk turn out not to be the greedy capitalists we might expect, and those in small face-to-face societies with strict norms for sharing in some areas don't appear to apply those fair-minded principles in other situations. Or at least not when no one is looking! Our data are also consistent with the hypothesis that clean government fosters both trust and trustworthiness. As yet we understand little of the processes by which fair-mindedness comes to be internalized as part of the way people behave toward each other, rather than having to be enforced externally. But this, together with the capacity to trust, undoubtedly contributes to a better-functioning economy. □

As a young English-literature major at Cornell, Jean Ensminger spent two years in Kenya helping paleontologist Louis Leakey write a book on the Kikuyu people. Seeing how big a role economics played in the day-to-day lives of the poor people there sparked her interest in anthropology and economics, and she changed to an anthropology major, earning a BA in anthropology in 1974, and, from Northwestern University, an MA in 1976, and a PhD in 1984. At Washington University in St. Louis, where she worked from 1985 until joining Caltech in 2000, she became Tileston Professor of Political Economy, and a fellow of the Center for Political Economy. Now professor of anthropology (indeed the only anthropologist on the faculty), she has just been appointed chair of the Division of the Humanities and Social Sciences, the first woman to lead a Caltech division. Her Kenyan experiences, which included living in the compound of the chief's family off and on since 1978, have given her an unusual insight into bottom-up administration that may help her in this new venture. This article is adapted from a talk given to the President's Circle of the Caltech Associates.

First Richard Feynman gives the talk; then, 28 years later, an actor who played him on the stage gives it. This is what's called entropy. This is what happens just before the cosmos reaches a temperature of absolute zero.

Finding Feynman

by Alan Alda

Actor Alan Alda spoke at Caltech's 108th annual commencement, on Friday, June 14. Here he is seen with, from left, Professor of Mechanical Engineering Melany Hunt; Marianne Bronner-Fraser, Ruddock Professor of Biology and chair of the faculty; and Trustee William Davidow. Alda is no stranger to science or Caltech, having hosted ***Scientific American Frontiers*** on PBS for the last eight years. His fascination with Feynman led to the creation of the play ***QED***, in which he starred as the late Nobel laureate.

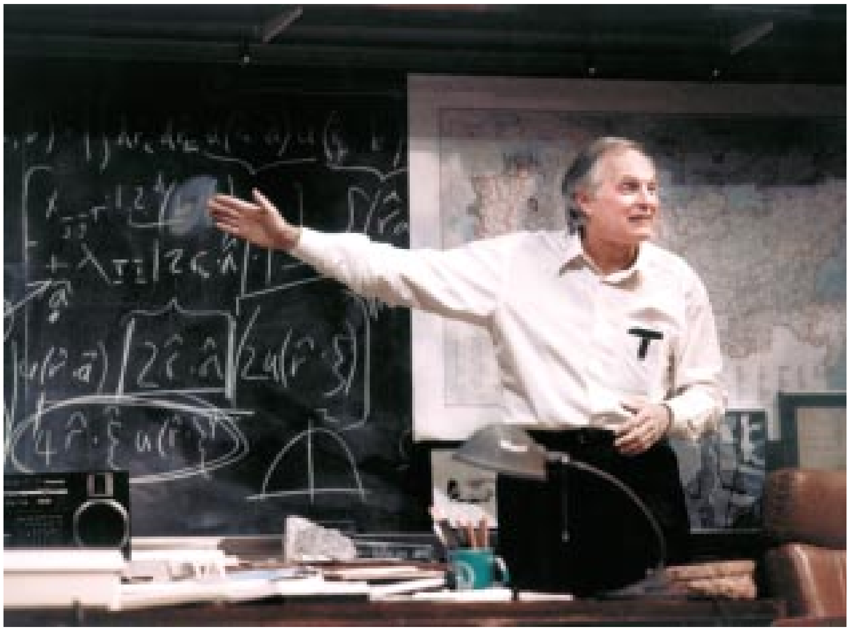
PICTURE CREDITS:
17, 19, 21 – Bob Paz;
18 – Craig Schwartz;
19 – Floyd Clark

Twenty-five or thirty years ago, on my days off from the Korean War, which was at that time being waged at Twentieth Century Fox in Beverly Hills, I would often come to Pasadena to visit the Rembrandts at the Norton Simon Museum, or take a walk in the Huntington Gardens. And sometimes I would drive by Caltech and give it a glance and wonder what interesting stuff was going on in there. I had been reading about science avidly for years, and I was immensely curious about how scientists went about what they did. It didn't occur to me each time I passed by that there was one particular man in one of these buildings who at that moment might have been drawing gluon tubes on a blackboard, or playing the bongos, or just standing looking out the window as a young woman passed by—a man in whom, in a few years, I would become intensely interested.

One day, exactly 28 years ago, he was standing right here, giving the Commencement address. This is the way the universe operates. First Richard Feynman gives the talk; then, 28 years later, an actor who played him on the stage gives it. This is what's called entropy. This is what happens just before the cosmos reaches a temperature of absolute zero.

Let me tell you a little about the path that led me here. After I had read several books about Richard Feynman, I brought one of them, a charming, touching book by Ralph Leighton, called *Tuva or Bust*, to Gordon Davidson at the Mark Taper Forum in Los Angeles. I wondered if he thought we might be able to make a play about Feynman. He suggested Peter Parnell to write the play, and the three of us started off on a journey to find out who Richard Feynman was. We thought we'd open the play a year or so later. Instead, it took us over six years.

We had no idea how hard it would be. For one thing, he was an extremely unusual person. Toward the end of his life, he knew he was dying,



Alda as Feynman.

and he knew exactly what the most important questions were, and he knew he had a shot at answering them—and yet he kept to his habit of doing only what *interested* him.

He spent a good part of his time trying to get to this little place in the middle of Asia called Tuva, mainly because its capital was spelled with no vowels, which, for some reason, he found *extremely* interesting. But, just as getting to Tuva was tantalizingly difficult for Feynman, getting to Feynman became maddeningly hard for us.

What part of him do you focus on? He helped create the atomic bomb; he helped figure out why the *Challenger* blew up; he understood the most puzzling questions in physics so deeply they gave him the Nobel Prize. Which facet of him do you let catch the most light? The one who was a revered teacher, a bongo player, an artist, a hilarious raconteur, or a safecracker?

A mathematician friend of mine suggested that a central image for a play about him could be Feynman's own idea of a sum over histories. Just as Feynman saw a photon taking every possible path on its way to your eye, Feynman himself took every possible path on his way through life. He was the sum of *all* his histories.

We wanted to make a play about Feynman, but *which* Feynman?

A mathematician friend of mine suggested that a central image for a play about him could be Feynman's own idea of a sum over histories. Just as Feynman saw a photon taking every possible path on its way to your eye, Feynman himself took every possible path on his way through life. He was the sum of *all* his histories.

Well, nature may be smart enough to know how to average all the paths of a photon. But we three theater people couldn't figure out how to add up all the histories that made up Feynman.

At one point, I said: "You know what we ought to do? We ought to write a play about three guys sitting around in a hotel room, trying to figure out a play about Feynman. They never figure it out. They just drive themselves crazy."

We researched him like mad, of course. The people who knew him and worked with him and loved him here at Caltech opened their doors and their hearts to us. They were extremely generous and helpful, as we struggled to reduce this irreducible person to an evening in the theater.

I think one of the things I most hoped would come through was his honesty. He never wanted to deceive anyone, especially himself. He questioned his every assumption. And when he was talking to ordinary people with no training in physics, he never fell back on his authority as a great thinker. He felt that if he couldn't say it in everyday words, he probably didn't understand it himself.

I was fascinated by this in him. He knew more than most of us will ever know, and yet he *insisted* on speaking our language.

Like Dante in *his* time, he could say the most exquisitely subtle things in the language of the common people. He was an American genius, and like many American artists, he was direct and colloquial—not afraid to take a look at the ordinary, and not afraid to go deeply into it to reveal the extraordinary roots of ordinary things. And yet, he recoiled from oversimplification. He wasn't interested in dumbing down science—he was looking for *clarity*.

If he left something out, he always told you what he was leaving out, so that you didn't get a false picture of a simplicity that wasn't there. And, later when things got more complex, you were prepared for it. He treated you, in other words, with respect.

But there was something else about him that fascinates me.

I was reading a book by Freeman Dyson the other day and a paragraph about Feynman jumped off the page at me.

> "Dick was … a profoundly original scientist. He refused to take anybody's word for anything. This meant that he was forced to rediscover or reinvent for himself almost the whole of physics…. He said that he couldn't understand the official version of quantum mechanics that was taught in textbooks, and so he had to begin afresh from the beginning…. At the end he had a version of quantum mechanics that he could understand."

When he was talking to ordinary people with no training in physics, he never fell back on his authority as a great thinker. He felt that if he couldn't say it in everyday words, he probably didn't understand it himself.

I think I saw something in this paragraph for the first time; something suddenly clicked into place. The fact that he wouldn't take anybody's word for anything wasn't new to me, or that he needed to go through every step himself in order to understand it. A phrase of his has been on the blackboard behind me every night as I've played Feynman: "What I cannot create, I do not understand."

(People have asked us why that phrase is given so much prominence in the play. It's because the blackboard on our set contains pretty much everything that was on the final blackboard left by Feynman in his office when he died. And "What I cannot create, I do not understand" was right up there at the top.)

Feynman's commencement address—"Cargo Cult Science," on science, pseudoscience, and learning how not to fool yourself—appeared in the June 1974 issue of *E&S*.

But what did jump out at me the other day was the phrase "he couldn't understand the official version of quantum mechanics that was taught in textbooks." Now, this is *Feynman* we're talking about. I suddenly had this picture in my head of Feynman going through the same experience the rest of us do—meeting that same blank wall half way up the mountain. I wondered. Did that give him the ability to remember what it was like to *start* that climb?

So, maybe it wasn't just that he could visualize these little particles and their interactions that made him able to communicate it to the rest of us. Maybe it was also that he could remember what it was like to feel dumb.

Now, here's why I'm going on about this. It may not seem *important* how Feynman did it. Maybe we should just be glad he could do it and let it go at that. But I think it *is* important. Because I think we have to figure out how *we* can do it, too.

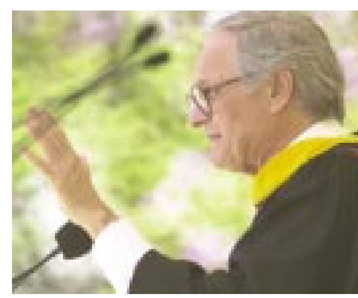
For one thing, we live in a time when massive means of destruction are right here in our hands. We're probably the first species capable of doing this much damage to our planet. We can make the birds stop singing; we can still the fish and make the insects fall from the trees like black rain. And ironically, we've been brought here by reason, by rationality. We cannot afford to live in a culture that doesn't use the power in its hands with the kind of rationality that produced it in the first place.

But right now, instead of reason, a lot of people are making use of wishes, dreams, mantras, and incantations. They're trying to heal themselves using crystals, magnets, and herbs with unknown properties. People will offer you a pill made from the leaf of an obscure plant and say, "Take it, it can't hurt you, it's natural." But so is deadly nightshade. Interestingly, they expect the plant to have active properties to cure them, but they're certain it has no active properties that can harm them. How do they know that?

I mention this, not to denigrate anyone's beliefs (I feel strongly that we're all entitled to our beliefs, just as we're entitled to our feelings), but I bring it up to point out that we're in a culture that increasingly holds that science is just another belief.

And I guess it's easier to believe *something*—anything—than not to know. We don't *like* uncertainty, so we gravitate back to the last comfortable solution we had—no matter how cockeyed it is.

But Feynman was *comfortable* with not knowing. He enjoyed it. He would proceed for a while with an idea as if he *believed* it was the answer. But that was only a temporary belief in order to allow himself to follow it wherever it led. Then, a little while later, he would vigorously attack the idea to see if it could stand up to every test he could think of. If it couldn't stand up, then he simply decided he just didn't know. "Not knowing," he said, "is much more interesting than believing an answer which might be wrong."



You're graduating today partly as Feynman's heirs in this gloriously courageous willingness to be unsure. And just as he was heir to Newton, who was in turn heir to Galileo, I hope you'll think about devoting some time to helping the rest of us become *your* heirs.

I'm assuming you're here at Caltech because you love science, and I'm assuming you've learned a great deal here about how to *do* science. I'm asking you today to devote some significant part of your life to figuring out how to share your love of science with the rest of us.

But, not just because explaining to us what you do will get you more funding for what you do—although it surely will—but just because you *love* what you do.

And while you're explaining it, remember that dazzling us with jargon might make us sit in awe of your work, but it won't make us love it.

Tell us frankly how you got there. If you got there by many twists and turns and blind alleys, don't leave that out. We love a detective story. If *you* enjoyed the adventure of getting there, so will we.

Most scientists do leave that out. By the time we hear about their great discoveries, a lot of the doubt is gone. The mistakes and wrong turns are

Bethe says, "That's interesting, Feynman, but what's the importance of it?" And Feynman says, "It has no importance, it's just fun!" ... Of course, what Feynman was looking for was *serious* fun. It was the awe he felt when he looked at nature. And not just the official great wonders of nature, but any little part of nature, because any little part of it is as amazing and beautiful and complicated as the whole thing is.

left out—and it doesn't sound like a human thing they've done. It separates us from the process.

Whatever you do, help us love science the way you do.

Like the young man, so head over heels about his sweetheart, he can't stop talking about her; like the young woman so in love with her young man, she wants everyone to know how wonderful he is—show us pictures, tell us stories, make us crave to meet your beloved.

Don't just tell us science is good for us and, therefore, we ought to fund you for it; don't tell us to trust you that your fancy words actually mean something; don't keep the tricks of your trade up an elite sleeve. Don't be merchants, or mandarins, or magicians—be lovers!

Look, in our culture we know when a commercial is coming. We know how to turn it off. But love we can't resist.

You may be swayed by people who insist they're only interested in hearing about the practical applications of science. You may be tempted to bend over backwards, telling them what they want to hear.

When Feynman stood here and spoke 28 years ago, he cautioned scientists against going too far in telling laypeople about the wonderful everyday applications of their work, especially if there weren't any. He felt it wasn't honest to pretend there *was* such a benefit—just to get funding for your work.

It's a powerful urge, but it's possible to resist it.

Robert R. Wilson resisted it beautifully. Bob Wilson was a physicist whom Feynman had known well. He had helped recruit Feynman for the Los Alamos project. Wilson was also an accomplished sculptor. He had a foot in each of C. P. Snow's "two cultures."

Wilson built Fermilab, the giant atom smasher in Illinois. But at a congressional hearing in 1969, he was grilled by Senator John Pastore, who wanted to know what an atom smasher was *good* for. Does it in any way contribute to the security of the country?

Wilson said, "No, sir, I do not believe so."

"It has *no value* in that respect?" the senator asked.

Wison looked at him and said, "It only has to do with the respect with which we regard one another, the dignity of people, our love of culture.... In that sense this new knowledge has all to do with honor and country. But it has nothing to do directly with *defending* our country—except to help make it worth defending."

Like Wilson, I don't think Feynman needed to justify his curiosity about nature.

Pure science was pure pleasure. It was fun.

It's like the story of the plate.

The one thing I was certain of from the beginning was that we had to have the story of the plate in the play. It was central. The author, Peter Parnell, would do draft after draft. And I would look at it and say, "Where's the plate?" I drove him crazy.

The plate story is this: After the war, Feynman became depressed. His first wife had just died of tuberculosis, and the realization of the awful destructive power of the bomb he had helped make had finally sunk in. He was teaching at Cornell, but he had no taste for it. He couldn't concentrate. Then, one day, he's in the school cafeteria and some guy starts fooling around, tossing a plate in the air. Feynman watches the design on the rim of the plate as it spins and he sees that as it spins, the plate wobbles. He gets fascinated, and he tries to figure out the relationship between the spin and the wobble. He spends months on this, and finally comes up with this complicated equation, which he shows to Hans Bethe.

And Bethe says, "That's interesting, Feynman, but what's the importance of it?" And Feynman says, "It has no importance, it's just fun!"

But, see, that's the thing—it not only brought him out of his slump, but that playful inquiry, according to Feynman, eventually led in a circuitous way to the work that won him the Nobel Prize.

But no matter *where* it might have led him, he made up his mind that day in the cafeteria never to work on anything that didn't interest him—that wasn't fun.

Of course, what Feynman was looking for was *serious* fun. It was the awe he felt when he looked at nature. And not just the official great wonders of nature, but any little part of nature, because any little part of it is as amazing and beautiful and complicated as the whole thing is.

So, this is interesting. I'm urging you to be like someone who I admit I've found to be pretty elusive.

Here I am, seven years later. And, just as Feynman never got to see Tuva, I never really found Feynman. Not really. I came close; but he was too many things. He had too many histories.

We came up with a play in *QED* that was immensely satisfying. It was beautifully written and beautifully directed, and it gave the audience a Feynman that was as close an approximation as we could come up with. But part of me feels that a large chunk of the man is still beyond our reach—probably beyond the reach of anyone. He's just out of sight, smiling at us. Laughing at how he put one over on us, letting us think he was just an ordinary guy. A guy we could *get*.

It turns out, though, that the old thing about the destination not being as valuable as the journey really *is* true.

Because, when we began, *finding* Feynman seemed important, and I guess it was—but, as it turned out, *looking* for Feynman has been the fun.

What if each of you decided to take just one thing you love about science and, no matter how complicated it is, figure out how to make it understood by a million people? ... If just a few of you were successful, that would make several million people a lot smarter.

Every once in a while, though, I can feel Feynman looking over my shoulder, and he's *not* smiling. Like right now. I'm at the end of my talk and I feel the pressure of the words he closed *his* talk with 28 years ago. "One last piece of advice," he said. "Never say you'll give a talk unless you know clearly what you're going to talk about and more or less what you're going to say."

In other words, where are the brass tacks?

Alda and Feynman finally meet in this montage by Caltech graphic artist Doug Cummings, presented at the post-Commencement luncheon by President David Baltimore.

Okay, let me be more or less practical. I'm going to propose something to you today. I realize it's a childish idea, something only an unschooled layperson would come up with—but it's specific enough that it might get you thinking.

What if each of you decided to take just one thing you love about science and, no matter how complicated it is, figure out how to make it understood by a million people? There are about 500 of you taking part in this ceremony today. If just a few of you were successful, that would make several million people a lot smarter.

How you do it is up to you. You're clever people, and I bet you come up with some ingenious solutions. On the other hand, you may be thinking, "*WHY?* Why should I do this impossible thing?"

Well, I don't know; maybe for the same reason that the birds sing.

If it does for you what it does for birds, there's a lot to recommend it:

1) It's a good way to improve your chances of having sex.

2) It feels good to sing.

3) Singing is the music nature makes when it dances the dance of life.

You are the universe announcing itself to itself. You open your mouth and a little muscle in your throat makes a corner of nature vibrate. You're one part of the forest saying, "This is what I think I know," while another part of the forest is saying, "Yeah? Well this is what I think *I* know!" Your chirpings are the harmony of all knowledge.

You've learned so much in this place about how nature works. Is there anything more beautiful than that? Is there anything greater to sing about?

So *sing*. Sing out. Sing. Out.

Thank you, and good luck. □

Alan Alda, of course, played Dr. Hawkeye Pierce in the classic TV series M*A*S*H. *He also knows a thing or two about writing—during the show's 11-year run, he became the first person ever to win Emmys as actor, director, and writer. (*M*A*S*H *netted him five Emmys and 25 nominations.) A native New Yorker and son of the distinguished actor Robert Alda, his first regular television gig was on the groundbreaking political satire* That Was the Week That Was*, in 1964.*

He has appeared in movies too numerous to mention, the most recent being What Women Want*, and wrote, directed, and starred in* The Four Seasons*,* Sweet Liberty*,* A New Life*, and* Betsy's Wedding*.*

His Broadway credits include The Owl and the Pussycat*;* Fair Game for Lovers*, which won him a Theatre World Award; and* The Apple Tree*, which earned a Tony nomination.* QED *had its world premiere at the Mark Taper Forum in Los Angeles on March 22, 2001, and concluded its New York run at Lincoln Center's Vivian Beaumont Theater on June 10.*

The Quantum-Classical Transition on Trial:

Is the Whole More Than the Sum of the Parts?

by Hideo Mabuchi

There are some things they don't tell aspiring young scientists. Most of us assume that you work very hard to get through school, you get your degrees, and then, if you're very, very fortunate, you manage to land a job at a prestigious academic institution. You get your research group going, and then life is good, because finally you can get down to the business of chasing after all those shining Holy Grails of science—like grand unified theories of physics, or cures for cancer or AIDS.

Some people certainly do pursue the good life in that way. But others of us, for whatever reason, decide to follow a somewhat different career path. Rather than running out to the great frontiers of science, we get stuck back in the land of things that most people think are already understood. That's because somewhere along the line, we stumble across something that feels to us like a slight inconsistency or incompleteness. Maybe it's just some little detail, just a small wrinkle that needs to get smoothed over. But I think if you look back at the history of any science, you will find moments where something seems to be a small inconsistency until you tug on the loose thread, and everything unravels.

IMAGE NOT APPROVED FOR WEB USE

My topic here concerns one of those inconsistencies, the quantum-classical transition, which, in a sense, dates back to the historical debates between Bohr and Einstein. I had added "on trial" to the title, but then I began to wonder: What court is trying this case, and what are the charges? Then I realized that this trial goes on inside my own head. I'm working on the quantum-classical transition because I think it's interesting, but is it really the most important thing that I could do with my early career? Is this one of those questions that can lead to big things? Or is it just going to prove to be a little wrinkle? If you asked a roomful of physicists whether this was a good thing to study, many of them would just shrug their shoulders.

So in this "trial" the prosecution will charge that the quantum-classical transition is trivial and uninteresting. As the defense attorney, I want to try to

What happens where quantum and classical theories meet? If one is yellow and the other blue, would you get a green theory? Unlikely—they're too different to mix together smoothly.

convince you that it is, in fact, an important thing to look at. But at the same time, I also want to play devil's advocate and present the prosecutor's case. I'll try to give you both sides of the story, but I'm biased, of course.

First, I'd like to explain what is classical, what is quantum, and what we mean when we say that there's a transition between the two. We can talk about things that are big and things that are small and what theories we use to describe them. If we start in the macroscopic realm with a football field and come down from that size 100 times, we get down to about a meter, which is approximately the size of, say, a bicycle. Another 100 times brings us down to the centimeter scale, about the size of a dime. If I come down another 100 times, then I'm talking about a grain of sand or something the size of a fraction of a millimeter. And another factor of 100 brings us to the micron scale and things that we can't see with our bare eyes—things like living cells.

Now let's jump across a big gap and go smaller by a factor of 100,000, which takes us from the cell down to about the scale of an atom. And if we go down from the scale of an atom by another factor of 100,000, then we're talking about the atomic nucleus—the clump of protons and neutrons that sits inside every atom. At this scale, I think it's safe to say that we're truly in the microscopic realm of physical theory.

One of the surprising legacies that 20th-century physics has left us is the understanding that, as we describe things that occur in nature, we have to use two very different physical theories, depending on whether we're talking about things in the macroscopic realm (bicycles, coins, and grains of sand) or things that are down in the microscopic (atoms and their nuclei). Classical physics describes behavior of the former, behavior that you're familiar with in your everyday experience: balls bounce, sticks fly through the air when you throw them. Then there's quantum mechanics, which is kind of strange and fuzzy. Quantum mechanics describes the way that atomic and subatomic particles behave. This is a behavior that we never get to experience directly, simply because these things are just too small.

Yet there's kind of a no-man's land in the middle where things are slightly bigger than atomic size, but much, much smaller than living cells. The question is: What's going on in this no-man's land between quantum mechanics and classical physics? We're just starting to be able to do sophisticated experiments on systems that live in this range of sizes, and therefore we're well positioned to start asking concrete questions and provide concrete answers about this transition zone.

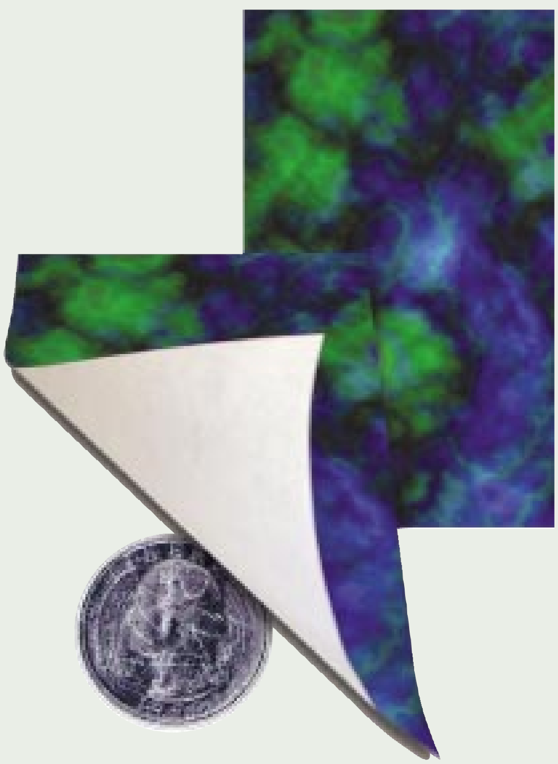
The counterargument would claim: But we're already able to design and construct and use very sophisticated technology that works in that range of sizes between atoms and cells. We have things like computer microchips at the micron scale, and even below that we have the techniques of biotechnology and genetic engineering. So, if we can already reach down there and do such amazing things, how can anybody claim that there's something mysterious about this transition zone?

Now, whatever you may think of technology (and who among us has never regretted the existence of e-mail?), it makes a compelling argument that we can use classical mechanics to compute and design things in the microscopic world. And, sure, we have to use different mathematics to describe or design in the truly microscopic realm, but there's nothing mysterious about that. We understand when we're supposed to use one theory and when we're supposed to use the other.

But these two theories are very different; they have a very different feel, a very different flavor. If I were to represent them by different colors, I could have a yellow theory that describes the behavior of small things and a blue theory that describes the behavior of large things. Then, you might expect some sort of mixture of the two in the middle: a green theory. But quantum mechanics and classical physics are so different—kind of like oil and water—that it's very difficult to understand how they might mix together in a smoothly graded transition from one to the other. So maybe this complicated mishmash of stuff in between *is* important to study.

Physicists often ask: Where are the frontiers of fundamental physics? And usually the answer is: at the extremes of the size scale. So, at the extreme microscopic end, we should be asking about the behavior of particles or systems that are much, much smaller even than atomic nuclei. This leads you to the study of things like string theory and

On the other hand, if we can understand exactly how it matches up to classical physics, I think we stand to learn a lot about what quantum mechanical theory really is and why it looks the way it does.

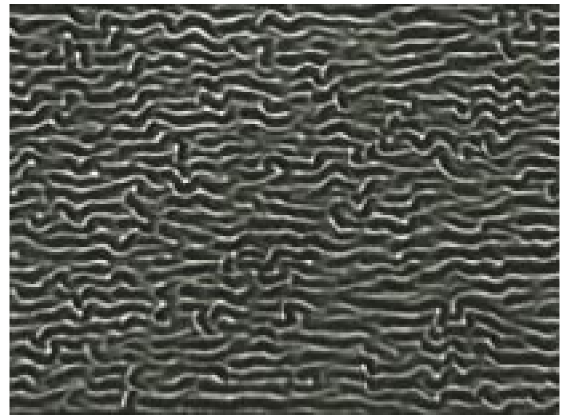


other sorts of grand unification theories. At the other extreme of the size spectrum, you could ask about things as large as the entire universe. At that point you're into astrophysics and cosmology. If those are the Wild West frontiers of physical theory, then this quantum-classical transition would be middle-class suburbia.

Now, it may turn out that, as you look into the great complexity of things that happen at this mesoscopic size scale between the microscopic and the macroscopic, you're just going to turn up a bunch of details. Maybe nothing fundamental happens there. On the other hand, if we can understand exactly how it matches up to classical physics, I think we stand to learn a lot about what quantum mechanical theory really is and why it looks the way it does. So, I'd like to give you a bit of a sense for the differences between the two in their basic features.

As an example, let's take a coin, a quarter. When I lay it on the table, I can place it heads up or tails up. Now, suppose I prepare this coin in some way that I don't describe to you: maybe I flip it and let it land as it may; maybe I spin it; maybe I ask somebody else to lay it heads up or tails up. Then I cover it with a card. I've done all this in the back room, so that you haven't been able to see what I've done. Now I bring the whole table out to you, and I tell you that I took an ordinary quarter and put it either heads up or tails up on the table and covered it with the card. If we're describing things in terms of classical physics, then I think it's fair to say that the coin is going to be either heads or tails. We don't happen to know which one of these two is the case, but it's either one or the other.

If we wanted to try to describe the state of this coin using quantum physics rather than classical physics, then I could have done something in the back room such that the coin was prepared as both heads and tails at the same time. It's not that it's one or the other and you just don't know which, Quantum systems interact in a linear way—like the ripples on Millikan Pond. Macroscopic classical systems, on the other hand, are nonlinear—like the convection pattern in a more viscous fluid (far right); © Nonlinear Phenomena Group, LASSP, Cornell University, May 2002.



but in some sense you have to imagine that it's both. In the classical world your inability to predict what we're going to see when we lift up the card is strictly the result of not knowing something about the system. If I just whispered "tails" (and if you believed me), then you would know exactly what you would find when you lifted up the card. So, one very important feature of quantum physics that we believe is truly distinct from classical physics is that in quantum physics we can prepare things in a way such that there is intrinsic uncertainty about what's going to happen in the future. In classical physics, if you're uncertain about what's going to happen, it's only because there's stuff that you don't know.

Another distinction between the basic features of quantum and classical theory is that, as far as we understand, the dynamical behavior of quantum systems—the way they move and interact with one another—is always linear. On the other hand, we know that the motions of macroscopic things —like planets and asteroids or, say, fluids in a tank—can be highly nonlinear, which is a much more complicated kind of behavior.

This is a little hard to illustrate, but I'll try to give you an idea in terms of how waves behave in linear and nonlinear dynamics. Think about ripples propagating on the surface of a pond: the peaks and troughs are more or less stationary—they travel pretty much undisturbed until they hit some kind of obstruction, such as a rowboat or a twig sticking up out of the water. When they hit that twig or rowboat, they do something rather complicated: they diffract and change direction. But what's important is that these waves are propagating independently. They don't mess each other up; they're happy to coexist. This is linear behavior.

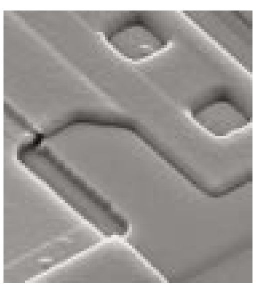
Nonlinear waves act very differently. You can see this complicated kind of behavior in the ripples or waves in a more viscous fluid. Even without any obstruction, the ripples propagate at different periods, and these different periods mess one another up. They look as if they're all tangling around one another. In quantum physics, the evolution of systems is simple and orderly in the sense of linear wave propagation, but when we make systems macroscopic, we're able to observe the kind of complex, nonlinear behavior illustrated by the second example.

This is why I think there seems to be something mysterious, or at least interesting, about the quantum-classical transition. The quantum world is kind of fuzzy (in other words, there is intrinsic uncertainty), but at the same time, systems evolve in a rather orderly way. On the other hand, in macroscopic systems, things are sharply defined (in the sense that uncertainty is never necessary), but the evolution of classical systems can be really complicated, even chaotic. Evidently, what's happening is that all the fuzziness and uncertainty at the small level in a way smooths itself out when you make things sufficiently large. We take these really small fuzzy globs that are evolving in an orderly fashion, and when we put enough of them together, for some reason everything crystallizes and becomes sharp while its dynamics becomes chaotic.

Why, exactly, does it work that way? Why doesn't it go in some other direction? And perhaps the most important question is: Why is this transition so robust? It doesn't seem to matter what kinds of quantum pieces we take or how we connect them together; as long as there are enough of them, we get this transition to classical behavior.

Now I'll turn to playing devil's advocate for a while. On the next page is a sequence of images. The first is a snowflake, a thing that you can pretty much see with your bare eye. You can't resolve very much without a microscope, but you can see it. The size scale here is something like a 10th of a millimeter. The next image, which was taken with an electron microscope, shows features

If we can isolate an atom sufficiently well so that nothing is touching it, nothing is poking or prodding it, there's no heat and no electromagnetic waves, then under these conditions the atom is happy and wants to do its quantum mechanical thing.



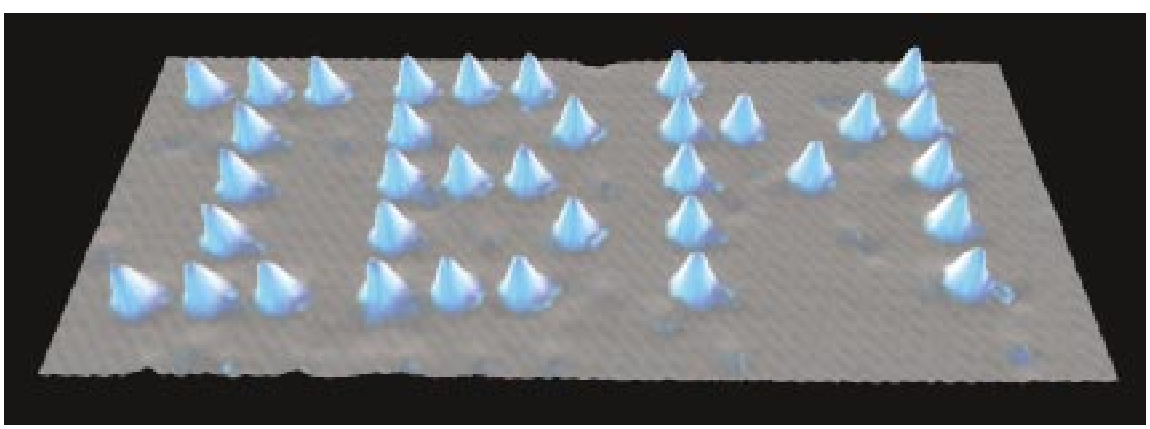
at the size scale of about 10 microns, or .01 millimeters. This is about the size of components on a computer chip, and it looks like a bar with holes in it sitting on a plateau of some kind. I could be showing you an aerial photo of a building, and it would look similar; there's nothing strange about it.

The last image was made by scientists at IBM's Almaden Research Center with a scanning tunneling microscope. The gray, fuzzy base is the very clean, flat surface of a chunk of nickel metal, and the conical, blue lumps are individual xenon atoms sitting on it. This microscopy technology enabled the scientists to pick up a bunch of atoms that they happened to find lying on the surface and rearrange them to spell out the name of the company. This wasn't generated as a computer graphic; it's a real microscope image of individual atoms. A single atom is something like a 10th of a nanometer, or .0000001 millimeters. This image could just as well be snow cones lying on the ground on a winter day. What is so quantum and strange about it?

I've been saying that microscopic systems—such as individual atoms—behave quantum mechanically. So, how is it that you can image those atoms sitting unmysteriously on the surface of the metal like blue lumps of clay? The reason is that, in order to get an individual atom to behave quantum mechanically, you have to put it in extreme isolation. Just the fact that this atom is sitting on the surface of a chunk of nickel is enough to induce a quantum-classical transition. We have to pick the atom up off that surface, suspend it in empty space—really, really empty space—and then allow it to do what it wants. The transition from this lump-of-clay-type behavior to something more quantum and mysterious has to do with how well isolated the system is. If we can isolate an atom sufficiently well so that nothing is touching it, nothing is poking or prodding it, there's no heat and no electromagnetic waves, then under these conditions the atom is happy and wants to do its quantum mechanical thing.

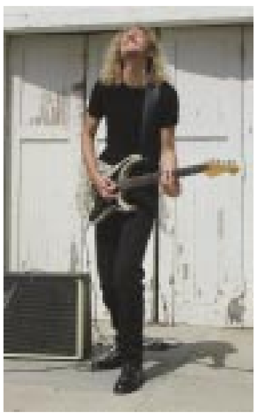
We might think of macroscopic behavior as being like a collection of musicians in a symphony orchestra all playing classical music together. But, perhaps, when nobody else is listening, one of those individual musicians might go off and play head-banging, heavy-metal music in private. Similarly, quantum systems in isolation behave in one way, but when we bring large collections of them together, they behave in a different way. There's a very sophisticated group dynamic that causes a different kind of behavior to emerge when individual people collect in large crowds and socialize and interact with one another. This is called an emergent behavior of collective systems.

Note that I've been saying that, when you take a single atom off the surface and put it in isolation, we expect it to behave in a quantum mechanical fashion. But I was very careful not to say that we expect to *observe* quantum mechanical behavior, because we're not even allowed to be looking at it. I think the fact that the act of measurement itself is very disruptive is actually a very big clue to what's going on in the quantum-classical transition. Somehow this act of measurement, of looking, we believe can force a transition from quantum mechanical behavior to classical. And our microscope image of the xenon atoms sitting on the nickel surface is really sort of a classical pro-



The snowflake at top left is on the scale of about a 10th of a millimeter. The components in the computer chip below it are about .01 millimeters. And the single xenon atoms (in blue) at left are about .0000001 millimeters. (Image courtesy of IBM Almaden Research Center.)

Musicians playing together, like the Los Angeles Philharmonic Orchestra shown here, exhibit the emergent behavior of collective systems. A single musician in isolation, behaving in an entirely different way, might be said to be acting quantumly.



jection of what these quantum objects are doing.

Everything we do in our lives (including every time I go into the laboratory and perform an experiment on individual atoms or individual photons) is classical and macroscopic. We go into the lab, we turn some knobs, an experiment happens, and data come out to us in the form of numbers or signals or whatever. The knob is never in two different positions at once; it's always in one or the other. All the interactions that I have with the experimental apparatus are all perfectly classical. And yet, somehow, by performing such experiments, we're able to convince ourselves that the behavior of the objects under study is actually qualitatively different. To me, that makes understanding the process of measurement a very important key in understanding the quantum-classical transition. The measurement theory has to look at microscopic systems, whose states start out being quantum, but the information that we get as a result of the measurement—the images from the microscope—has to be sensible in a classical way.

Remember the example of the quantum coin under the card. What actually happens when we finally make a measurement—when we lift up that card and look underneath? What would it look like to see a coin both heads up and tails up at the same time? When you look, you have to see one side or the other. There's no way that it can be both when you're sitting there staring at it. So, something about this act of measurement took a quantum precondition—being both heads up and tails up—and turned it into something that makes sense to us. And if we then put the card back over the coin and very carefully look again, it will be the same side up the second time.

Quantum physics imposes intrinsic uncertainty: We can't predict whether we'll see heads or tails because the coin is in both states at once. It's not just that we don't know enough; it really is completely unpredictable. A measurement made on a quantum system removes the uncertainty and forces something definite to happen. So in a sense, measurement is tied to this whole business about intrinsic uncertainty turning into classical uncertainty. Let's say that we looked at the coin once and found tails. Now we bring in somebody who was out of the room at the time and show him this card and say: "Initially we prepared a quantum state that was both heads and tails, and then we looked and we saw something and then put the card back." But we don't tell this new person *what* we saw. The uncertainty that this new person has about what's under the card is now a completely classical uncertainty, because they know that we had to have seen something definite. So they will now describe the state of the coin under the card as being either heads or tails. Even though it's a gross oversimplification to say that this is a quantum-classical transition, it does suggest that measurement plays an important role.

If I'm going to claim that measurement is the key to the quantum-classical transition, I have to try to explain what measurement has to do with the kind of group dynamics that causes classical behavior to emerge out of quantum behavior when you put enough stuff together. I have to be able to relate this to what happens when nobody is peeking. If we look back at the image of the individual atoms sitting on the nickel surface, it's a bit unsatisfying to think that the only reason that we see "IBM" is that we looked. (Or that, had we not been looking, this might say "Apple" or "Sun.") I think even a specialist in quantum physics would agree that it's probably safe to believe that even when you're not looking at them, those atoms are, in fact, sitting there like little lumps of clay spelling out "IBM."

But why is it exactly that when you put lots of small systems together, somehow collectively they decide that they need to behave classically? This is a profound idea that we don't really understand very well. But the advent of the laser about 30 years ago gave us a new way of thinking about the problem and clarifying the relationship between measurements made on quantum systems and emergent group dynamics. The details of this theory (which was originally developed largely for the purpose of modeling masers and lasers) are beyond the scope of this article, but I'll try to give you a sense for our modern understanding. Using the coin-and-card example, we say that the state of being both heads and tails at the same time is a *coherent* mixture of the two possible states, whereas heads *or* tails is an *incoherent* mixture. *Decoherence* is the term we use for describing the process of turning one of those kinds of uncertainty into the other, such as by looking underneath and then asking somebody else to do so.

Decoherence doctrine offers us a way of understanding all this stuff. We can say that, even when nobody is looking at these xenon atoms on the nickel surface, any physical environment that these atoms happen to be coupled to is, in a way, continuously measuring where they are. So, even though the individual atoms are sitting on what looks to us like a smooth surface, that metal surface is made up of lots and lots of atoms. A chunk of metal that size would have something like a million billion billion atoms or more. Decoherence doctrine says that the xenon atoms are sort of pushing off against the nickel atoms. And when one atom starts to jiggle, it shoves everybody else around. So coupling can be viewed as the atoms in the metal making continuous measurements of the xenon atoms, asking at every point in time: "Where are you?" They force the xenon atoms to stop exhibiting quantum behavior and decide where they're going to be.

Now, as systems get larger and larger, it becomes harder and harder to isolate them in order for them to behave quantum mechanically in the first place. And, while we know how to isolate a single atom, we have no idea how to pick up a baseball and levitate it in empty space, completely isolated from everything else in the universe. The decoherence process happens faster and faster, and more and more inevitably, as we start considering larger systems.

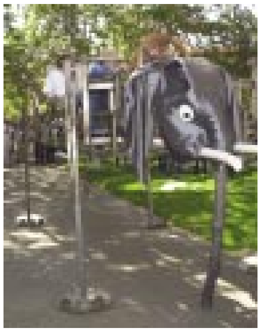
So, for people who work in decoherence (and I have to admit that I'm one of them), the doctrine explains the quantum-classical transition in the sense that we can at least point to a few examples where we feel that we understand how quantum uncertainties get turned into classical uncertainties. And maybe in a few cases we understand where nonlinearity comes from. But, going back to our "trial" again, does this make quantum-classical transition an important and fundamental field or just a set of little quantitative wrinkles?

One powerful argument for the defense is the field of quantum computing. Of great interest in the past five years and a very active topic of research here at Caltech, the idea is that it may be possible to build computing devices in which the little logic things—the chips, the elements—inside the computer behave according to quantum physics and not classical physics. These would be computers made not of chips and resistors and logic gates, but of individual atoms somehow coupled together. This is a very exciting idea, and many people around the world are chasing after it. A large enough quantum computer would be able to solve mathematical problems that a regular computer could hardly begin to solve. So far, only baby steps have been taken in terms of actually building something or even writing down theories about how it might work. It's probably a good 20, 30, or 50 years off before we really have any hope of building such a thing. Nonetheless, we're starting to do the basic science in this field.

You see it in the respectable science journals (one famous article published in *Physics Today* bore the title, "Quantum Computing—Dream or Nightmare?"), and the popular press has picked it up as well. One quote that I really like came from my thesis adviser, Jeff Kimble (the Valentine Professor and professor of physics), and appeared on page 2 of the February 18, 1997, *New York Times* as the Quotation of the Day. On progress in the field of quantum computing, Jeff said: "Theory is way ahead of experiment. It's like Hannibal trying to cross the Alps. We'd really like to run ahead and see what's on top, but we have all these elephants to deal with." Jeff is referring to trying to build something that's fairly large and make it behave quantum mechanically. We have to fight against this process of decoherence, and the things we think we already understand about the quantum-classical transition suggest to us that this may be a losing battle. As we try to make a quantum computer larger and larger, so that it can solve bigger and bigger problems, we may find it harder and harder to prevent this thing from just making a transition into classical behavior. Then we would just have a very expensive version of an ordinary old computer.

Despite the problems, the reason why people are staying in this field is that there have been some remarkable developments in the theory of quantum computing—largely here at Caltech—called fault-tolerant architectures. It works in somewhat the same way that classical computing

"Theory is way ahead of experiment. It's like Hannibal trying to cross the Alps. We'd really like to run ahead and see what's on top, but we have all these elephants to deal with." —*Jeff Kimble*



Not exactly Hannibal and the Alps—or quantum computing—but Caltech students have dealt with experimental elephants, as demonstrated here. The walking (sometimes) mechanical elephant appeared on Ditch Day, May 22.

devices deal with small errors. You may have stored some information, but when the disk drive reads it off, it occasionally makes some errors in reading all the zeroes and ones. You rarely notice this because there are mathematical procedures for correcting those errors. With quantum computing devices, we're not talking about errors per se, but about decoherence problems, which are very much like errors in that they influence the computer to behave incorrectly. A team led by Professor of Theoretical Physics John Preskill was able to show that if you build the architecture of a quantum computer in a very specific way, or in one of a class of specific ways, it's possible to correct the kinds of errors caused by decoherence.

Decoherence theory tells us that, when we take lots of tiny quantum parts and connect them together in a general way, we can pretty much expect to get a classical whole out of them. But the lesson that we think we're learning from the theory of fault-tolerant architectures is that it *is* possible to find very specialized and specific configurations of parts inside a quantum computer such that you can resist that transition to classical.

We already know of a couple of ways to try to do this, but we don't know whether the schemes that people have come up with so far are the best possible ones. Are they, in fact, really clumsy schemes, and if we look harder will we actually find much better ones? Now, the fact that we don't even understand whether the schemes that have been suggested so far are good or bad tells you that we're just at the stoop and trying to get into the door of understanding what's going on in there.

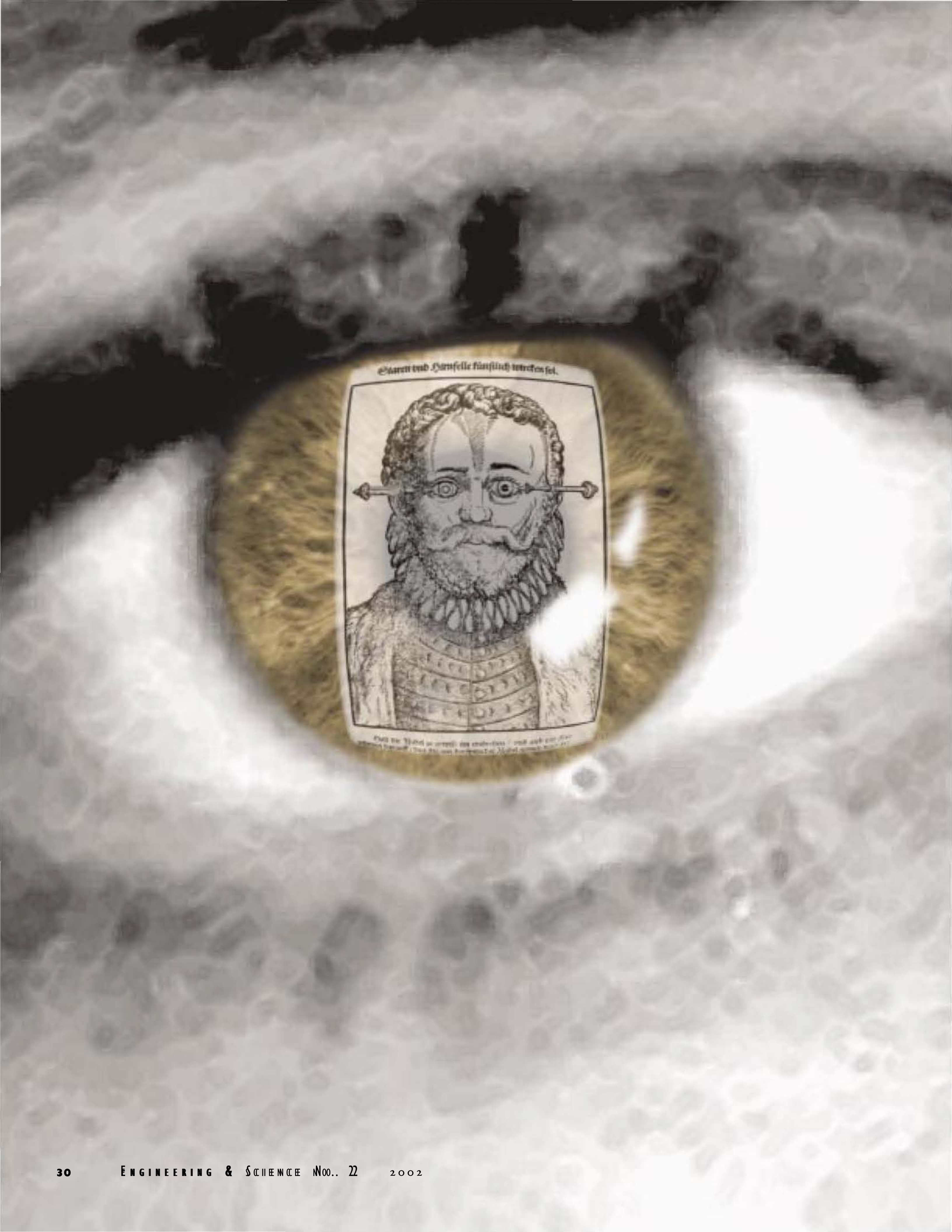
Back to the trial: the prosecution has been trying to argue that the quantum-classical transition is just an estuarial zone between two very well understood theories. We know about classical physics; we know how to compute with it; we can design technology at the nanoscale. And we know about quantum mechanics; we know when we're allowed to use it. If there's an incomplete understanding of the stuff in the middle, it doesn't matter much.

The good thing about being on the defense is that you don't necessarily have to make a compelling case. You just need to introduce a reasonable doubt that the prosecution's argument is not airtight. I have tried to give you some examples of reasons why I think our understanding of this transition is incomplete in some really fundamental way, and that many interesting questions remain completely unanswered. And now we have a couple of leads on how we're supposed to study things in this region. We think that quantum measurement (this is what my group does) is going to be an important key, and we understand a little bit of what's going on in the now traditional theory of decoherence. Defining what happens in the quantum-classical transition may be critical to building a quantum computer that will resist that transition. □

Hideo Mabuchi came to Caltech as a grad student in 1992, after earning his AB from Princeton. When he finished his PhD (1998), he stayed on as assistant professor of physics and became associate professor in 2001. He is currently associate professor of physics and control and dynamical systems. For his work in quantum optics and atomic physics and in optical biophysics, he has been named a Sloan Research Fellow and an Office of Naval Research Young Investigator, and has received a $500,000 "genius" grant from the John D. and Catherine T. MacArthur Foundation (class of 2000). Mabuchi was listed by Discover *magazine among the "Twenty Scientists to Watch in the Next Twenty Years." This article was adapted from his Watson Lecture of last November.*

(The quantum musician on page 37 is Ulrik Beierholm, grad student in Computation and Neural Systems.)

PICTURE CREDITS: 24, 25, 27 – Doug Cummings; 26 – Ken Libbrecht; 27– L.A. Philharmonic; 29 – Bob Paz



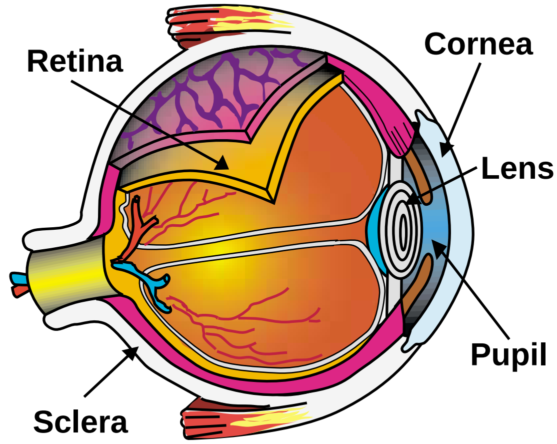
Squishy Is Good

by Douglas L. Smith

Titanium implants are all very well for bones, but soft and pliable is the name of the game for tissues. Professor of Chemical Engineering Julia Kornfield works with stretchy, flexible molecules called polymers. Polymers are long chains of short, simple units, called monomers; plastics are polymers, as are proteins. Kornfield studies how these molecules bend, flow, melt, solidify, and sometimes dissolve, and how you exploit these properties to create everything from squeeze bottles to seat belts. These days, Kornfield, who got bitten by the biotechnology bug as an undergrad, is spending more and more of her time experimenting with gloppy goos for internal use.

One such use addresses a problem that in time comes to most of us—cataracts. So called because it was believed that cloudy material was flowing down through your eyes like a waterfall, they are in fact caused by your eye's lenses losing transparency with age. This can swaddle the outside world in perpetual fog, and in extreme cases leads to blindness. "Most people who are 60 years old have incipient cataracts," says Kornfield. "And by the age of 75, you're a very lucky person indeed if they're not bothering you." The standard treatment calls for replacing the cloudy lens with an artificial one. These lenses are usually made of flexible plastic, which can be rolled up and inserted into the eye through an incision as small as two millimeters—about the diameter of a cooked rice grain. "This is, in fact, the most common surgical procedure for individuals 65 and over. Three million operations a year are performed in the United States; 13 million worldwide."

But it's not an ideal solution: as the eye heals, the accumulating scar tissue changes the position and orientation of the new lens, and even the shape of the eye itself. So a lens that was perfect beforehand generally won't be quite right in the end. There's no way to predict exactly how the scar tissue will grow, and the lenses aren't adjustable, so about one-half of all cataract patients wind up needing glasses or contact lenses. Of course, wearing glasses is infinitely preferable to not being able to see at all, but eye surgeons would love to

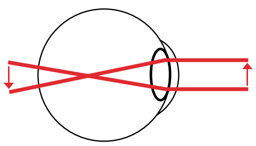


Left: The first known treatment for cataracts was a poke in the eye with a sharp stick. Called "couching," it pushed the cloudy lens aside so that patients could at least see form and color, and was described by the Hindu surgeon Susruta circa the fifth century B.C. Things had not progressed much by 1583, when Georg Bartisch wrote *Augendienst*, from which this woodcut is taken. (The text below the drawing admonishes the surgeon to be careful while screwing the needle into the eye!) Benito Daza De Valdes (1591–1634), an official of the Spanish Inquisition, proposed replacing the lens with an implant—a notion he presumably came up with in his spare time. But these implants, usually of glass, were dismal failures because the body rejected them. During World War II, British ophthalmologist Harold Ridley noticed that airmen showed no adverse reactions to the shards of Plexiglas from bullet-riddled canopies that sometimes lodged in their eyes. Ridley performed the first successful implant, of Plexiglas, in 1949.

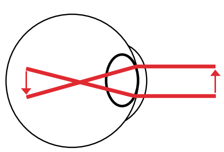
Right: The anatomy of your eye. The sclera is the white part of the eyeball; the cornea, the transparent part where the light enters. The light-sensitive cells live in the retina.

have every patient come out of the operation seeing clearly.

Daniel Schwartz, associate professor of ophthalmology at the University of California, San Francisco, wanted to create a lens whose prescription could be adjusted precisely, without touching the eye in any way, once everything had healed completely and the patient's vision had stabilized. Such a lens would have to be adjustable for nearsightedness, farsightedness, and astigmatism; the adjustment would have to remain stable for years afterward; and, of course, the lens would have to be biocompatible. So Kornfield's phone rang one day, and there was Schwartz, looking for advice. Kornfield, in turn, called Robert Grubbs, the Atkins Professor of Chemistry, whose specialty is making custom-tailored polymers with unusual properties, and who had even ventured into the world of cataract-replacement lenses back in the '80s. Schwartz flew down, and the threesome had a brainstorming session.

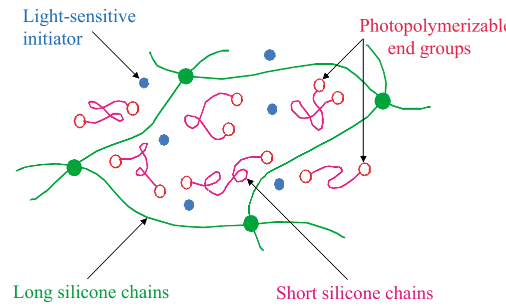


Above: If a lens is too flat, it won't bend the light rays enough to focus them on the retina. Below: But if the lens is too steeply curved, it will focus short of the retina.



There are basically two ways to change a lens's power. One is to change its shape. The more it bulges in the central region, the shorter its focal length. So if an eye is farsighted—that is, the lens is focusing behind the retina—and you thicken the lens up just a smidgen, you can bring the focal plane forward onto the retina; conversely, in a nearsighted eye, you can flatten out the lens to push the focal plane back to the retina. The other option is to change the lens's refractive index. If you have two lenses of the same thickness and radius of curvature, the one made of the higher-refractive-index material will be more powerful. Recalls Kornfield, "Dan said, 'You know, lasers are very frequently used in the eye; eye surgeons feel very comfortable with them.' Bob and I were aware of polymers that had a refractive index you could increase with light—that's how they write holograms on credit cards—so together we envisioned a laser-adjustable lens" made of such a polymer.

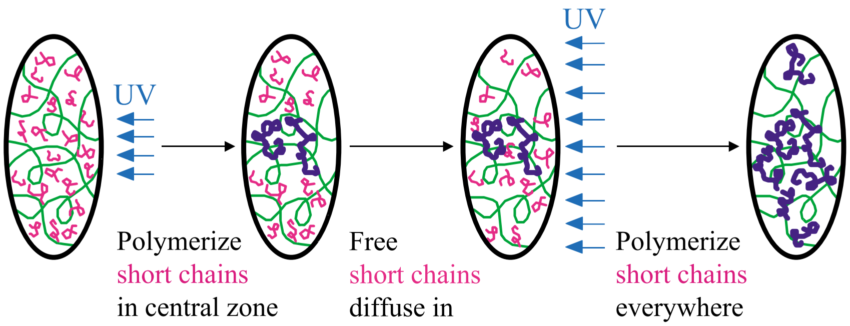
The chemists' first notion was to make the lens from a glassy polymer such as polymethyl methacrylate, better known as Plexiglas, whose chains of 100 to 200 monomers would be connected to one another to form a space-filling, three-dimensional mesh. Swimming through the mesh like minnows through a tuna net would be smaller molecules of only 10 or 20 monomers—too big to be water-soluble and escape into the eye, but small enough to be relatively nimble. The free ends of these molecules would be designed to link up when exposed to strong ultraviolet light, a process called photopolymerization. And a clever choice of monomer would give the short molecules a higher refractive index than the big ones that make up the net. After the eye had thoroughly healed, explains Kornfield, "if we were to shine light at the middle of the lens, all the short guys there would hold hands. Then the free chains on the outskirts would say, 'Hey, there are no short



chains over there!' and they'd diffuse in, raising the refractive index" and correcting residual farsightedness. Alternatively, shining the laser around the lens's periphery would suck the free chains out of the central region, decreasing the refractive index there and fixing nearsighted folk. And astigmatism, in which the lens focuses asymmetrically, could be dealt with by shining the laser along the appropriate meridian. Then, after a thorough vision test to confirm that the lens's prescription was exactly right, flooding the entire lens with UV light would make all the remaining free chains hold hands, locking in the adjustment.

But there's a catch—glassy polymers tend to be very rigid and slow-moving, which is why Plexiglas is stiff. This is no problem if you're writing a hologram on a credit-card sticker, because the holographic elements are less than a millionth of a meter wide. You can create a hologram in a few minutes, but it would take two years for the short chains to permeate across the central three to four millimeters of a lens implant made of the same material. Instead, the chains need to move about as fast as water diffuses through Jell-O—not a blinding speed, exactly, but fast enough to swim into place overnight. (A rigid Plexiglas implant would also require an incision the size of your own lens—about seven millimeters in diameter—that would take much longer to heal.)

"So we asked Dan, 'What polymers are approved for use in the eye?' And he said, "Well, polymethyl methacrylate, silicones, certain acrylics…' and my eyes just lit up when he said silicones. Silicones are some of wiggliest, jiggliest, fastest-moving molecules out there—they just might diffuse fast enough for this to work!" Silicones are made up of alternating atoms of silicon and oxygen, with various side chains dangling from the silicon atoms like charms on a bracelet. Silicones are also old friends to chemists, finding use in everything from lubricants and greases to bathtub caulk, baby-bottle nipples, Silly Putty, and—surprise!—



Left: The adjustable lens' ingredients. The light-sensitive initiator is a separate molecule that triggers the short chains' end groups to link up. Above: The plan for correcting a farsighted lens.

the current crop of flexible lens implants approved for cataract surgery. "So that's how far we got in the brainstorming session. And on the spot, Dan said, 'You've got money for a postdoc!'" It took the postdoc, Jagdish Jethmalani, two years to work through the details, but he came up with a polymer that he calculated could give 98 percent of cataract patients 20/20 vision. "So then Dan said, 'You've got money for a second postdoc! Let's get an optics guy in here and start making some lenses!'" So Kornfield and Grubbs recruited Christian Sandstedt to build a double-convex mold out of concave glass lenses sandwiched together, and they were off to the races.

Making the lenses was relatively easy, but getting them out of the mold wasn't. The silicone kept sticking to the glass, and the lenses refused to peel free once the polymer set. Kornfield was full of advisorly suggestions. "I said, 'In polymer processing, people coat the mold with Teflon spray. Why don't you try that?' They tried every idea I had. None of them worked. So finally one day, Jagdish was patiently waiting for his sister at a beauty parlor. He's the kind of guy who soaks up information from all kinds of things, and leafing through a copy of *Redbook* he saw this new nail polish called Teflon Tough. The ad raved about how smooth it was, and about its tough surface of *real Teflon*. So he ordered some, painted it on the mold, and we've been sailing ever since. We've never found anything that works better."

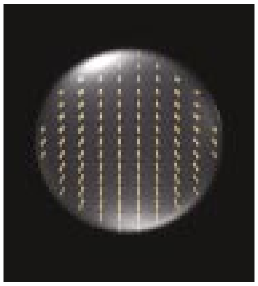
But the serendipity didn't end there. The very first batch of lenses to be treated with ultraviolet light became four times more powerful than they should have. Clearly, the refractive index wasn't the only thing that was changing. After some head-scratching, the chemists realized that making the short chains long enough to stay in the lens had had the unintended consequence of keeping them stretchy after the laser light linked them together. As the free chains shouldered their way into the laser-zapped area, the linked chains had enough "give" to move aside, causing the lens to bulge. (Previously reported photopolymers containing dispersed monomers had actually shrunk slightly.) The effect of the shape change far outweighed the refractive-index change, and is now the basis of the lens design.

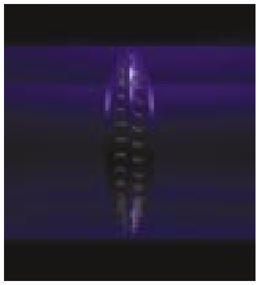
The postdocs assessed the lenses' optical quality by photographing a test pattern through them. If you look at a set of very thin and precisely ruled parallel lines, their uniformity and sharpness of focus will tell you how good the lens is, and their degree of magnification allows you to calculate the lens's power. If the power isn't uniform, the lines will be thicker in some spots than in others, and if the surface isn't perfectly smooth the lines will be grainy. The lines seen through the silicone lenses were crisp and clear. Furthermore, revisiting a batch of lenses left to sit for several days within inches of a fluorescent ceiling light showed that ambient light didn't spur the short chains into action, so a cataract patient could get adjusted one day and come back the next for a final test without ruining the unlocked lens. (But as with vampires, direct sunlight is to be avoided, especially during those two to four weeks it takes for the eye to heal before the adjustment. A good pair of UV-blocking sunglasses will suffice.) Other tests confirmed that the process of locking the changes in didn't itself further alter their power.

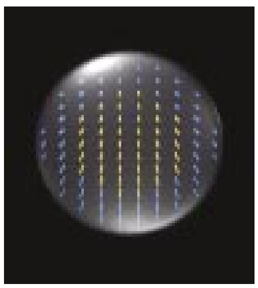
All this has led to the inevitable start-up company, Calhoun Vision, Inc., and the inevitable Web site, www.calhounvision.com. Schwartz is the chairman, and Sandstedt and Jethmalani are among those working on optics and materials research, respectively. The company has built a system to deliver an exact dose of ultraviolet light to a precise location within the eye, and has shown that the lenses respond to different doses in a very predictable manner and that, within batches of lenses zapped with the same dose, the variations are less than humans can perceive. The company plans to begin clinical trials this summer.

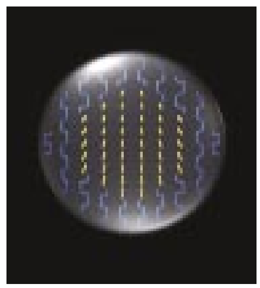
But why stop at cataract patients? Why not fix everyone's vision? Such eye surgeries are already big business, with the most popular method being LASIK, for LAser in-SItu Keratomileusis, which uses a laser to sculpt the cornea—the clear part of your eye in front of the lens, whose shape accounts for roughly two-thirds of the eye's focusing power. "LASIK is a very successful procedure," Kornfield says. "But it has a couple of drawbacks that basically trace back to the nonpredictability of wound healing." Furthermore, LASIK does not work reliably on extremely nearsighted or far-sighted people.

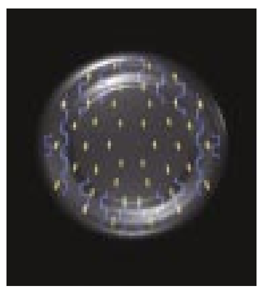
To touch up your vision with an implant, the eye's natural lens would be left in place and the implant inserted in front of it. Eye surgeons are already testing nonadjustable implants for this purpose, but are again running afoul of the vagaries of wound healing. So a laser-tweakable version would be the ultimate in extended-wear

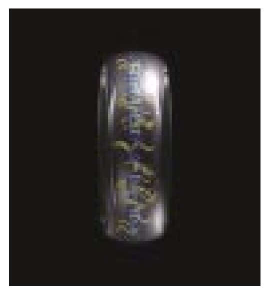


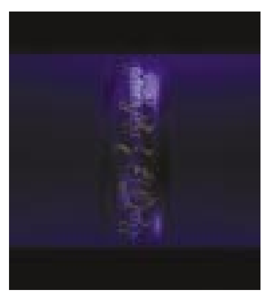


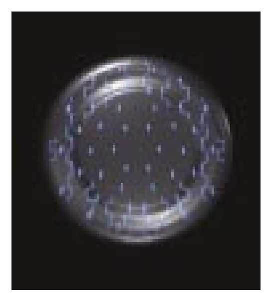


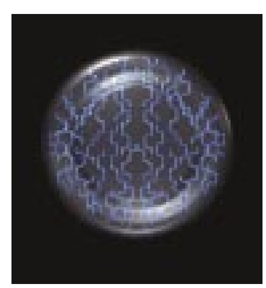












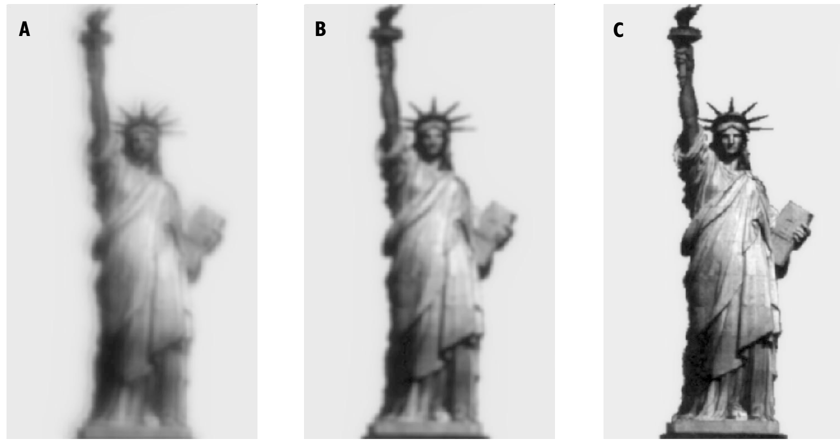
These frames from a Calhoun Vision video show how the nearsightedness correction actually works. The yellow squiggles are the short chains. On activation, they turn blue and link up.

contact lenses. For a while, anyway…. Somewhere around age 45, the eye's own lens loses its ability to bulge on command—or "accommodate," as the eye doctors say—which allows you to focus on nearby objects. "Another really neat breakthrough will be implanting the lenses in such a way that the muscles in your eye that perform accommodation can act on them," says Kornfield. "Perhaps it will be possible for us to enjoy near and far vision into old age."

The implant could even be set for "supernormal" vision. Adaptive optics, which astronomers use to take the twinkle out of starlight, employs a system of computer-controlled sensors and mirrors to compensate for changes in the atmosphere's refractive index—the same phenomenon that causes mirages to appear in the middle of the road on hot summer days. A postdoc at the University of Rochester, Donald Miller, with his advisor David Williams and colleagues Jun Zhong and G. Michael Morris (MS '76, PhD '79) adapted that notion to a microscope to give eye doctors the sharpest view yet of the retina. The researchers photographed individual photoreceptor cells in several patients, something that had never happened before because the human lens and cornea aren't precision optical instruments. At the same time, says Kornfield, "the patients looking back out through this system raved about how sharp and crisp their vision was." In theory, Miller says, "electronic spectacles" with adaptive-optics technology could achieve retinal image quality equivalent to 20/2.5 vision, or six times normal. At that point, however, the details being brought into focus are finer than the visual neural system can handle, so

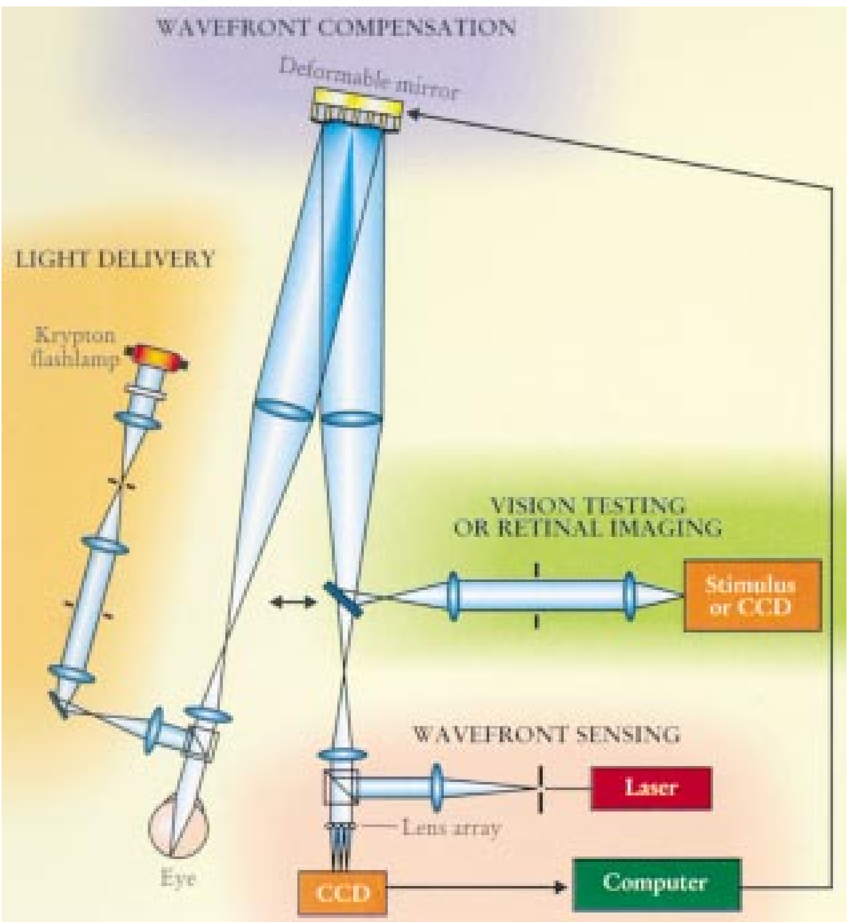
As with vampires, direct sunlight is to be avoided, especially during those two to four weeks it takes for the eye to heal before the adjustment. A good pair of UV-blocking sunglasses will suffice.

The Statue of Liberty, as seen by a person with normal vision standing on a boat three kilometers away, would look like photo A. An adaptive-optic lens that compensated for all the eye's imperfections would sharpen Lady Liberty to look like photo B. Dilating the pupil from its normal daylight diameter of three millimeters to its maximum diameter of eight millimeters would gather more light and sharpen her up even more, as in photo C. On the other hand, simply dilating the pupil without adaptive optics would actually make her fuzzier, as the effect of the imperfections increases with pupil size, which may be one reason why we squint when we're trying to make out highway signs while driving at night.



Donald T. Miller, "Retinal Imaging and Vision at the Frontiers of Adaptive Optics", *Physics Today*, January 2000, Vol. 53, No. 1, p. 36.

Donald T. Miller, "Retinal Imaging and Vision at the Frontiers of Adaptive Optics", *Physics Today*, January 2000, Vol. 53, No. 1, p. 32.



The adaptive-optic system used a Shack-Hartmann wavefront sensor. The light waves of a near-infrared laser (bottom right) reflecting off a point on the retina are distorted by the eye's imperfections. The returning beam passes through an array of tiny lenses that focus pieces of the beam onto a CCD camera. Local errors in the wavefront will move each lenslet's point of focus, allowing a computer to reconstruct what happened to the beam. Actuators behind the mirror then nudge it as needed to bring the wavefronts back into perfect alignment. (The krypton flashlamp is for photographing the retina.)

20/10 vision is a more realistic goal.

Kornfield's lab is also working on implants that can be done on a larger scale. Tissue transplants, for example. Cutting big holes in people is bad, so it would be nice to suspend "starter" cells of the tissue in a liquid polymer that, once injected into the body, would congeal in place. Then the solid would have to pull a slow-motion disappearing act, letting the trapped cells multiply and unite into a tissue. At this early stage in the game people are trying to figure out how to grow simple, undifferentiated tissues, but perhaps someday one could grow a new liver. Or at least a part of one. Meanwhile, back in the real world, a self-destructing scaffolding could act as a timed-release mechanism—for drugs that have to be given as daily shots, or perhaps for blood cells in people awaiting bone-marrow transplants. Or the plastic could simply act as a barrier—an internal bandage, or a support to keep something in place while it heals. Creating such a polymer is a tall order: the liquid would have to harden at the snap of a finger—on command, and so quickly that it can't seep into places you don't want it to go. And the solid would have to be sturdy enough to survive within the body, yet dissolve at a controlled rate.

Several approaches have been tried over the years, each with assorted shortcomings. You can inject short chains that photopolymerize, like the free chains in the lens implant. But it's pretty dark in the rest of the body, so it takes complex fiber-optic systems to get the light where you need it. Alternatively, there are thermosetting polymers that link up at body temperature. Of course, if the material turns solid when cooled to 98.6° F, then the liquid clearly has to be kept warmer. The coolest practical temperature is about 104° F—as high as you can safely set your hot tub—and a fire in the belly should be a literary metaphor, not a side effect of therapy. Furthermore, at least so far, all the solids that form this way aren't very strong

A hydrogel is a gel that likes water. Soaks it up like a sponge, in fact, swelling and getting soft. Which is good for an implant—who wants a stabbing pain every time they bend over?

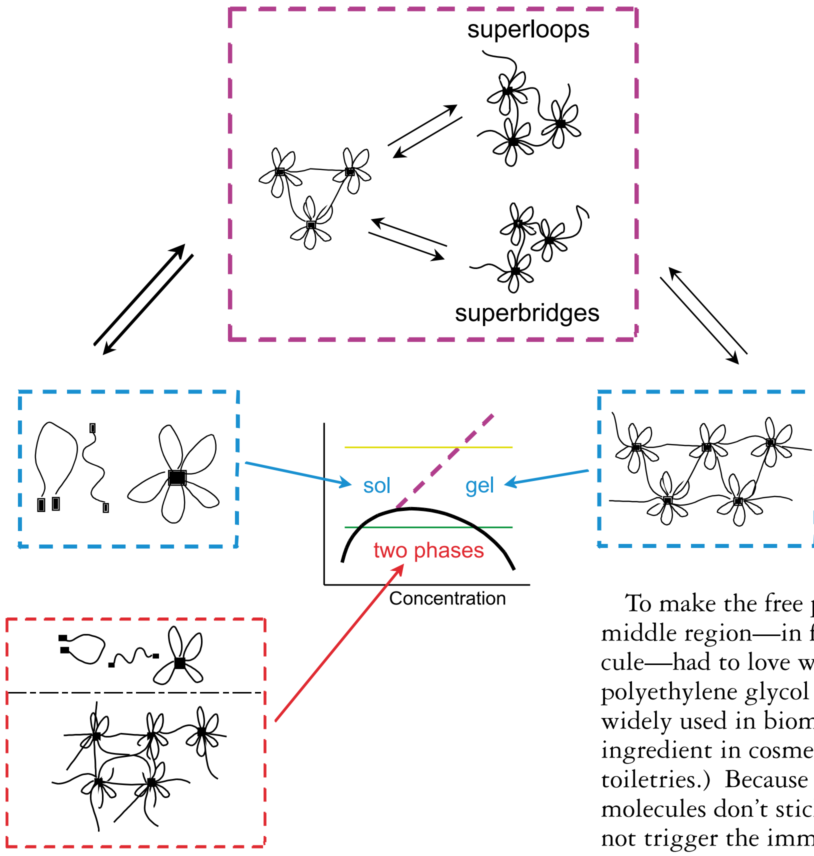
and tend to dissolve too quickly. Another notion is to dissolve the polymer in an organic solvent, and let the polymer precipitate out as the solvent diffuses away. The downside is that the ocean of solvent needed to dissolve the stuff in the first place causes problems of its own, ethyl acetate on your breath being the least of them. And you can't deliver timed-release cells or proteins this way, because the solvent kills the cells and prevents protein molecules from folding into their biologically active shapes. And finally, you can inject two precursor molecules into the body separately, and let them react on site. But the reactions aren't very selective, so you wind up with the internal equivalent of supergluing your fingers together.

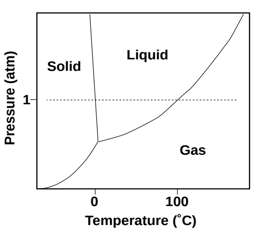
Tissue engineer Jeff Hubbell, then at Caltech and now at ETH, the Swiss Federal Institute of Technology in Zurich, had been thinking that a hydrogel (essentially waterlogged Jell-O) would be just the ticket. Gels, like silicones, are three-dimensional polymer networks, with plenty of room between the chains to fit protein molecules. And a hydrogel is a gel that likes water. Soaks it up like a sponge, in fact, swelling and getting soft. Which is good for an implant—who wants a stabbing pain every time they bend over? A hydrogel is usually more than 90 percent water, so that any embedded cells can easily absorb all the nutrients and other goodies they need. In fact, many tissues *are* hydrogels, including the cornea in your eye. So Hubbell asked Kornfield to design a hydrophilic, or water-loving, polymer that would spontaneously harden—if that's the right word for something so squishy—after injection, and dissolve at a controlled rate thereafter. Kornfield and Hubbell jointly enlisted a grad student, Giyoong Tae (PhD '02), to give it a try.

Hubbell, Kornfield, and Tae wanted a gel that dissolves the way a snowball melts, slowly sloughing off its outermost layer of molecules. That way, a fresh supply of the cells or drug within would be continuously exposed. This is called surface erosion. The trouble is, all the self-assembling hydrogels known when the project began eroded in bulk, dissolving from within, says Kornfield. "If I implanted a slab of one of those gels, it would swell, get softer, swell more, and just fall apart. And at some point, usually in hours or days, the stuff you were trying to release over time would all be dumped at once."

Melting snow is a phase transition between ice and water coexisting at the same temperature. Similarly, this hydrogel needed to make a phase transition between the solid gel and its dissolved state, which is called the sol, coexisting over a range of concentrations. (If you had a vial full of the stuff, you'd see a layer of gel at the bottom and the sol on the top.) So just as the temperature inside a melting snowball "hangs" at the melting point as the air temperature outside continues to rise, the concentration of the sol and the gel inside the lump of polymer remain at equilibrium even if that lump is drowning in enough water to dissolve it fully. The water within the polymer network is saturated with sol-phase molecules that are too big to swim away, preventing further dissolution. The bulk-eroding polymers, by contrast, never reach equilibrium—the material just swells and swells as the trapped water keeps dissolving more and more gel molecules, until suddenly the whole thing lets go.

In order to make the polymer molecules gel in the first place, they're endowed with water-hating, or hydrophobic, ends. Given half a chance, these ends—dozens of them—spontaneously cluster together, each one trying to put its fellows between the body's water molecules and itself. So Tae chose fluoroalkyls, which are notoriously hydrophobic, for the end groups. An ordinary alkyl is made of carbon and hydrogen—it's wax, basically, which is pretty water-repellent already. But replace the hydrogen atoms with fluorine, and you get a fluoroalkyl, like Teflon. And we've all seen water beading up on a nonstick frying pan as the Teflon coating shoves the drops away. As luck (or chemistry) would have it, fluoroalkyls are also more biocompatible than regular alkyls. "We think it's because when you replace the hydrogens with fluorine, you make a molecule that hates water *and* it hates regular alkyl molecules—oils and fats—as well," Kornfield explains. "So it tends not to go into cell membranes and the bloodstream the way that alkyl chains do, which makes the cells very unhappy." Altering the length of the fluoroalkyl would influence how strongly it would want to cluster, and thus how easy it would be to make the polymer gel. Neutron-scattering measurements that Tae did in collaboration with Jyotsana Lal at Argonne National Lab showed that the C_8 fluoroalkyls liked to cluster in bunches of roughly 30, while the bigger and more hydrophobic C_{10} groups preferred to huddle in crowds of 50 or so.





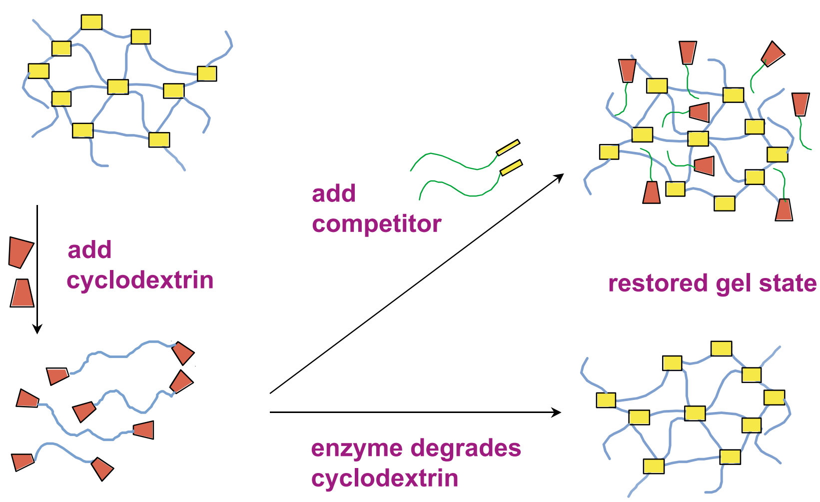
The phase diagram for water (left) shows its physical state at a given temperature and pressure. At constant pressure, say one atmosphere, changing the temperature is equivalent to moving horizontally (dotted line). A phase diagram for Tae's family of polymers (above, center) would be conceptually similar, depicting the transition from sol to gel as a function of concentration. The vertical axis has no label, because it represents a complex balancing act between the many competing forces that act on the molecule's water-loving middle and water-hating ends.

A typical bulk-dissolving polymer's behavior is shown by the yellow line. At low concentrations, the molecules tend to bite their own tails as the end groups cling to each other (blue box at left). The scattered flowerlike clusters that do manage to form aren't big enough to fall out of solution. As the concentration rises and it becomes easier for the fluoroalkyls to find one another, the molecules spontaneously begin to assemble themselves into larger structures (purple box). There's no clear-cut transition into a gel, but rather a continuum of coagulation, so the boundary between the sol and the gel is shown as a dashed (purple) line. By contrast, a two-phase system (green line) goes through a well-defined intermediate state (red box) in which the sol and the gel coexist.

To make the free polymer water-soluble, the middle region—in fact, almost all of the molecule—had to love water. For this, Tae chose polyethylene glycol (PEG), a simple molecule widely used in biomedicine. (It's also a popular ingredient in cosmetics, shampoos, and other toiletries.) Because PEG loves water, oily protein molecules don't stick to it. Therefore, PEG does not trigger the immune system nor does blood clot when exposed to it, making it an ideal coating for various kinds of implants. Controlling the length of the PEG region would govern how much water the gel could absorb, and how big a protein could be caught in the mesh.

So Tae made an assortment of materials, with some initial assistance from Thieo Hogen-Esch, a professor of chemistry at USC. The hydrophilic PEG region varied from about 140 to 460 monomers, and the hydrophobic fluoroalkyl groups ranged from six carbons (C_6F_{13}) to 10 carbons ($C_{10}F_{21}$) long. As it turned out, this spanned the entire behavior range, from bulk erosion to sol-gel coexistence, to complete insolubility. But it remained to be seen whether the gel in the two-phase material was really eroding from the surface, and if the erosion rate was slow enough to be useful.

To find out, Tae and Diethelm Johannsmann, at the Max Planck Institute for Polymer Research in Mainz, Germany, applied a coating of the gel to a very thin gold film and immersed it in running water. This experiment is a good example of the roundabout route one sometimes has to take to tease out the piece of data you're looking for. If the film is thin enough, a beam of light hitting it will actually interact with the atoms just beyond it. In other words, light reflecting off the top of the gold film will be "aware" of the gel coating on the underside. Not only that, but the light "sees" the gel-phase molecules to a depth of about one micron, or one millionth of a meter. If the coating is several microns thick, and the gel dissolves



How the uncapping was supposed to work. The yellow boxes are the fluoroalkyl end groups.

exclusively from the exposed surface, then the reflection won't change until the gel has worn down to that last micron, and the time lag until this occurs gives the disintegration rate. But if the stuff is dissolving throughout, the light will see fewer and fewer molecules in the gel state, and the reflection—specifically, its strength as a function of the angle at which the light hits the surface—will also change continuously. The effect is most pronounced at angles near the one at which the light excites the gold's so-called plasmon mode, so the method is known as surface plasmon resonance.

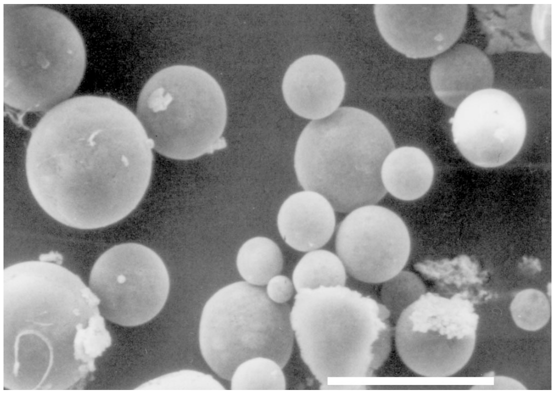
The tests confirmed that the gel was eroding from the surface, and further revealed that the rate was adjustable. Polymers with identical eight-carbon fluoroalkyl groups dissolved at rates controlled by the lengths of their PEG middles, since having more water-loving PEG made the molecule go into solution faster. But for a fixed PEG length, making the fluoroalkyl groups just a little bit longer slowed the erosion rate over a hundredfold. "Imagine each cluster of fluoroalkyl groups is a centipede, and the polymer molecules coming out from it are the legs," says Kornfield. "A cluster of C_8 end groups has 30 legs, and a C_{10} cluster has 50 legs. They all have to let go at once in order for the centipede to wash away. It's really cool, because one of the main parameters that people would love to dial in for use in the body is the erosion rate." By using the fluoroalkyl length and the PEG length as the coarse- and fine-adjustment knobs, respectively, you can tune the time-release setting from days to weeks.

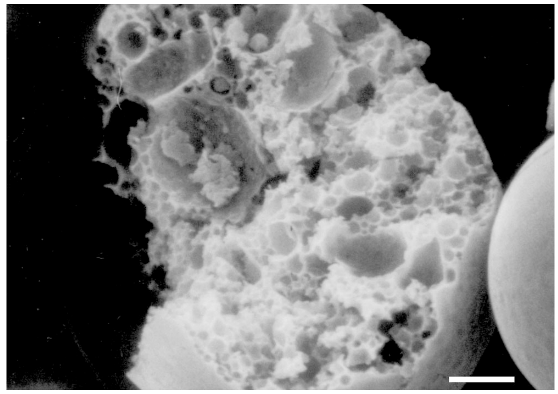
At this point, you may be wondering: if the polymer is concentrated enough to gel in the body, what keeps the sol from gelling in the hypodermic? A good question—that was the final hurdle. "We tried a few ideas that looked really great on paper, but just didn't work out," says Kornfield. "We all pursue dead ends. That's part of science." For example, they tried capping each fluoroalkyl with a flowerpot-shaped molecule called a beta-cyclodextrin. Cyclodextrins are made up of sugar molecules; the beta means there are seven sugars per flowerpot. The sugars are arranged so that the cyclodextrin's outer surface loves water, but the inner surface hates it. The fluoroalkyl can hide inside the flowerpot, which is so cozy that it would rather do that than snuggle with its fellows. This worked really well, and the concentrated polymer dissolved nicely. In fact, it worked a little too well—like grown-ups wrestling with a child-proof bottle, the chemists couldn't get the cap off fast enough. "We tried to use an enzyme to degrade it. What could be better? Maybe it would even respond to enzymes in the body—how cool! Well, we learned that the human body doesn't make any enzyme that's really good at degrading cyclodextrins, and the best ones we could find come from a fungus. But you can't put fungus enzymes into a person without an immune response, which is bad. And even the best enzyme we could find wasn't fast enough." Then they tried to use a competitor molecule—a length of PEG with only one fluoroalkyl end group—to pry the cap off. Just as you would wedge a butter knife under the childproof top to pop it free, a polymer with a C_{10} end should displace a polymer with a C_8 end, because the bigger the fluoroalkyl, the better it likes to climb into the flowerpot. Unfortunately, the competitor molecules were too big to diffuse very fast, so this idea only worked in a test tube, where they could be stirred into the sol with some vigor.

"Giyoong tried all sorts of molecules, and it was very frustrating. Then one day he came into my office and he said, 'Did you know, Julie, that there are organic solvents that are already approved for use in the body?' I said, 'No. You've got to be kidding!' In fact, I probably said, 'That's gross!' But it's true, and it turns out that one of them, called *N*-methyl pyrrolidone, dissolves both the middle *and* the ends of our polymer. Giyoong showed that you could make a solution that's 50 percent polymer and 50 percent solvent and still be runny enough to be injected through a syringe." Furthermore, when exposed to water—or intercellular fluid—the solvent diffuses away within minutes, leaving behind a gel that behaves as if the solvent had never been there. If the solvent molecules had hung around the fluoroalkyl ends, they wouldn't have clustered properly and the polymer wouldn't gel well.

With a workable system in hand, it was time to try it out. Hubbell, Kornfield, and Tae opted to attempt a controlled release of human growth hormone, also known as somatotropin. This protein, made up of 191 amino acids, is released by the pituitary gland. Children lacking the hormone grow less than two inches per year, and in normal quantities the hormone helps kids to grow at the rate of two to three inches per year. Children in the shortest 5 percent of the height

H.K. Kim and T. G. Park, "Microencapsulation of Human Growth Hormone within Biodegradable Polyester Microspheres: Protein Aggregation Stability and Incomplete Release Mechanism," *Biotechnology and Bioengineering*, Vol. 65, No. 6, December 20, 1999, p. 662.





The surface (left) and a cross section through the interior (right) of the glycolic acid microspheres. The scale bar in the left image is 0.1 millimeters; the one at right is 0.01 millimeters.

range for their age and sex are frequently given hormone treatments to help them catch up. This means three-times-a-week or even daily hormone shots, administered year after never-ending year. The shots can be given at home, but it's clearly no fun for the little ones. And even the older kids who have gotten used to needles and are doing their own injections would rather do it just once a month. Which is as infrequent as is practical, because proteins—even in gel storage—can be degraded by hydrolysis or attacked by enzymes called proteases. By the month's end, you can't be sure the protein's any good any more, and it's best to start fresh again.

Human growth hormone presents some particular problems as a timed-release candidate. The pituitary gland stores the stuff in granules, in which pairs of the protein molecule are held together by two zinc ions that stabilize the protein's active form—without the zinc, the molecules aggregate into useless clumps. So the trick is to store the stuff bound to the zinc ions so it won't clump up, and then release it one pair at a time. Many people have been working on this problem, but the only approach that has been approved for clinical use to date encapsulates the zinc-protein dimer inside biodegradable particles made of a molecule called poly(D,L-lactic-*co*-glycolic acid). There are several variations on this theme, but they all suffer from an "initial burst"—as much as half of the hormone escapes in the first day after injection, before the release rate stabilizes. This royally screws up the dosage calculations, and the kids wind up getting less of the hormone than they should. Not surprisingly, children on whom this method was tried did not grow as fast as those who were given the daily injections. And there's a side effect—the biodegradation products of poly(D,L-lactic-*co*-glycolic acid) are, well, acidic, which can lead to a painful inflammation at the injection site.

Tae demonstrated that, at least in a test tube, the hydrogel released the hormone at a nice, even rate with no initial burst. The release could be sustained for two to four weeks, depending on the polymer chosen. And a veterinarian in Hubbell's lab at the ETH injected some into mice to verify that it was biocompatible. Says Kornfield, "Three days later, there was a beautiful, clear, spherical gel under the mouse's skin, and very little inflammation. In fact, the vet said it was quite remarkable how little inflammation there was." However, lots more work remains to be done before these polymers will be ready for clinical trials in humans. But PEG is widely used to stabilize the active forms of various other proteins that are given by injection, so if the trials go well, lots of other applications await.

And goo may be good for you in any number of other ways. Kornfield's lab, and those of a host of other researchers, are just beginning to explore the possibilities of biocompatible polymers. So if your doctor ever tells you that you're in line for an implant, you may wind up with something that owes less to the exotic alloys of the aircraft industry than it does to fifty cents' worth of Jell-O. □

Professor of Chemical Engineering Julia A. Kornfield (BS '83, MS '84) is also the director of the National Science Foundation's Center for the Science and Engineering of Materials, located at Caltech, which pioneers exotic and futuristic materials. A 1981 Summer Undergraduate Reseach Fellowship (SURF) project involving nerve cells got her hooked on biotechnology, to which her polymer-physics background lends a different perspective. Besides her Caltech degrees, she earned a PhD in chemical engineering from Stanford in 1988. She has been on the Caltech faculty since 1990, earning three teaching awards in that time.

The lens and gel halves of this article were adapted from a Watson lecture given last March and a SURF brown-bag lunch presentation last August, respectively.

PICTURE CREDITS:
30, 31, 32, 37 – Doug Cummings; 30 – National Library of Medicine; 32, 33, 38 – Julia Kornfield

Richard D. McKelvey
1944 – 2002

Richard D. McKelvey, the Edie and Lew Wasserman Professor of Political Science, died April 22 of cancer. He was 57.

McKelvey was best known for his leading role in the development of mathematical theories of voting but also made fundamental contributions to game theory, social-choice theory, experimental political science, and computational economics. Director of the William D. Hacker Social Science Experimental Laboratory, he was also a pioneer in the use of laboratory experiments to test theories of voting and other group behavior and in the application of computational techniques to understanding strategic behavior. Some of his experimental work investigated the effects of different voting rules on the accuracy of jury verdicts, the effect of polls on election outcomes, and impasses in negotiations and bargaining. In one celebrated paper, McKelvey showed that decisions made under one-person/one-vote, majority-rule democratic systems do not necessarily cluster around "middle-ground" policy out-comes, but are sensitive to details of the process, such as who controls the agenda.

After graduating from Oberlin College in 1966 with a bachelor's degree in mathematics, McKelvey earned a master's in mathematics from Washington University in St. Louis (1967), and a master's (1970) and doctorate (1972) in political science from the University of Rochester. He joined the faculty at Rochester and then at Carnegie Mellon before visiting Caltech as a Sherman Fairchild Distinguished Scholar in 1978. He stayed on as full professor starting the following year and was named the Wasserman professor in 1998.

McKelvey was elected to the National Academy of Sciences in 1993 and to the American Academy of Arts and Sciences in 1992; he was also a fellow of the Econometric Society.

On June 8, his colleagues and former students gathered in Dabney Lounge for a memorial service to celebrate McKelvey's life. "He was an unselfish coauthor and totally unselfish with his students," said John Ledyard, professor of economics and social sciences and outgoing chair of the Division of the Humanities and Social Sciences. "A lot of Richard was poured into his students," he said. So it was appropriate that the opening speakers were McKelvey's first student, John Aldrich, and his last, Elizabeth (Maggie) Penn.

Aldrich, now the Pfizer-Pratt Professor of Political Science at Duke University, met McKelvey at Rochester, where they were both graduate students. "He was a co-grad student with me as well as my dissertation adviser," he explained. "While he was the most important political scientist that I've had the good fortune to know, it's also the case that in many ways he was the best teacher. I don't think he actually knew that he was teaching all the time; it's just that he was inherently the finest teacher. I'm pleased and honored to have been his first graduate student."

"Working with Richard was the best part of graduate school for me," said Penn, currently a student at Caltech. "Even in his last few days he was still incredibly giving of his time and his ideas. The week before he died, he was at school almost every day, and three days before he died, he came to a seminar that I gave for the department. I think it just shows how dedicated he was to all of his students. I don't think there's a person in the world that I respect and admire more than I respect him," she said.

Another former student from Rochester was Keith Poole, now the Kenneth L. Lay Professor of Political Science at the University of Houston. "Dick was my teacher, my supervisor, and my friend," said Poole. "More than any other person, he shaped my intellectual life as an academic." But Poole added that when he thought of him, he would not think of the "McKelvey, Richard D." of the citations in the scientific papers. "*That* McKelvey's work will last for generations. I think of the *person* I knew—a great guy, modest, unpretentious, and generous, whose company I so enjoyed."

"I wasn't officially a student of his," said Tom Palfrey, professor of economics and political science at Caltech, "but he taught me a great deal about how to think, about being cautious and skeptical about my own thinking, even in the exciting moments of discovery. And he tried to teach me, and to teach us all by his own example, to be modest in our claims and generous with our ideas. He's someone I admired for his scholarship, his integrity, his humility, and his general decency."

Palfrey spoke of McKelvey's scholarship and things that fascinated him, such as the Nash Equilibrium ("he believed deeply that this was a fundamental principle underlying human interactive behavior"), which underlay their work together over the past 15 years, particularly on a general statistical theory of games called quantal response equilibrium. He noted that McKelvey's celebrated computer program, Gambit, which finds numerical approximations of solutions to games, also followed from that fascination.

"He really wanted to figure out how something worked in a very detailed, algorithmic, almost mechanical way," said Palfrey. "He had to have a deep, almost physical sense of the model. This may seem odd to someone who saw Richard as esoteric and theoretical—a guy who wrote papers that were mired in notation, in complex mathematical argument, and who lectured to the board as he wrote down all this notation. But even in complicated proofs he was building things."

Norman Schofield, the William Taussig Professor of Political Economy at Washington University in St. Louis, worked with McKelvey on cooperative game theory. He described his impression, as a visitor from England, of this field of mathematical political science in America as being "like a great family," with various branches in Rochester, St. Louis, and Caltech. "I met most of the branches of this family, and I was surprised how generous and interested in this snotty little Brit the members of this family were. Dick in particular was really interesting and helpful." They collaborated for several years "and then Dick very generously arranged for me to come to Caltech in 1983. For me, this was probably the best two years of my life." Schofield felt that he had been "sort of adopted into the family, as a brother in a sense."

Another Caltech colleague, Peter Ordeshook, professor of political science, declined to detail McKelvey's contribution to his career. "Dick *was* my career. Up until 1991 a third of my *vita* was Dick McKelvey," he said. "Working with Dick was challenging, humbling, exhausting, stimulating, etc., but it was also fun—an enormous amount of fun."

Ordeshook also described some of McKelvey's less academic pursuits, such as "one of the world's largest collections of credit cards." "You all thought he went to those conventions for intellectual reasons," he said, but actually McKelvey was using every new venue as an excuse to apply for credit cards at every department store in town. And he was also the inventor of a little device "that would count the number of people who came into the room and the number of people who went out, so that the light was always on when there was somebody in the room."

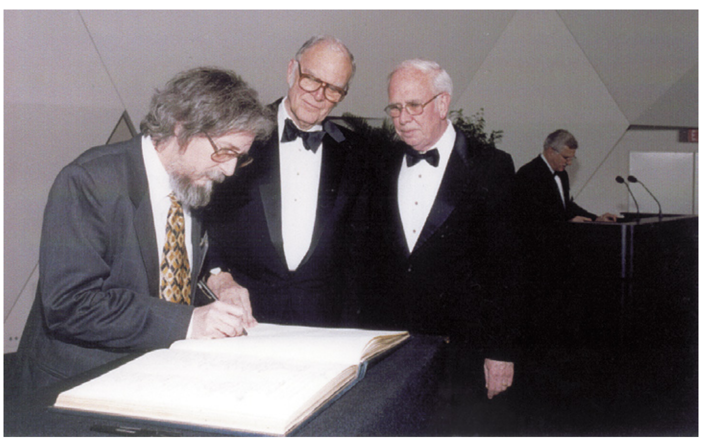
Ledyard added that "to me Richard was the epitome of what a Caltech professor really ought to be. He was committed to discovery. He never did anything because he thought it would make him famous; he did it because he was trying to find something

McKelvey and daughter, Holly, at Green Creek campground in the eastern Sierra Nevada two years ago.

out. And he found out a lot of really neat stuff. He didn't believe he could change the way things were; it was his job to figure out *how* things were—the true scientist, in a sense." McKelvey was responsible, Ledyard said, for convincing him to change his mind and come to Caltech after he had already turned down the job.

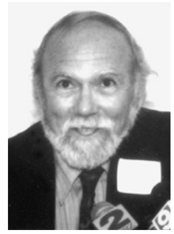
McKelvey's most recent work will play out posthumously. He initiated a contest called a Turing Tournament, designed to improve the ability to predict how people will behave in strategic situations, and this summer, leading scholars in the fields of economics and game theory will compete in the tournament for a cash prize, to be awarded to the theory that best matches actual human behavior in experimental situations.

McKelvey is survived by his wife, Stephenie Frederick, and three children, Kirk, Christopher, and Holly. At the end of the memorial service, Frederick thanked everyone for coming and invited all back to their house, where the credit card collection and the light switch system would be on display ("he didn't want to take the time to look for the light switch and switch it on and off, and so he spent hours, *years*, working on this system"). She also thanked "all of you in academia for creating a world that Richard could love so much." And after reciting, with mock resentment, a litany of household disasters over the years that McKelvey had managed to evade because "he was with a graduate student—maybe one of you," Frederick said that she wanted to do something "to honor Richard's dedication to his students." So the Richard D. McKelvey Prize Fellowship has been established, to be awarded annually to a student doing superior work in social sciences. She gave Ledyard a check for $5,000 toward the fellowship. "This is from Richard and me," she said. □



McKelvey signs the book of members of the National Academy of Sciences in 1993.

Faculty File



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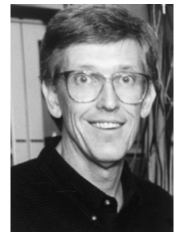

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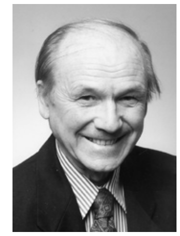


Roshko

Four Elected to NAS

Four Caltech professors were elected to the prestigious National Academy of Sciences in April: Barry Barish, the Linde Professor of Physics and director of the Laser Interferometer Gravitational-Wave Observatory (LIGO), an experimental high-energy physicist; Jacqueline Barton, the Hanisch Memorial Professor and professor of chemistry, who has pioneered the application of transition metal complexes as tools to probe recognition and reactions of double-helical DNA; H. Jeff Kimble, the Valentine Professor and professor of physics, an expert in quantum optics, who has made groundbreaking discoveries relating to quantum measurement and to the new science of quantum information; and Anatol Roshko, the Von Kármán Professor of Aeronautics, Emeritus, known for his research in several areas of gas dynamics and fluid mechanics.

This brings to 67 the number of living Caltech professors and emeritus professors who have earned this honor. □

And Five Elected to AAAS

Five members of the Caltech faculty have been elected to the American Academy of Arts and Sciences, joining the 177 Fellows and 30 Foreign Honorary Members in the academy's "class of 2002." They are: Richard Andersen, the Boswell Professor of Neuroscience, whose work focuses on neural mechanisms for visual-motor integration, spatial perception, and visual-motion analysis; David Anderson, professor of biology, as well as an investigator with the Howard Hughes Medical Institute (HHMI), whose main areas of investigation include the development of the nervous system, the development of the circulatory system, and the functional neuroanatomy of fear; Ronald Drever, professor of physics, whose research interests include experimental gravitation and the detection of gravitational waves; Mary Kennedy, the Davis Professor of Biology, who studies how brains store new information; and Mark

Wise, the McCone Professor of High Energy Physics, whose interests include particle physics, nuclear physics, and cosmology—and finance. Their election brings to 80 the number of Caltech faculty who are Fellows of the academy. □

Tech Review's Top 100

Michelle Effros, associate professor of electrical engineering, Stephen Quake, associate professor of applied physics, and three Caltech PhDs, Howie Choset, Kelvin Lee, and Suzie Hwang Pun, have been named to the TR100, the world's top 100 young innovators according to *Technology Review* magazine, published by the Massachusetts Institute of Technology.

Effros, who is director of Caltech's data-compression lab, conducts research on information compression and communication, with applications to the World Wide Web, signal processing, wireless communications, Internet and wireless networks, data storage devices, and speech recognition. Quake's work involves biophysics and microfluidic devices. He uses biological molecules as model systems for studying physics, and his work in microfluidics has led to the development of "lab-on-a-chip" devices.

Choset, who received his PhD in mechanical engineering in 1996 and is now an associate professor at Carnegie Mellon, builds "snakebots," highly articulated robots designed for complex exploration tasks. Lee, PhD '95 in chemical engineering and an assistant professor at Cornell, discovered a marker protein for identifying Creutzfeldt-Jakob disease in humans and "mad cow disease." And Pun, PhD '01 in chemical engineering, is a senior scientist at Insert Therapeutics, a company founded to exploit her work on using polymers, rather than viruses, to carry injected genes through the bloodstream to precise locations.

The theme for the 2002 TR100 selection has been the transformation of existing industries and the creation of new ones, particularly in "hot spots" such as information technology, biotechnology and medicine, nanotechnology and materials, energy, and transportation. □

More Honors and Awards

The 2002 ASCIT (Associated Students of Caltech) Teaching Awards were given to James Arvo, associate professor of computer science; Niles Pierce, assistant professor of applied and computational mathematics; Darryl Yong, von Kármán Instructor in Applied and Computational Mathematics; Vladimir Baranovsky, the Olga Taussky and John Todd Instructor in Mathematics; John Preskill, the MacArthur Professor of Theoretical Physics; and John Sutherland, visiting professor of literature. A lifetime teaching award was presented to Michael Shumate for his years as lecturer in applied physics.

The Graduate Student Council conferred its teaching awards on Oscar Bruno, professor of applied and computational mathematics; and Yaser Abu-Mostafa, professor of electrical engineering and computer science. Bruno also received a mentoring award.

Seymour Benzer, Boswell Professor of Neuroscience, Emeritus, has been chosen to receive this year's March of Dimes Prize in Developmental Biology. He is being honored "for research that addressed many of the mysteries of human biology and contributed to the design of new treatments for birth defects and other disorders." The prize's cash award of $250,000 will be shared equally by Benzer and his corecipient, Sydney Brenner, Distinguished Professor at the Salk Institute.

Christopher Brennen, professor of mechanical engineering and former vice president for student affairs, has received the Fluids Engineering Award, the highest award given by the Fluids Engineering Division of the Japan Society of Mechanical Engineers. It has never before been awarded to a non-Japanese. Brennen will deliver a lecture and accept the award in Tokyo in September.

William Deverell, associate professor of history, will serve as the 2002–03 Haynes Fellow beginning July 1. An authority on the West, he has written extensively about the history of California and Los Angeles. The oldest private foundation in the city of Los Angeles, the Haynes Foundation has been supporting social science research into regional policy issues since 1926.

Jim Eisenstein, professor of physics, has been invited to present a series of Morris Loeb Lectures at Harvard next winter. These lectureships, dealing with research topics of special interest to the lecturers, usually involve

John Todd and Olga Taussky Todd (painting by Sylvia Posner).

talks for both specialized and less-specialized audiences.

Charles Elachi, Caltech vice president, director of the Jet Propulsion Laboratory, and lecturer in electrical engineering and planetary science, has been elected a fellow of the American Institute of Aeronautics and Astronautics "for his leadership and contributions in the field of space-borne imaging radars." He has also received the Wernher Von Braun Award from the German Organization of Air and Space Travel, given in recognition of the Shuttle Radar Topography Mission team (*E&S* No. 1, 2002), and has been named the 2002 Distinguished Alumnus of UCLA's department of earth and space science.

Michael Hoffmann, the Irvine Professor of Environmental Science, was honored as the Dodge Distinguished Lecturer in Chemical Engineering at Yale in April.

Alexander Kechris, professor of mathematics, has won a 2002 John Simon Guggenheim Memorial Foundation Fellowship; the award will support his work in "classification problems in mathematics, group actions, and equivalence relations." Guggenheim Fellows "are appointed on the basis of distinguished achievement in the past and exceptional promise for future accomplishment."

Shrinivas Kulkarni, the MacArthur Professor of Astronomy and Planetary Science, has been chosen as the 2002 Jansky Lecturer. Established in 1966 by the trustees of Associated Universities, Inc., the Karl G. Jansky Lectureship recognizes outstanding contributions to the advancement of astronomy.

David MacMillan, associate professor of chemistry, has been selected to receive a Sloan Research Fellowship. Fellows are chosen by the Alfred P. Sloan Foundation "from among hundreds of highly qualified scientists in the early stages of their careers on the basis of their exceptional promise to contribute to the advancement of knowledge."

Dianne Newman, the Luce Assistant Professor of Geobiology and Environmental Science and Engineering, has been selected by the Department of the Navy as a recipient of the Office of Naval Research Young Investigator Award. The program "is designed to attract young scientists and engineers who show exceptional promise for outstanding research and teaching careers."

Michael Ortiz, professor of aeronautics and mechanical engineering, has been selected to receive a Humboldt Research Award for Senior U.S. Scientists. The Alexander von Humboldt Foundation of Germany "grants up to 150 Humboldt Research Awards annually to foreign scholars with internationally recognized academic qualifications. The award is intended as a lifelong tribute to the past academic accomplishments of award winners." Ortiz's award is in the amount of 65,000 euros.

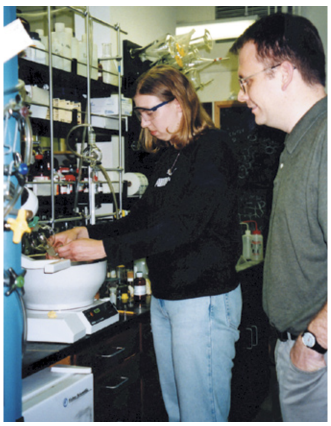
Jonas Peters, assistant professor of chemistry, has received a 2002 Camille Dreyfus Teacher-Scholar Award from the Camille and Henry Dreyfus Foundation. Only 15 Teacher-Scholars were chosen. The program "is designed to provide external support to young faculty members at early stages of their academic careers. It is the Foundation's expectation that this award will assist these outstanding scientists to continue the high level of accomplishment in education and research that they have demonstrated thus far." The award to Peters is for $60,000.

John Todd, professor of mathematics, emeritus, and his late wife, Olga Taussky Todd, who was also professor of mathematics, emeritus, have been selected to have their pictures displayed in the Portrait Gallery of Distinguished NBS/NIST Alumni. The gallery honors staff members and research associates of the National Bureau of Standards—now the National Institute of Standards and Technology—from 1901 to the present.

Theodore Y. Wu, professor of engineering science, emeritus, has been elected a Foreign Member of the Chinese Academy of Sciences, for his distinguished contributions to fluid mechanics and for his international academic collaboration. His election brings the foreign membership of the Chinese Academy to 41. □

Caltech's chemistry faculty in 1950. Zechmeister (white jacket) and Pauling are seated in the middle of the second row.

Student Support: A Lasting Legacy



Chemistry graduate student Sarah Spessard receives support from the Zechmeister Fund. Working with her advisor, Brian Stoltz, she has synthesized the core structure of garsubellin A, a molecule that may prove useful in the treatment of Alzheimer's disease.

László Zechmeister arrived in 1940 at the invitation of Linus Pauling, who was looking for an internationally known senior organic chemist. Crellin Laboratory had just been finished; it was a good time to come to Caltech. And it was a good time to get out of Hungary, where Zechmeister was professor of chemistry and director of the chemical institute at the medical school of the University of Pécs.

Born in Hungary in 1889, Zechmeister received his Dr Ing degree from the Federal Institute of Technology (ETH) in Zurich, where he was a student of Nobel laureate Richard Willstätter. As an Austro-Hungarian officer in World War I, he spent two years as a Russian prisoner of war, a time he put to good use teaching himself English from a Russian-English dictionary.

Zechmeister was an expert in chromatography, a technique that he used to purify organic molecules and study their structure, in particular that of carotenes (also complex sugars, vitamin A, natural plant pigments, and natural fluorescent compounds). He was a pioneer in chromotography, which he helped introduce in the United States, and in spectroscopy. He remained on the Caltech faculty as professor of organic chemistry until taking emeritus status in 1959.

Zechmeister had a wide influence on international science through *Progress in the Chemistry of Organic Natural Products*, a trilingual review journal which he founded in 1938 and continued to edit until 1969. He often referred to his second wife, Elizabeth (Lilly), whom he married in 1949, as his associate in this work, because of her assistance in editing and translating.

"Zecky" died in 1972. In memory of her husband, Lilly Zechmeister established an endowment fund at Caltech to be used for tuition grants or other financial assistance to graduate students in chemistry. Initial funding for the László and Elizabeth Zechmeister Memorial Fund was provided by gifts from Zechmeister's friends and colleagues. Jack Roberts, then acting division head, noted that by establishing this fund, "László's interests and work will be perpetuated along with his name." The fund was ultimately endowed with the proceeds of Lilly's charitable trusts, upon her death in 1995. □

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