California Institute
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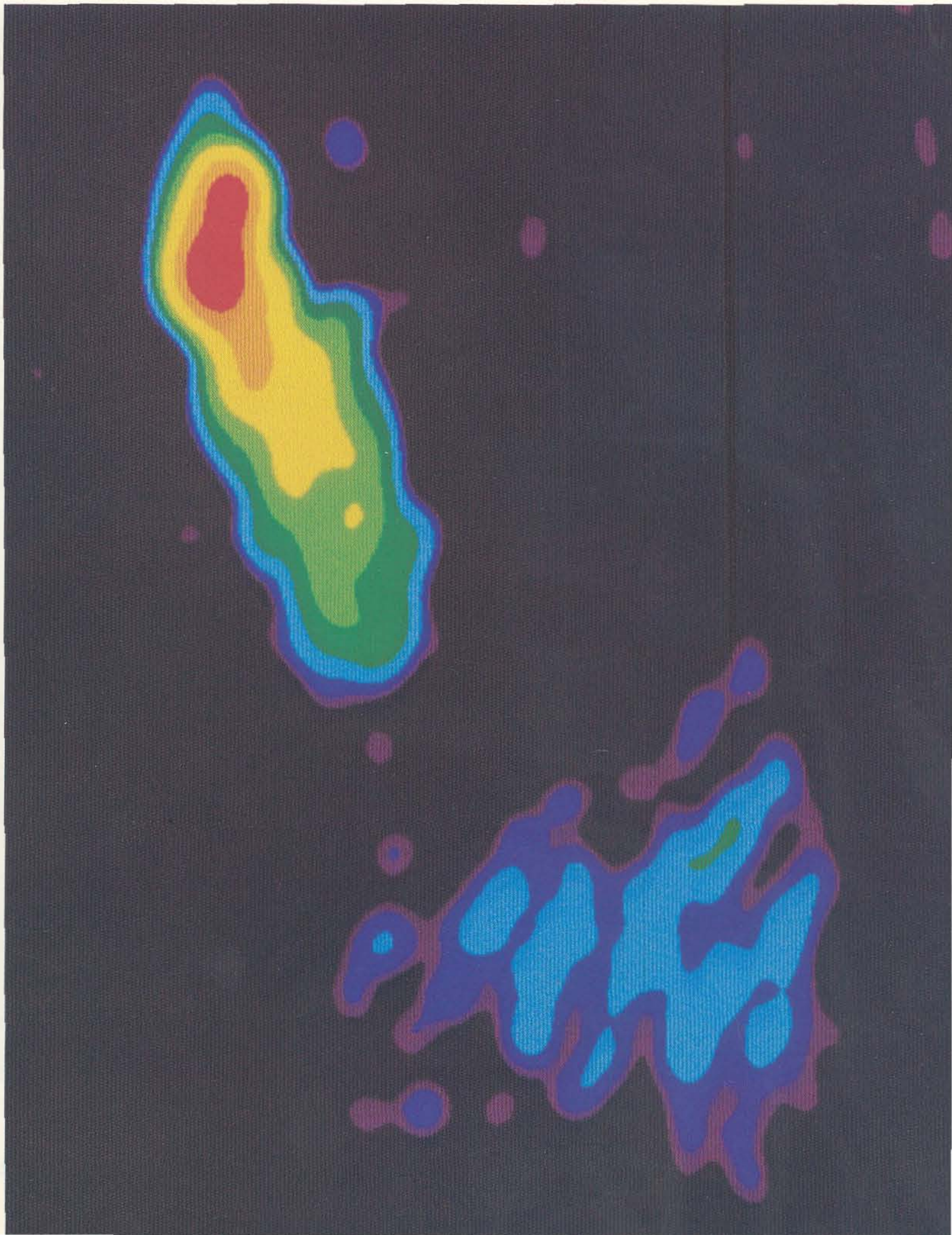
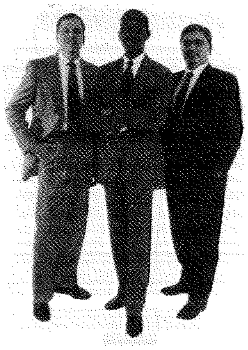
Engineering & Science

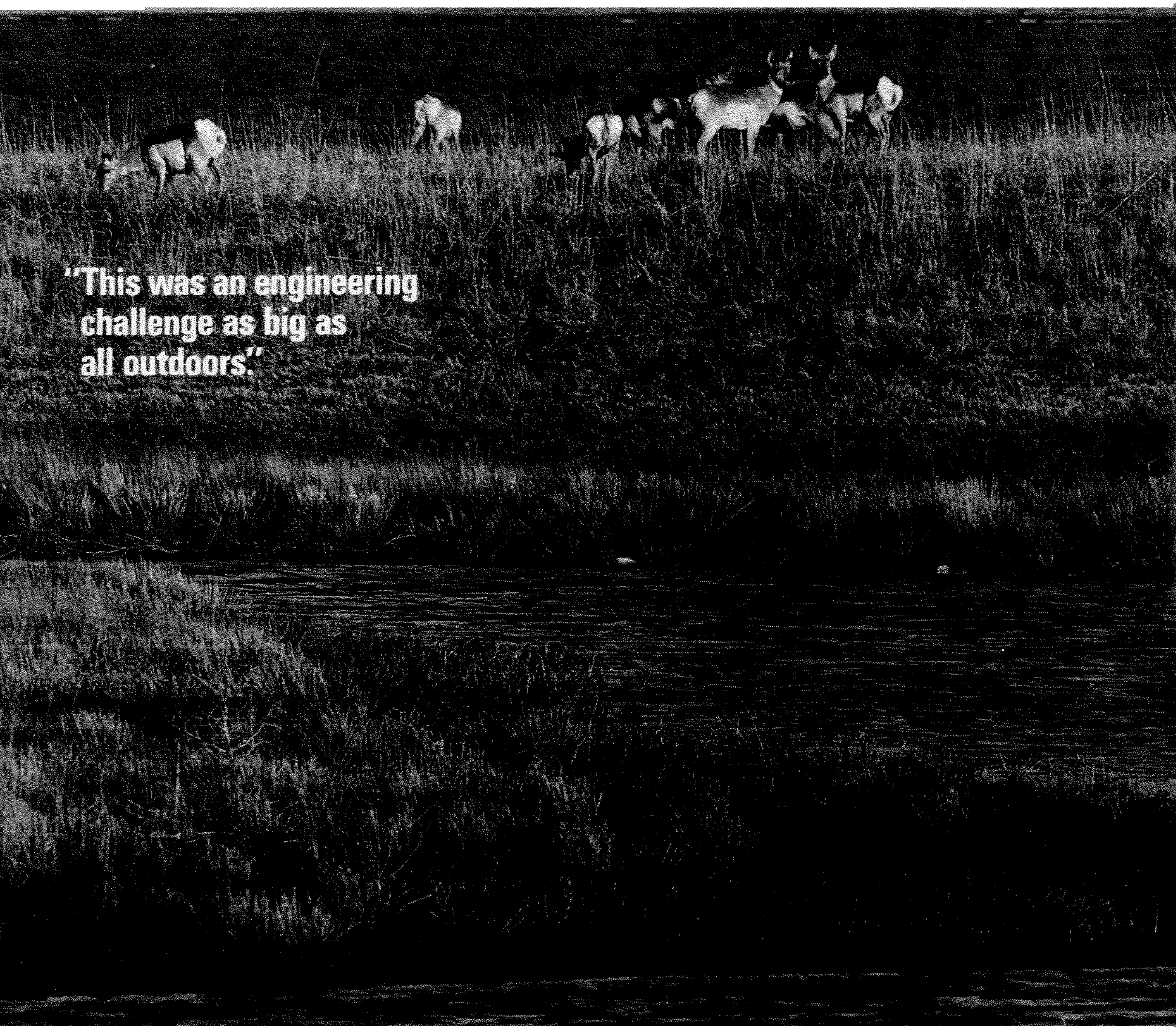
Fall 1991



In this issue



Not long ago, the EPA tightened waste water guidelines for benzene emissions, and asked for fast results. Amoco not only met the challenge, but went beyond. We developed technology to purify both water and air streams at our Casper, Wyoming plant. All in four short months, thanks to people like LaRence Snowden. **A** young mechanical engineer, LaRence coordinated a round-the-clock, interdisciplinary effort. He helped us develop and install large-scale equipment that typically takes a year to put in place. Giant air strippers, off-gas treaters, a customized control trailer, and several thousand feet of pipes and pumps. **A**s you can see, it truly was a challenge big as all outdoors. Especially since our Casper plant supplies water to Soda Lake, an Audubon Society preserve teeming with wildlife. **I**f you'd like to do something for the outdoors, the community, or even the world at large, talk to the people at Amoco.



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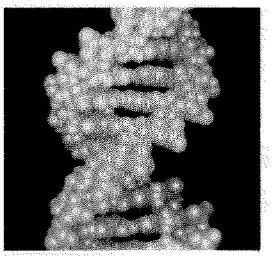

On the cover: A black hole? This false-color radio image shows the quasar 3C 345, more than 6 billion light years away, which is thought to harbor a massive black hole at its center (red). Stephen Unwin, a member of the professional staff, constructed this image using very-long-baseline-interferometry data from a worldwide network of radio telescopes. The different colors, representing the intensity of emission, indicate the end result of the black hole's gravitational pull—a jet of radio-emitting plasma streaming out of it at more than 98 percent of the speed of light. The nature of black holes and their possible role in the Big Crunch are discussed by Stephen Hawking in "The Future of the Universe," which begins on page 12.

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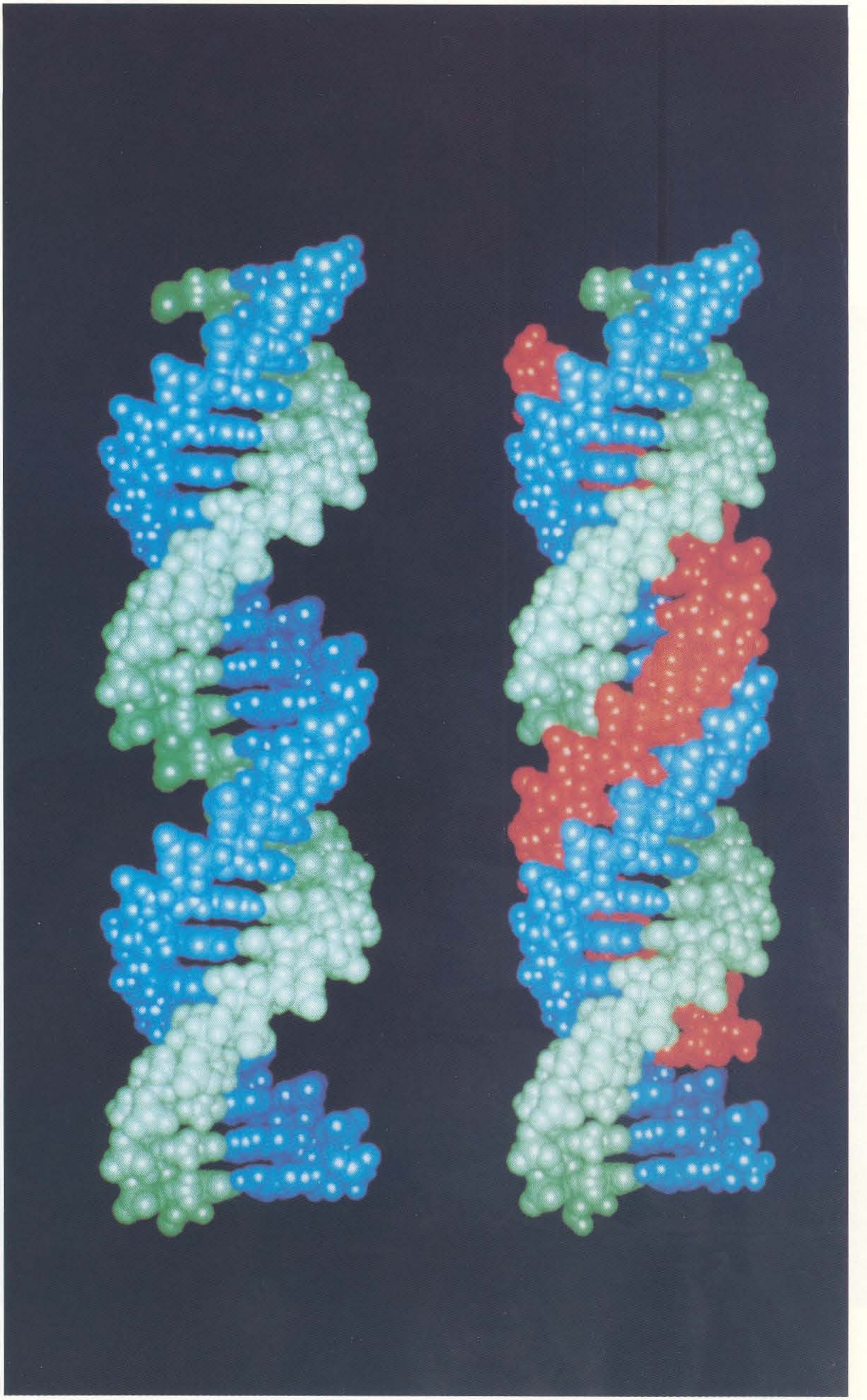


Designing Molecular Machines to Read the Genetic Blueprint

What is the genome project, and what does chemistry at Caltech have to do with it?

by Peter B. Dervan

Opposite, left: Two strands of DNA twist around each other into Watson and Crick's famous double helix. In this computer-generated image, one strand is colored blue, the other green. Each horizontal link between the strands is a letter in the genetic code—there are 24 letters in this image, and about 3 billion in a human cell. Right: A third strand of DNA, colored Caltech orange, can bind to the Watson and Crick strands without disrupting them. This chemical approach may be a general method for locating single sites in the human genome. This Caltech strand is 18 letters long.

The human genome project is an ambitious effort to map all of the 100,000 or so genes that make up the blueprint of man. I'm not going to talk about how much money we should spend on this, or how fast we should do it. Suffice it to say that it will happen sooner or later, and that it will affect everybody's life when it does. But what is the genome project, and what does chemistry at Caltech have to do with it?

Physicians have been mapping the human body for hundreds of years—charting where the bones are, and the muscles, and the blood vessels, and so on. Mapping the genome means finding the genes that make us what we are—the coded instructions that govern how we develop and grow, and determine what makes one person different from another—and pinpointing their specific locations in the genetic material. So in fact, this is the highest-resolution map of man.

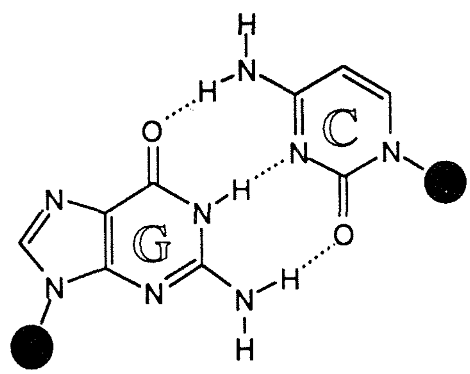
You can think of this genetic blueprint as an encyclopedia containing 2,000 volumes, each having 500 pages, and with 3,000 letters on each page. Say you want to know what makes your eyes blue, or predisposes you to cardiovascular disease. You need to be able to find out that the pertinent information is in Volume III, say, on page 357, and then you can turn to that page and look at that gene, or set of genes. So by mapping the genome, we are really writing the encyclopedia's index.

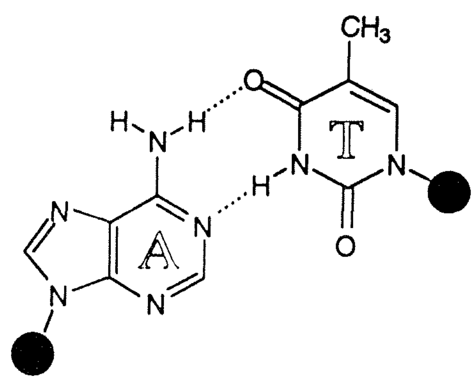
Our cells actually store this information in coded form in a molecule called deoxyribonucleic acid (DNA). The code is written in an alphabet much simpler than that of English, having only four letters instead of 26. The letters are chemical entities that we designate A (adenine), C (cytosine), G (guanine), and T (thymine). These letters are strung together in long sequences, like beads on a string, to make DNA. The DNA is such a valuable set of reference books that the library—a cell's nucleus—keeps it on reserve. When the cell needs to use the information, it doesn't let the DNA circulate out into the cell, but copies the information onto another molecule called RNA (ribonucleic acid), which is chemically very similar to DNA but not quite as stable. The RNA carries the blueprint's instructions to the cell's manufacturing centers, which make all the protein machines that give us hair, or make our muscles work, or digest our food. And when the cell has finished making the protein, it breaks down and recycles the RNA.

DNA is pretty sturdy stuff. It will last for millions of years in water at room temperature. So the chemical bonds—called covalent bonds—that hold the letters together in their correct sequence are very strong. This makes good sense—after all, if you are a cell, you don't want your master blueprint to fall apart on you. A human analogy to these strong bonds would be the bond between my elbow and my wrist. Chemists know a lot about these strong bonds—we synthesize small bits of genes in the laboratory routinely, on a machine. This machine is basically a lot of fancy plumbing and computer-controlled valves that mix the chemical ingredients in the right order.

But there's another set of weaker bonds that are very important to this story, and our understanding of these bonds is quite poor. This is the

My research group is trying to build a molecular machine that can scan this whole meter of DNA and find one single location on it, reading its bumps and edges, its nooks and crannies, like Braille.

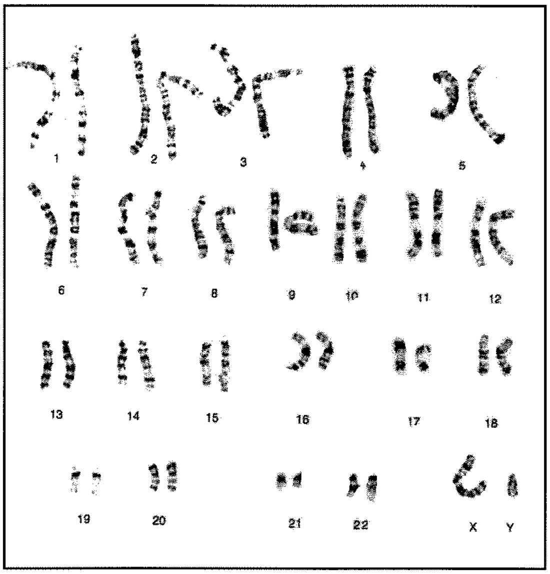




The four letters—C, G, A, and T—of the genetic code, and how they recognize each other. The hydrogen bonds that, taken together, constitute each "secret handshake" are shown as dotted lines. These bonds result from the attraction between a hydrogen atom (H) in one code letter and an atom of oxygen (O) or nitrogen (N) in the other code letter. The solid black circles represent the DNA molecule's backbone.

set of bonds that allows the stored information to be communicated so that the RNA copies can be made. You see, an A only talks to a T, and a G only talks to a C. Each pair of letters interacts with each other in a very specific way—a secret handshake, if you will, that allows each letter to recognize its partner. To carry the anatomical analogy further, this handshake is a very specific interaction—we don't shake shoulders—and it's strong enough that, if I have you by the hand, I could pull you from a river and save your life. But the interaction is weak enough that we can break it in an instant at a very specific place. If I shake your hand and then we turn and walk away from each other, you wouldn't tear my hand off and take it with you. You could also think of these weak bonds as being made of Velcro. It's these weak bonds—some of them are called hydrogen bonds—that give proteins and other biopolymers their specific three-dimensional shape, and it's a molecule's shape that allows it to perform its function. Chemists today are struggling to understand these weak bonds to the point where we can predict their behavior, so that we can design our own proteins from scratch.

Cellular DNA is actually two strands of letters laid head to toe, with each letter in one strand paired up with its partner in the other strand by these secret handshakes. The whole arrangement resembles a ladder, with the pairs of letters—base pairs—being the rungs. In fact, the molecule is twisted, so it really looks like a spiral staircase—Watson and Crick's famous double helix. When the cell wants to copy a particular piece of genetic information, it unwinds the relevant stretch of



A set of human chromosomes.

You might say that my assignment as a chemist is to develop a general method for finding needles in haystacks.

DNA and then separates the two strands from each other, like a zipper unzipping. Then pieces of RNA come in and make their handshakes with the exposed letters, so that the assembled RNA molecule transcribes the DNA's sequence and the information it contains.

The DNA ladder in each one of your cells contains about three billion rungs. Each rung is 3.4 Ångstroms tall—an Ångstrom is a ten-billionth of a meter—so at three billion rungs, that's roughly one meter of DNA per cell. The DNA obviously has got to be very tightly packed to fit in the cell. The DNA is tightly coiled like a telephone cord, except that the DNA coil is held together by proteins, and the coil twists around itself the way that the cord does when you hang up the phone. This tightly wound tangle of DNA is called a chromosome, and it's big enough to be visible under the microscope. A human being's meter of DNA is divided up into 23 chromosomes.

My research group is trying to build a molecular machine that can scan this whole meter of DNA and find one single location in it, reading its bumps and edges, its nooks and crannies, like Braille. DNA looks ribbon-smooth from a distance, but it's really quite lumpy when you look at it up close. (I should mention here that we understand the details of DNA's contours imperfectly, even today. It's only in the last few years that we've begun to get our first high-resolution glimpses of the double helix's stair-steps.) If we could learn a set of general rules for reading those contours, then we could design a set of molecules that would behave like a child's Lego set. We could assemble a bunch of pieces and the assembly would automatically snap onto the stretch of DNA that fits its shape The analogy is an apt one—each block has knobs, almost like teeth, that fit precisely into the holes in another block. If there's an extra knob sticking out, or the spacing between the holes is a bit off, the two blocks won't bind. Each and every knob–hole pair has to make the right handshake. We need such exact matching in the handshakes between our molecule and the DNA to guarantee letter-perfect sequence recognition. The problem is very difficult, because we need to be able to read DNA that's sitting on the library shelf, as it were—DNA in its compact, twisted-up form with the two strands zipped together. The zipped-up form only leaves a little bit of the edge of each base pair exposed, so we don't have much to work with.

But let's say we *can* find the rules to make the right set of Lego blocks to get that one-to-one recognition. Then, since we know the shape of each of the possible base pairs—and there are only four of them: AT, TA, CG, and GC—a biologist could give us a sequence of letters and say, "Here's part of the gene for cystic fibrosis," or "This belongs to a cancer gene," and we could assemble a molecule that would bind precisely, exclusively to the one spot in all that DNA where the gene actually is. You might say that my assignment as a chemist is to develop a general method for finding needles in haystacks. The biologists and the medical researchers will tell me what needles to look for—what sequences are important. In many cases, biologists know part

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of the sequence of a disease-causing gene without knowing where the gene is. This is because genes are the blueprints for proteins, and if an aberrant or malfunctioning protein can be tied to a disease, then biologists can work backward from the protein to deduce what the gene looked like that gave rise to the protein.

We want to do more than just find genes, which can contain as many as 100,000 letters. We now know that a change of one—or a few—letters out of the whole three billion is sufficient to cause some diseases—not all, but some. There's no need to get too nervous about this news, though, because there can be lots and lots of mistakes all through your DNA, and they won't affect your health at all. And there are bits of machinery in every cell that go around all the time, fixing mistakes and repairing the DNA. But some errors, in some specific locations, can be very bad. We want to be able to find these errors, too.

What does it really mean, finding one letter in three billion? According to the 1980 census, there are roughly 100 million residences in the United States. Let's assume that each one has 30 electrical outlets. (That may sound like a lot, but try counting the ones where you live sometime. You'd be surprised.) If there's a single dead wall socket anywhere in the U.S., I want to be able to find it rapidly. And I want to develop a general method, so that if I identify a bad socket in Wisconsin, and another one shorts out in Vermont, I can find it instantaneously.

I can explain our strategy by returning to the encyclopedia analogy. If we pull a volume off the

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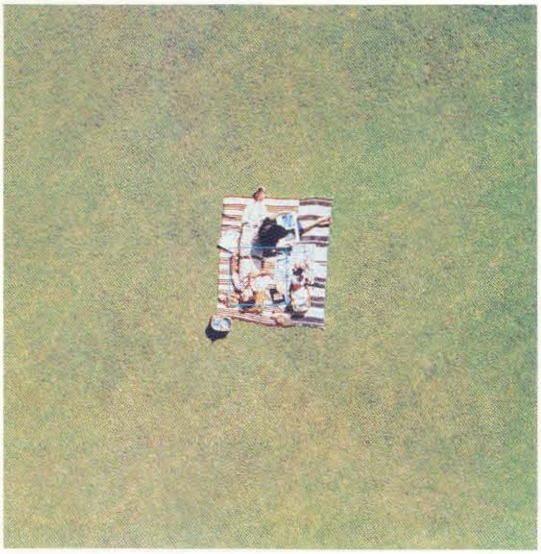

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What does 3 billion really mean? The back of an adult human hand (opposite, top left) is about 3 inches from wrist to knuckle. Each successive picture shows an area ten times wider than the previous one, but centered on the same spot. The small square in each picture outlines the previous picture. Thus the hand belongs to a man having a picnic in Grant Park, in downtown Chicago, on the shore of Lake Michigan, on the continent of North America, on planet Earth. The final picture spans a view roughly 3 billion inches wide.

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shelf and open it at random, a three-letter word like THE would occur often, but a 16-letter word like PREDETERMINATION would probably appear rarely. The larger the word, the rarer it's going to be. It's a simple mathematical exercise, really. There are 64—4 x 4 x 4, or 4^3—possible three-letter words we can make with a four-letter alphabet. In the three billion letters of the genome, each one of those 64 words should appear about 16 million times, assuming that all four letters, on average, are equally distributed throughout the genome. But there are roughly four and a quarter billion—4^{16}—ways to write a 16-letter word. Statistically, in the three billion letters of the genome, each one of those 16-letter sequences should appear rarely, or only once. In reality, some sequences occur over and over again in many different genes, but the point is that if we know a 16-letter sequence that's unique to the gene we're looking for, we can find it. And if we're looking for a single-letter error, what we need to do is look for a 16-letter sequence that includes our errant quarry. (Think what this would be like if we were working with English words—26^{16} is roughly 40,000,000,000,000, 000,000,000!)

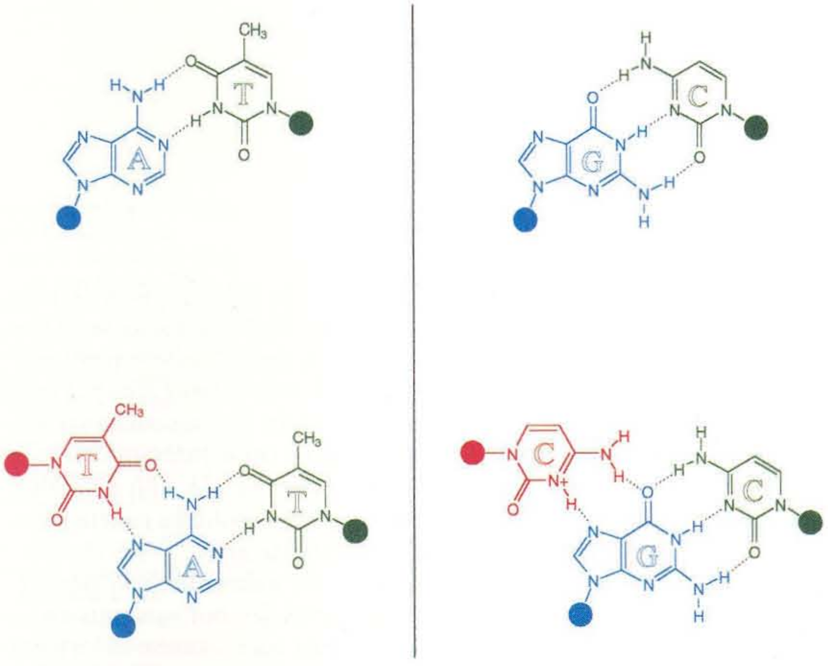
Biology—if not biologists—solved this problem a long time ago. Our cells are turning genes on and off at will every moment of our lives. Nature uses proteins—another class of polymer, another set of beads on a string—as molecular on/off switches. The protein alphabet is more complicated, having 20 letters. These letters also make handshakes with each other that cause the protein to fold up into a complex three-dimensional shape, and one portion of this shape's exterior surface reads the texture of the steps of the DNA spiral staircase. When the protein finds a location on the staircase that matches its reading surface exactly, it snaps onto that spot in yet another handshake. This DNA–protein handshake is an extraordinary one that scientists would dearly love to reproduce. The whole problem of how proteins fold to create such precisely engineered surfaces is a very complicated one that will probably take 10 years, and many researchers, to crack.

The ancients watched birds fly, and built themselves bird's wings, feathers and all, and jumped off of cliffs. That would be like me trying to duplicate how proteins recognize DNA.

But I'm impatient. I don't want to wait another decade (or two) until we figure out how proteins fold. Chemists are inventors—we're always creating new materials or rearranging old ones. Is there a way for chemists to make something that mimics nature's function—something whose behavior we could predict in advance, so that we could custom-design it to read the right shape? The key to the problem is the relationship between structure and function. Biology has evolved a structure that performs this function, but there might be other, less complex structures that are easier for humans to work with. The ancients watched birds fly, and built themselves bird's wings, feathers and all, and jumped off of cliffs. That would be like me trying to duplicate how proteins recognize DNA. But then people realized that a wing could be built out of wood and cloth—and later out of aluminum. It didn't look like a bird's wing anymore, but it had the same function. And now we can fly from Los Angeles to London in comfort, with air-conditioning and a movie, without having to ride on a bird's back, or, worse, flap our arms the whole distance.

Scientists are always building on other scientists' work. Every once in a while a Watson or Crick do something stunning that changes a whole field, but most science is built brick by brick. Sometimes a paper sits in the literature for a long time before someone sees an application for that work. Such a paper was written back in 1957, just after Watson and Crick proposed their double helix. Davies, Felsenfeld, and Rich—three physical chemists—reported that if you took double-helical RNA and simply added magnesium salts, the two-stranded polymer wound itself into a three-stranded polymer—a triple helix! This was an interesting anomaly but no one knew if it was really important. It was just a laboratory curiosity—an amusing chemical oddity—so it was duly written up. Nobody knew how the three strands bound together—they had no high-tech instruments back then to determine its detailed chemical structure.

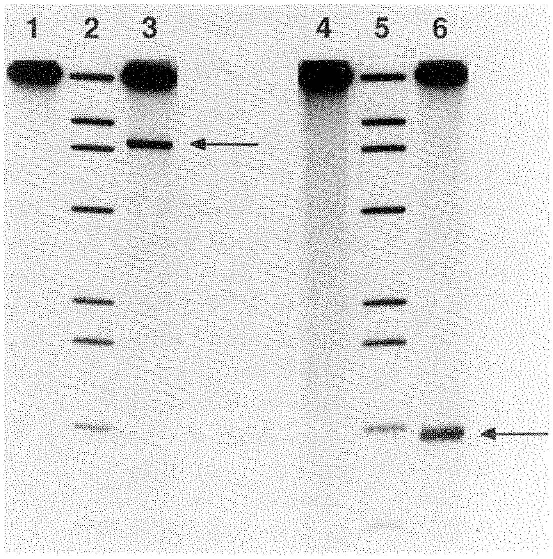
Could this three-stranded structure—the details of which are still imperfectly understood, and whose biological use, if any, remains unknown—be used for a new function: sequence recognition?



How the Caltech strand makes three-way handshakes with the Watson and Crick strands. The upper drawings on either side of the vertical line show the normal base-pair recognition seen previously on page 4. In the lower drawings, the Caltech strand is binding to the normal base pairs. The color scheme is the same as in the three-dimensional view on page 2.

Thirty years later, we read this paper and realized that if the third strand was lying on the steps of a normal, two-stranded piece of DNA like a carpet runner on a staircase, then we might be able to read a single site within a large piece of double-helical DNA by creating a short piece of DNA that would form a local third strand at that one site. In other words, could this three-stranded structure—the details of which are still imperfectly understood, and whose biological use, if any, remains unknown—be used for a new function: sequence recognition? And, in fact, the third strand can make very special three-way handshakes with the letters in the two normal strands. If we have an A-T pair as a step in the Watson-Crick spiral staircase, a T on the third strand can make a new handshake with the A without disturbing the rest of the staircase. Similarly, if we modify a C a little bit by putting an extra hydrogen ion on it, it will read the G in a G-C pair on the staircase. So if we have a DNA sequence on one Watson–Crick strand that consists only of As and Gs, we can make a third strand of Ts and Cs that will read that sequence and bind only to it. This is a very simple idea, because we can string together short, i.e., 16 letters long, sequences of Ts and Cs—called oligodeoxyribonucleotides—in our machines, and we don't have to worry about how the molecule we've made will fold up.

It's all very well to say that we're binding to one 16-letter sequence in three billion base pairs of DNA and no other, but how do we prove it? Well, we just add some new chemistry—we're inventors, after all. We give our molecule an



The result of the first site-specific recognition by triple-helix formation experiment. The DNA sample started at the top of each of the numbered lanes, and the fragments were drawn down the page by the electric field. Lanes 1 and 4 are the uncut DNA; lane 3 is the 3,000-letter fragment; lane 6 is the 1,000-letter fragment; lanes 2 and 5 contain sets of standard DNA fragments of known lengths that allow biologists to estimate the lengths of fragments in the other lanes.

Wherever our molecule binds, it will cut the DNA right next to that spot, leaving a permanent record of where it's been.

attachment that cuts DNA. We put it on the end of the molecule, like a stinger on a scorpion's tail. Wherever our molecule binds, it will cut the DNA right next to that spot, leaving a permanent record of where it's been. So if we've built a smart scalpel that finds a single site and cuts there, the DNA will be broken into two pieces. And if we use a piece of DNA whose sequence is already known, then we'll know exactly where that binding occurred, and how long each of the two broken pieces was. If we start with a DNA sequence 4,000 letters long, for example, and intend to cleave it after the 3,000th letter, we should get one fragment 3,000 letters long and one 1,000 letters long. But if we've built a molecule that doesn't recognize its target, then it will bind anywhere, and the DNA will be sliced into a million bits of different sizes.

Biologists have a powerful separation technique to measure the size of DNA fragments. It's called gel electrophoresis. They put the DNA sample on one end of a slab of polymer, called a gel, that the DNA wants to stick to. Then they apply an electrical field across the gel. Now DNA is a polyanion, meaning that it has lots of negative charges scattered along its length, so the electrical field attracts it and starts to drag the fragments along the gel. The little fragments are easier to move than the bigger ones. After a time, the little fragments have moved a long way down the gel, with the smallest fragments moving farthest, while the big fragments are still lying near where they started. So if we've really cut our DNA sample in just one spot to make two pieces of unequal length, we will see two nice, sharp bands on the gel—one for each piece. But if we've cut the DNA at random, we'll get one long smear down the gel, made up of fragments of all sizes. Postdoc Heinz Moser, a member of our group, did the first experimental site-specific recognition by triple-helix formation in 1987. He used a piece of DNA a bit more than 4,000 letters long, and behold! he got the two fragments he expected to get.

Then we raised the stakes. We still weren't ready for human DNA yet, but two years ago we moved up to brewer's yeast—*saccharomyces cerevisiae*—which has 14 million base pairs of DNA in its genome. We picked one of its 14 chromosomes, which happens to be 340,000 base pairs long, and found that we could break it at a single site of our choosing. We did the cleavage with 95 percent yield, so we then knew that the method works on large DNA from a real organism.

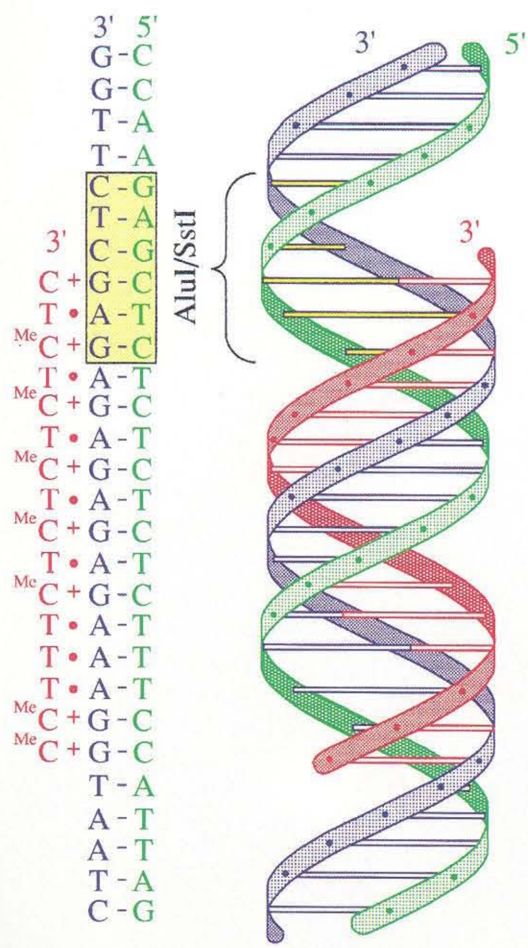
Now we're ready to take on the real challenge—a human chromosome. We want to take this basic research, which started as a purely academic study of the chemical principles behind weak bonds, and perhaps do something useful with it, while at the same time we explore its scope and limitations. It turns out that the gene for Huntington's disease, an inherited neurological disorder, is on the tip of human chromosome 4. This has been known for several years, ever since Nancy Wexler did a pioneering study on a group of Venezuelan villagers. Everyone in the village came from just a few ancestors, and a large fraction of the village's population had Huntington's disease. So she was able to draw up a genealogy for every villager, and trace the

That doesn't mean that we've isolated Huntington's disease, but we've got a piece of DNA in our test tube that's 3.6 million base pairs in size, and it comes from the right place.

gene's inheritance. Then she took DNA samples from the villagers, and, with the help of some brilliant biologists in Boston, was able to prove that the gene was on the tip of chromosome 4. The gene has been localized to the final three to four million base pairs of the chromosome—only a few percent of the overall length—so it's truly right on the tip. Now if we could isolate that piece in a test tube, perhaps the biologists could go in and actually pull out the specific gene.

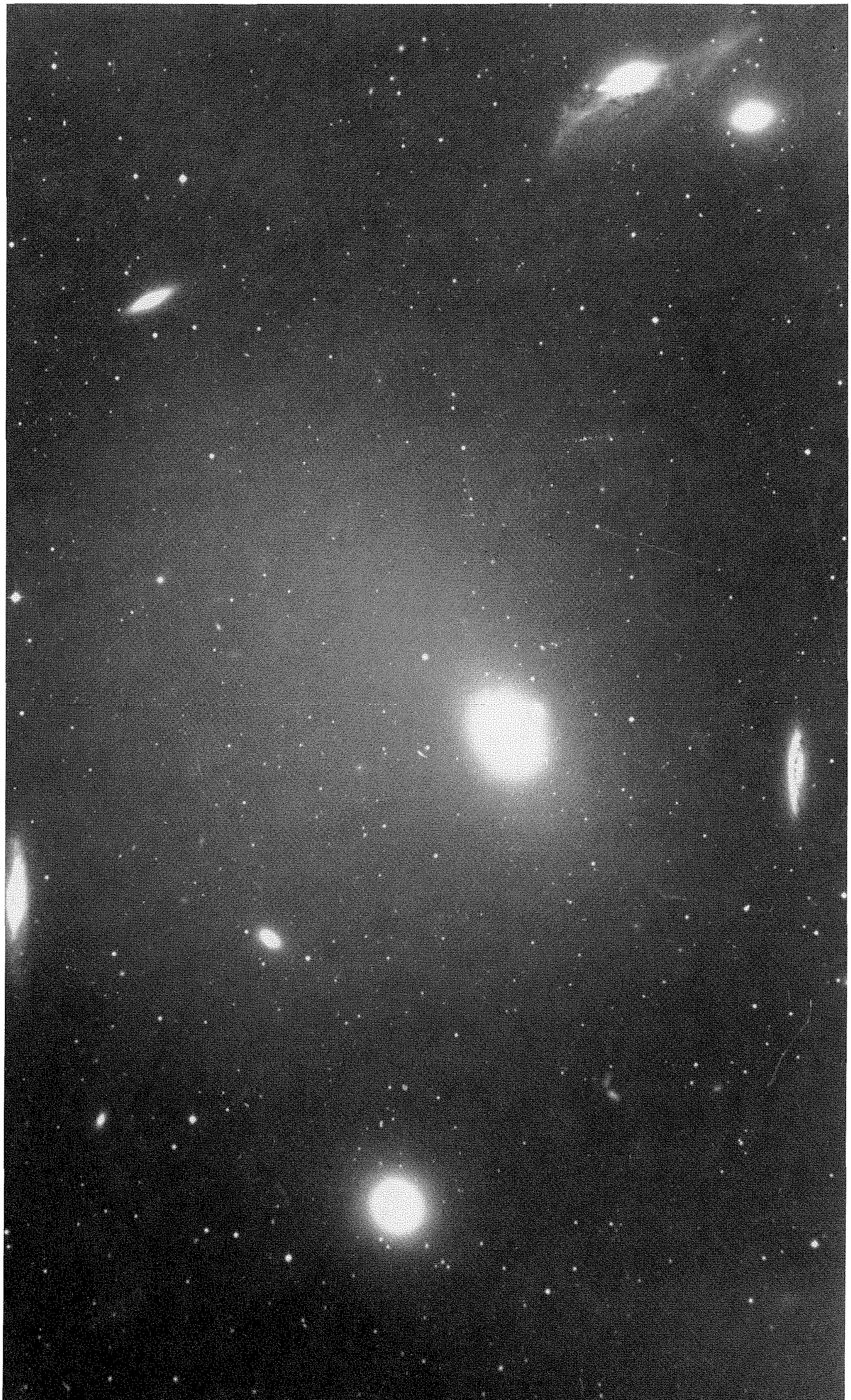
We found a 16-base-pair sequence that we could recognize, because each of its letters was capable of making one of our three-way handshakes. It was in a piece of DNA, called a genetic marker, that is known to lie very close to the gene and is inherited along with it. Scott Strobel, a graduate student in our laboratory, has built the "Caltech strand" that should bind to that sequence and direct the cutting at that site by a DNA-cleaving enzyme. He's doing an experiment, in collaboration with David Housman's group at MIT, to see if that strand will find the Huntington's marker out of a background of 10 billion base pairs—10 billion, because he's working with mouse cells that have been altered to include human chromosome 4—and will snip the tip off of that chromosome. I believe he's done it. That doesn't mean that we've isolated Huntington's disease, but we've got a piece of DNA in our test tube that's 3.6 million base pairs in size, and it comes from the right place.

Whether isolating this particular fragment has placed a key piece of the puzzle in the hands of the geneticists who're searching for the Huntington's gene isn't yet known. It's fair to say, however, that our method will in all likelihood be sufficiently general to isolate almost any fragment of the human genome that's a few million base pairs in size. This may speed up the physical mapping of the thousands of genes important in human disease, and accelerate progress in the rational design of novel therapeutics that will alleviate human suffering. □



Gene hunters at work. Left: Designing a Caltech strand to bind to human DNA. The color scheme is the same as on pages 2 and 9. The set of six base pairs in the yellow box is the enzyme's cleavage site, which overlaps the 16-letter sequence that the Caltech strand can recognize. The normal base-pair handshakes are shown as dashes, while the three-way handshakes are symbolized as plus signs or bullets. The 3' and 5' indicate in which direction the DNA strand is running. Right: The Caltech strand nestles into the larger one of the DNA's two grooves—called the major groove—like a carpet runner on a spiral staircase.

Peter B. Dervan, Bren Professor of Chemistry, got his BS in chemistry from Boston College in 1967, and came to Caltech in 1973 after earning his PhD in physical organic chemistry at Yale. This article is adapted from his Centennial Seminar Day talk.



The Future of the Universe

There are certain situations in which we think that we can make reliable predictions, and the future of the universe, on a very large scale, is one of them.

by Stephen W. Hawking

Gravity attracts some galaxies into groups like the Virgo Cluster, whose central region is shown here. The galaxies are moving within the clusters at such high speeds, however, that they would fly apart unless there were some extra mass, greater than the masses of the galaxies, keeping the cluster together. What is not known is whether this unseen dark matter exists in great enough quantities to cause the universe to collapse in a Big Crunch.

In this lecture I'm supposed to tell you about the future of the universe, or rather, what scientists think the future will be. Of course, predicting the future is very difficult. I once thought I would like to have written a book called *Yesterday's Tomorrow—A History of the Future.* It would have been a history of predictions of the future, nearly all of which have been very wide of the mark. But I don't suppose it would have sold as well as my history of the past.

Foretelling the future was the job of oracles or sibyls. These were often women who would be put into a trance by some drug or by breathing the fumes from a volcanic vent. Their ravings would then be interpreted by the surrounding priests. The real skill lay in the interpretation. The famous oracle at Delphi in ancient Greece was notorious for hedging its bets or being ambiguous. When the Spartans asked what would happen when the Persians attacked Greece, the oracle replied: Either Sparta will be destroyed or its king will be killed. I suppose the priests reckoned that if neither of these eventualities actually happened, the Spartans would be so grateful to the god Apollo that they would overlook the fact that his oracle had been wrong. In fact, the king *was* killed, defending the pass at Thermopylae, in an action that saved Sparta and led to the ultimate defeat of the Persians.

On another occasion, Croesus, king of Lydia, the richest man in the world, asked what would happen if he invaded Persia. The answer was, a great kingdom would fall. Croesus thought this meant the Persian Empire, but it was his own kingdom that fell, and he himself ended up on a pyre about to be burned alive.

Recent prophets of doom have been more ready to stick their necks out by setting definite dates for the end of the world. These have tended to depress the stock market, though it beats me why the end of the world should make one want to sell shares for money. Presumably, you can't take either with you.

A number of dates have been set for the end of the world. So far they have all passed without incident. But the prophets have often had an explanation of their apparent failures. For example, William Miller, the founder of the Seventh Day Adventists, predicted that the Second Coming would occur between March 21, 1843, and March 21, 1844. When nothing happened, the date was revised to October 22, 1844. When that passed without apparent incident, a new interpretation was put forward. According to this, 1844 was the start of the Second Coming. But first, the names in the Book of Life had to be counted. Only then would the Day of Judgment come for those not in the book. Fortunately for the rest of us, this counting seems to be taking a long time.

Of course, scientific predictions may not be any more reliable than those of oracles or prophets. One only has to think of the example of weather forecasts. But there are certain situations in which we think that we can make reliable predictions, and the future of the universe, on a very large scale, is one of them.

Over the last 300 years we have discovered the scientific laws that govern matter in all normal situations. We still don't know the exact laws

Even though we may know the relevant laws that govern the universe, we may not be able to use them to predict very far into the future. This is because the solutions to the equations of physics may exhibit a property known as chaos.

that govern matter under very extreme conditions. These laws are important for understanding how the universe began, but they do not affect the future evolution of the universe unless and until the universe recollapses to a high density state. In fact, it is a measure of how little these high energy laws affect the universe, that we have to spend large amounts of money to build giant particle accelerators to test them.

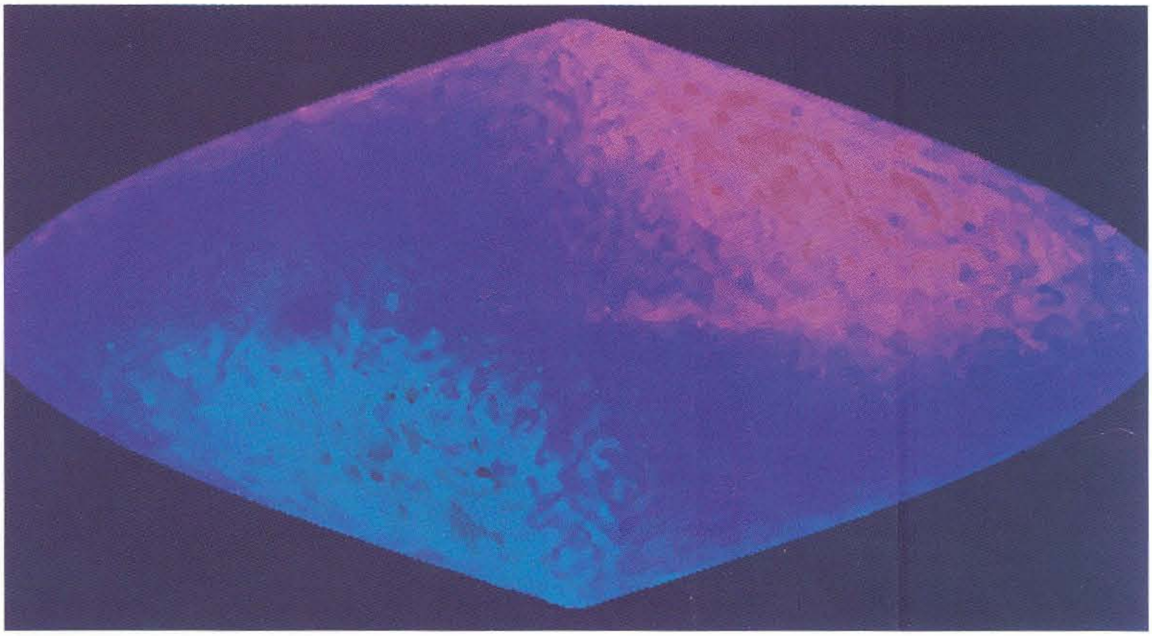
Even though we may know the relevant laws that govern the universe, we may not be able to use them to predict very far into the future. This is because the solutions to the equations of physics may exhibit a property known as chaos. What this means is that a slight change in the starting conditions may make the equations unstable. Change the way a system is by a small amount at one time, and the later behavior of the system may soon become completely different. For example, if you change slightly the way you spin a roulette wheel, you will change the number that comes up. It is practically impossible to predict the number that will come up; otherwise, physicists would make a fortune at casinos.

With unstable and chaotic systems there is generally a certain time scale on which a small change in the initial state will grow into a change that is twice as big. In the case of Earth's atmosphere, this time scale is of the order of five days, about the time it takes for air to blow all the way around the world. One can make reasonably accurate weather forecasts for periods up to five days, but to predict the weather much further ahead would require both a very accurate knowledge of the present state of the atmosphere and an impossibly complicated calculation. There is no way that we can predict the weather six months ahead beyond giving the seasonal average.

We also know the basic laws that govern chemistry and biology. So, in principle, we ought to be able to determine how the brain works. But the equations that govern the brain almost certainly have chaotic behavior, in that a very small change in the initial state can lead to a very different outcome. Thus, in practice, we cannot predict human behavior, even though we know the equations that govern it. Science cannot predict the future of society, or even whether it *has* any future. The danger is that our power to damage or destroy the environment, or each other, is increasing much more rapidly than our wisdom in using this power.

Whatever happens on Earth, the rest of the universe will carry on regardless. It seems that the motion of the planets around the sun is ultimately chaotic, though with a long time scale. This means that the errors in any prediction get bigger as time goes on. After a certain time, it becomes impossible to predict the motion in detail. We can be fairly sure that Earth will not have a close encounter with Venus for quite a long time. But we cannot be certain that small perturbations in the orbits could not add up to cause such an encounter a billion years from now.

The motion of the sun and other stars around the galaxy, and the motion of the galaxy in the Local Group of galaxies, is also chaotic. In contrast, the motion of the universe on very large scales seems to be uniform and not chaotic. We observe that other galaxies are moving away from



This full-sky microwave map from the Cosmic Background Explorer (COBE) satellite shows a smooth variation between hot and cold spots on opposite sides of the sky. This variation is caused by a Doppler shift due to the Earth's motion through the universe. When the Doppler shift is removed, the intensity of the radiation is the same from every direction, indicating that the universe's expansion on very large scales is highly uniform and not chaotic, and can be predicted far into the future.

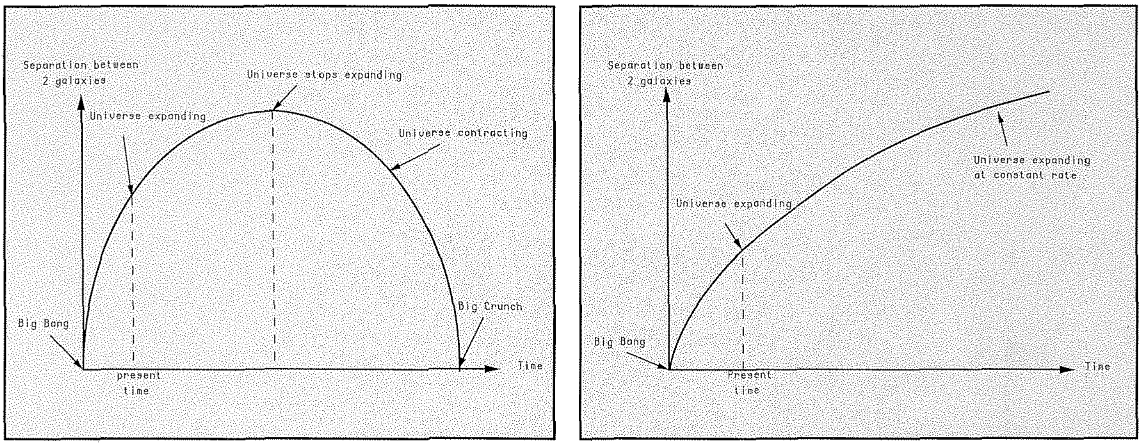
us, and the farther they are from us, the faster they are moving away. This means that the universe is expanding in our neighborhood: the distances between different galaxies are increasing with time.

We also observe a background of microwave radiation coming from outer space. You can actually observe this radiation yourself by turning your television to an empty channel. A few percent of the flecks you see on the screen are due to microwaves from beyond the solar system. It is the same kind of radiation that you get in a microwave oven, but very much weaker. It would only raise food 2.7 degrees above absolute zero, so it is not much good for warming up your take-out pizza. This radiation is thought to be left over from a hot early stage of the universe. But the most remarkable thing about it is that the amount of radiation seems to be the same from every direction. This radiation has been measured very accurately by the Cosmic Background Explorer satellite. The map of the sky above was made from these observations. Different intensities of radiation are indicated by different colors. As you can see, the color is the same in every direction. What differences there are, are consistent with the noise in the experiment. There is no evidence of any variation in the background with direction, to a level of one part in 10,000.

Whatever happens on Earth, the rest of the universe will carry on regardless.

In ancient times, people believed that the Earth was at the center of the universe. They would therefore not have been surprised that the background was the same in every direction. However, since the time of Copernicus, we have been demoted to a minor planet, going around a very average star, in the outer edge of a typical galaxy that is only one of a hundred billion we can see. We are now so modest that we wouldn't claim any special position in the universe. We must therefore assume that the background is also the same in any direction about any other galaxy. This is possible only if the average density of the universe and the rate of expansion are the same everywhere. Any variation in the average density or the rate of expansion over a large region would cause the microwave background to be different in different directions. This means that on a very large scale the behavior of the universe is simple and is not chaotic. It can therefore be predicted far into the future.

Because the expansion of the universe is so uniform, we can describe it in terms of a single number—the distance between two galaxies. This is increasing at the present time, but we would expect the gravitational attraction between different galaxies to be slowing down the rate of expansion. If the density of the universe is greater than a certain critical value, gravitational attraction will eventually stop the expansion and make the universe start to contract again. The universe would collapse to a Big Crunch. This would be rather like the Big Bang that began the universe. The Big Crunch would be what is called a singularity, a state of infinite density at which the laws of physics would break down. This means that, even if there were events after the Big Crunch, what happened at them could not be predicted. But without a causal connection between events, there is no meaningful way



Expansion of the universe can be described by the separation between two galaxies. Gravitational attraction should slow down the expansion and eventually, if the average density of the universe is above a certain critical value, cause it to collapse to a Big Crunch. If the density is less than the critical value, the universe will continue to expand forever, the gravitational attraction having less and less of an effect on slowing it down.

Even if the universe is going to recollapse, I can confidently predict that it will not stop expanding for at least 10 billion years. I don't expect to be around to be proved wrong.

that one can say that one event happened after another. One might as well say that our universe came to an end at the Big Crunch, and that events that occurred "after" were part of another, separate universe. It's a bit like reincarnation. What meaning can one give to the claim that a new baby is the same as someone who died, if the baby doesn't inherit any characteristics or memories from its previous life? One might as well say that it is a different individual.

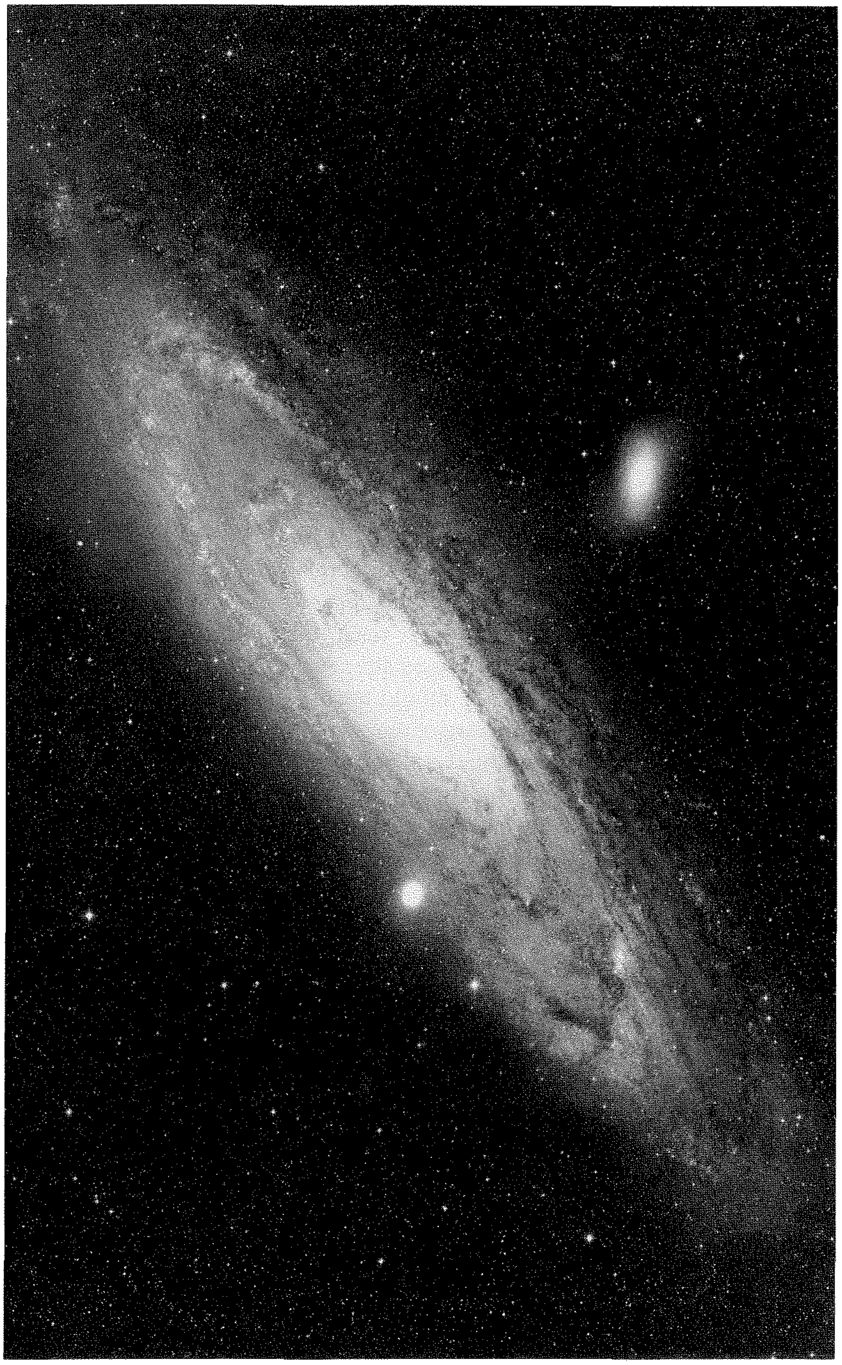
If the average density of the universe is less than a critical value, it will not recollapse but will continue to expand forever. After a certain time, the density will become so low that gravitational attraction will not have any significant effect on slowing down the expansion. The galaxies will continue to move apart at a constant speed.

So the crucial question for the future of the universe is: What is the average density? If it is less than the critical value, the universe will expand forever. But if it is greater, the universe will recollapse, and time itself will come to an end at the Big Crunch. I do, however, have certain advantages over other prophets of doom. Even if the universe is going to recollapse, I can confidently predict that it will not stop expanding for at least 10 billion years. I don't expect to be around to be proved wrong.

We can try to estimate the average density of the universe from observations. If we count the stars we can see and add up their masses, we get less than 1 percent of the critical density. Even if we add in the masses of the clouds of gas that we observe in the universe, it still only brings the total up to about 1 percent of the critical value. However, we know that the universe must also contain what is called dark matter, which we cannot observe directly. One piece of evidence for this dark matter comes from spiral galaxies. These are enormous pancake-shaped collections of stars and gas. We observe that they are rotating about their centers. But the rate of rotation is so high that they would fly apart if they contained only the stars and gas that we observe. There must be some unseen form of matter, whose gravitational attraction is great enough to hold the galaxies together as they rotate.

Another piece of evidence for dark matter comes from clusters of galaxies. We observe that galaxies are not uniformly distributed throughout space, but are gathered together in clusters that range from a few galaxies to millions. Presumably these clusters are formed because the galaxies attract each other into groups. We can, however, measure the speeds at which individual galaxies are moving in these clusters. We find they are so high that the clusters would fly apart unless they were held together by the gravitational attraction. The mass required is considerably greater than the masses of all the galaxies. This is the case, even if we take the galaxies to have the masses required to hold themselves together as they rotate. It follows, therefore, that there must be extra dark matter present in clusters of galaxies, besides the galaxies that we see.

We can make a fairly reliable estimate of the amount of the dark matter in galaxies and clusters for which we have definite evidence. But this estimate is still only about 10 percent of the



Spiral galaxies, such as the great galaxy in Andromeda, offer evidence of the presence of dark matter in the universe, which can't be observed directly. If the stars and gas that can be seen rotating around the galaxy's center were all that it contained, it would fly apart. The gravitational attraction of some other form of matter must be holding it intact.

critical density needed to cause the universe to collapse again. Thus, if one went just by the observational evidence, one would predict that the universe would continue to expand forever. But our solar system won't last forever. After another 5 billion years or so, the sun will reach the end of its nuclear fuel. It will swell up as a red giant star until it has swallowed up Earth and the other nearer planets. It will then settle down to be a white dwarf, a few thousand miles across. So I *am* predicting the end of the world, but not just yet. I don't think this prediction will depress the stock market too much. There are one or two more immediate problems on the horizon. Anyway, by the time the sun blows up we should have mastered the art of interstellar travel, if we have not already destroyed ourselves.

After 10 billion years or so, most of the stars in the universe will have burnt out. Stars with masses like that of the sun will become white dwarfs, or neutron stars which are even smaller and more dense. More massive stars can become black holes, which are still smaller, and which have such a strong gravitational field that no light can escape. However, these remnants will still continue to go around the center of our galaxy about once every hundred million years. Close encounters between the remnants will cause a few to be flung right out of the galaxy. The remainder will settle down to closer orbits about the center and will eventually collect together to form a giant black hole at the center of the galaxy. Whatever the dark matter in galaxies and clusters is, it might also be expected to fall into these very large black holes.

A large black hole at the center of a galaxy could take 1,000,000,000,000,000,000,000, 000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000, 000,000,000 years to evaporate away and disappear completely.

The future of a universe that expanded forever would be rather boring.

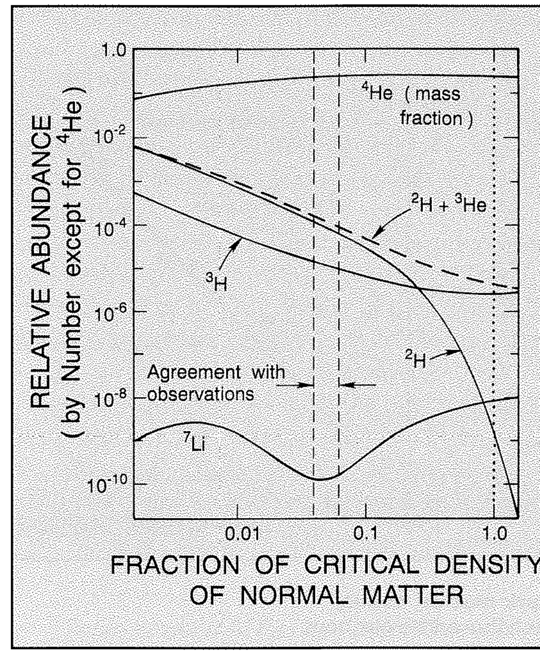
One might expect, therefore, that most matter in galaxies and clusters would eventually end up in black holes. However, some time ago I discovered that black holes weren't as black as they had been painted. The uncertainty principle of quantum mechanics says that particles cannot have both a well-defined position and a well-defined speed. The more accurately the position of a particle is defined, the less accurately its speed can be defined, and vice versa. If a particle is in a black hole, its position is well defined to be within the black hole. This means that its speed cannot be exactly defined. It is therefore possible for the speed of the particle to be greater than the speed of light. This would enable it to escape from the black hole. Particles and radiation will thus slowly leak out of a black hole. A giant black hole at the center of a galaxy would be millions of miles across. Thus there would be a large uncertainty in the position of a particle inside it. The uncertainty in the particle's speed would therefore be small. This means that it would take a very long time for a particle to escape from the black hole. But it would eventually. A large black hole at the center of a galaxy could take 10^{90} years to evaporate away and disappear completely. That is 1 followed by 90 zeros. This is far longer than the present age of the universe, which is a mere 10^{10}. Still, there will be plenty of time if the universe is going to expand forever.

The future of a universe that expanded forever would be rather boring. But it is by no means certain that the universe will expand forever. We have definite evidence only for about a tenth of the density needed to cause the universe to recollapse. But there might be further kinds of dark matter that we have not detected, which could raise the average density of the universe to the critical value or above it. This additional dark matter would have to be located outside galaxies and clusters of galaxies. Otherwise we would have noticed its effect on the rotation of galaxies or on the motions of galaxies in clusters.

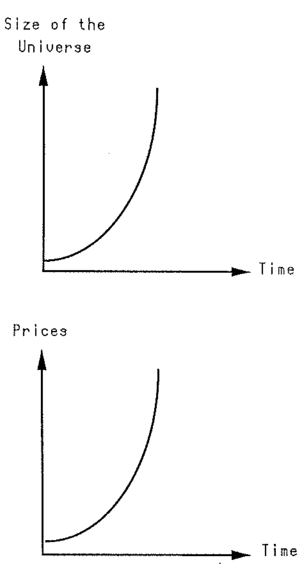
Why should we think there might be enough dark matter to make the universe recollapse eventually? Why don't we just believe in the matter for which we have definite evidence? The reason is that having even a tenth of the critical density now, requires an incredibly careful choice of the initial density and rate of expansion. If the density of the universe one second after the Big Bang had been greater by one part in a thousand billion, the universe would have recollapsed after 10 years. On the other hand, if the density of the universe at that time had been less by the same amount, the universe would have been essentially empty since the time it was about 10 years old. How was it that the initial density of the universe was chosen so carefully? Maybe there is some reason that the universe should have precisely the critical density.

There seem to be two possible explanations. One is the so-called anthropic principle, which can be paraphrased as: The universe is as it is, because if it were different, we wouldn't be here to observe it. The idea is that there could be many universes with different densities. Only those that were very close to the critical density would last for long and contain enough matter

The amounts of various light elements produced in the Big Bang can be calculated, but these abundances depend on the amount of normal matter in the universe. The actual observed abundances of these elements fall within the dashed-line vertical column, at the point where the amount of normal matter is just less than a tenth of the critical density. If the theory of inflation is correct so the total density is critical, then the other nine-tenths cannot be normal matter.



for stars and planets to form. Only in those universes would there be intelligent beings to ask the question: Why is the density so close to the critical density? If this is the explanation of the present density of the universe, there is no reason to believe that the universe contains more matter than we have already detected. A tenth of the critical density would be enough matter for galaxies and stars to form.

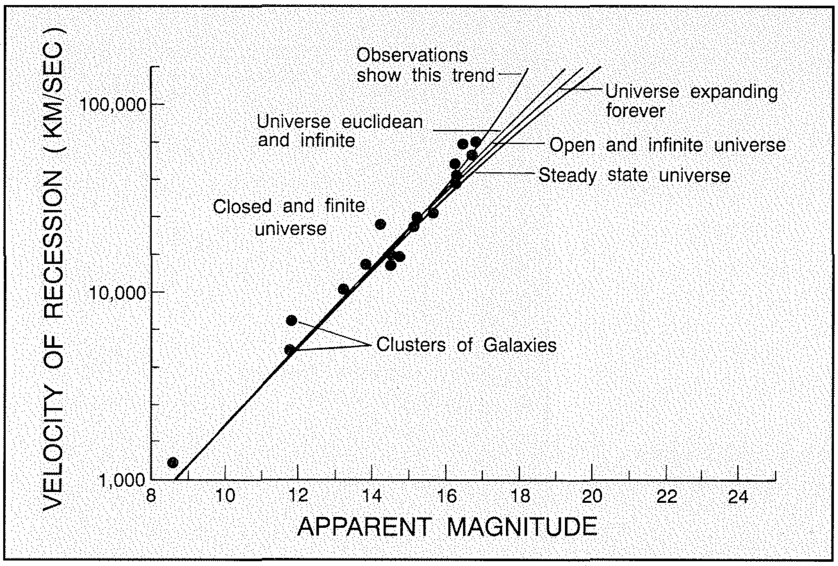


The theory of inflation in the infant universe is much like inflation in the economy, although the early universe would have inflated a bit faster—increasing by a factor of a billion billion billion in a fraction of a second.

Many people, however, do not like the anthropic principle because it seems to attach too much importance to our own existence. There has thus been a search for another possible explanation of why the density should be so close to the critical value. This search led to the theory of inflation in the early universe. The idea is that the size of the universe might have kept doubling like the way prices double every few months in some countries. The inflation of the universe, however, would have been much more rapid and extreme: an increase by a factor of at least a billion billion billion in a tiny fraction of a second. This amount of inflation would have caused the universe to have so nearly the exact critical density that it would still be very near the critical density now. Thus, if the theory of inflation is correct, the universe must contain enough dark matter to bring the density up to the critical value. But because of the uncertainty principle of quantum mechanics, the universe could not be exactly the same everywhere and could not have the same critical density. This means that the universe would probably recollapse eventually, but not for much longer than the 15 billion years or so that it has already been expanding.

What could the extra dark matter be that must be there if the theory of inflation is correct? It seems that it is probably different from normal matter, the kind that makes up the stars and planets. We can calculate the amounts of various light elements that would have been produced in the hot early stages of the universe, in the first three minutes after the Big Bang. The amounts of these light elements depend on the amount of normal matter in the universe. One can draw graphs, with the amount of light elements shown vertically, and the amount of normal matter in the universe along the horizontal axis. One gets good agreement with the observed abundances if the total amount of normal matter is only about one tenth of the critical amount now. It could be that these calculations are wrong, but the fact that we get the observed abundances for several different elements is quite impressive.

If there really is a critical density of dark matter, and it is not the kind of matter that stars and galaxies are made of, what could it be? The main candidates would be remnants left over from the early stages of the universe. One possibility is elementary particles. There are several hypothetical candidates, particles that we think might exist but which we have not actually detected yet. But the most promising case is a particle for which we have good evidence—the neutrino. This was thought to have no mass of its own. Some recent observations, however, have suggested that the neutrino may have a small mass. If this is confirmed and found to be of the right value, neutrinos would provide enough mass to bring the density of the



Isn't there something we can do to make the future more interesting? One way that would certainly do that would be to steer ourselves into a black hole.

Dark matter, evenly distributed throughout the universe, would slow down the expansion, which can be measured by the speed at which distant galaxies are receding. This speed can be plotted against the galaxies' apparent brightness—the farther away (in distance or back in time) they are, the dimmer they are (which means the larger their "apparent magnitude"), and the faster they move. Brightness, however, has turned out to be an unreliable measure of distance, so this sort of calculation doesn't determine the real rate of slowing, but only indicates that it's not happening very fast.

universe up to the critical value.

Another possibility is black holes. It is possible that the early universe underwent what is called a phase transition. The boiling or freezing of water are examples of phase transitions. In a phase transition, an initially uniform medium, such as water, develops irregularities, such as lumps of ice or bubbles of steam. These irregularities might collapse to form black holes. If the black holes were very small, they would have evaporated by now because of the effects of the quantum mechanical uncertainty principle, as I described earlier. But if they were over a few billion tons (the mass of a mountain), they would still be around today and would be very difficult to detect.

The only way we could detect dark matter that was uniformly distributed throughout the universe would be by its effect on the expansion of the universe. One can determine how fast the expansion is slowing down by measuring the speed at which distant galaxies are moving away from us. The point is that we are observing these galaxies in the distant past, when light left them on its journey to us. One can plot a graph of the speed of the galaxies against their apparent brightness, or magnitude, which is a measure of their distance from us. Different lines on this graph correspond to different rates of slowing of the expansion. A graph that goes straight, or flattens out, corresponds to a universe that will expand forever. And a graph that bends up corresponds to a universe that will recollapse. At first sight the observations seem to indicate recollapse. But the trouble is that the apparent brightness of a galaxy is not a very good indication of its distance from us. Not only is there considerable variation in the intrinsic brightness of galaxies, but there is also evidence that their brightness is varying with time. Since we do not know how much to allow for the evolution of brightness, we can't yet say what the rate of slowing down is—whether it is fast enough for the universe to recollapse eventually, or whether it will continue to expand forever. That will have to wait until we develop better ways of measuring the distances of galaxies. But we can be sure that the rate of slowing down is not so rapid that the universe will collapse in the next few billion years. That should give us time to sort out the Middle East crisis and one or two other problems.

Neither expanding forever nor recollapsing in a hundred billion years or so are very exciting prospects. Isn't there something we can do to make the future more interesting? One way that would certainly do that would be to steer ourselves into a black hole. It would have to be a fairly big black hole—more than a million times the mass of the sun. Otherwise, the difference in the gravitational pull on our head and our feet would tear us into spaghetti before we got inside. But there is a good chance that there's a black hole that big at the center of the galaxy.

We are not quite sure what happens inside a black hole. There are solutions of the equations of general relativity that would allow one to fall into a black hole and come out of a white hole somewhere else. A white hole is the time reverse of a black hole. It is an object that things can come out of, but nothing can fall into.

But the best evidence we have that time travel is not possible and never will be, is that we have not been invaded by hordes of tourists from the future.

The white hole could be in another part of the universe. Thus, this would seem to offer the possibility of rapid intergalactic travel. The trouble is, it might be too rapid. If travel through black holes were possible, there would seem to be nothing to prevent you from arriving back before you set off. You could then do something, like kill your mother, that would have prevented you from going in the first place. You only have to watch *Back to the Future* to see the problems that time travel could cause.

However, perhaps fortunately for our survival and that of our mothers, it seems that the laws of physics do not allow such time travel. There seems to be a Chronology Protection Agency that makes the world safe for historians by preventing travel into the past. What seems to happen is that the effects of the uncertainty principle cause a large amount of radiation, if one can travel into the past. This radiation would either warp spacetime so much that it would not be possible to go back in time; or it would cause spacetime to come to an end in a singularity—like the Big Bang and the Big Crunch. Either way, our past would be safe from evil-minded persons. The Chronology Protection Hypothesis is supported by some recent calculations that I and other people have done. But the best evidence we have that time travel is not possible and never will be, is that we have not been invaded by hordes of tourists from the future.

To sum up: Scientists believe that the universe is governed by well-defined laws that in principle allow one to predict the future. But the motion given by the laws is often chaotic. This means that a tiny change in the initial situation can lead to change in the subsequent behavior, a change that rapidly grows large. Thus, in practice one can often predict accurately only a fairly short time into the future. However, the behavior of the universe on a very large scale seems to be simple and not chaotic. One can therefore predict whether the universe will expand forever or whether it will recollapse eventually. This depends on the present density of the universe. In fact, the present density seems to be very close to the critical density that separates recollapse from indefinite expansion. If the theory of inflation is correct, the universe will actually be on the knife edge. So I'm in the well-established tradition of oracles and prophets of hedging my bets by predicting both ways. □

The largest crowd in Beckman Auditorium history, spilling over into Ramo Auditorium for simulcast video and mere audio on the grass outside, came to hear Stephen Hawking give the talk on which this article is based. Hawking, who has suffered from amyotrophic lateral sclerosis since he was 21, delivered his lecture and answered questions afterward using a computer voice machine, which he operates from a keyboard. The lecture was part of a Caltech Centennial symposium—"The Origin and Evolution of Large Scale Structure in the Universe"—in late September, and was arguably the Centennial's biggest hit, at least in numbers.

Hawking is the Lucasian Professor of Mathematics at Cambridge University—Isaac Newton's old chair. He received his undergraduate degree in 1962 from Oxford and PhD in 1965 from Cambridge, where he has remained ever since; he's been a professor in the Department of Applied Mathematics and Theoretical Physics there since 1977. A longtime friend of Kip Thorne, the Feynman Professor of Theoretical Physics, Hawking spent a year at Caltech in the mid-seventies as a Sherman Fairchild Distinguished Scholar and will return as a Fairchild Scholar this January. Widely regarded by physicists as one of the most brilliant theoreticians since Einstein, Hawking also brought cosmology to the masses with his 1988 bestseller, A Brief History of Time.

When asked in 1975 (in an interview published in Caltech News*) whether he believed that humans will ever discover the ultimate laws that control the universe, Hawking replied: "I rather hope not. There may be ultimate answers, but if there are, I would be sorry if we were to find them. For my own sake I would like very much to find them, but their discovery would leave nothing for those coming after me to seek. Each generation builds on the advances of the previous generation, and this is as it should be. As human beings, we need the quest."*

And take thou this (O thoughts of men accurs'd)
" *Past, and to Come, seemes best; things Present worst.*
Mow. Shall we go draw our numbers, and set on?
Hast. We are Times subiects, and Time bids, be gon.

Actus Secundus. Scœna Prima.

Enter Hostesse, with two Officers, Fang, and Snare.

Hostesse. Mr. *Fang*, haue you entred the Action?

Fang. It is enter'd.

Hostesse. Wher's your Yeoman? Is it a lusty yeoman? Will he stand to it?

Fang. Sirrah, where's *Snare*?

Hostesse. I, I, good M. *Snare*.

Snare. Heere, heere.

Fang. *Snare*, we must Arrest Sir *Iohn Falstaffe*.

Host. I good M. *Snare*, I haue enter'd him, and all.

Sn. It may chance cost some of vs our liues: he wil stab

Hostesse. Alas the day: take heed of him: he stabd me in mine owne house, and that most beastly: he cares not what mischeefe he doth, if his weapon be out. Hee will foyne like any diuell, he will spare neither man, woman, nor childe.

Fang. If I can close with him, I care not for his thrust.

Hostesse. No, nor I neither: Ile be at your elbow.

Fang. If I but fist him once: if he come but within my Vice.

Host. I am vndone with his going: I warrant he is an infinitiue thing vpon my score. Good M. *Fang* hold him sure: good M. *Snare* let him not scape, he comes continuantly to Py-Corner (sauing your manhoods) to buy a saddle, and hee is indited to dinner to the Lubbars head in Lombardstreet, to M. *Smoothes* the Silkman. I pra'ye, since my Exion is enter'd, and my Case so openly known to the world, let him be brought in to his answer: A 100 Marke is a long one, for a poore lone woman to beare: & I haue borne, and borne, and borne, and haue bin fub'd off, and fub'd-off, from this day to that day, that it is a shame to be thought on. There is no honesty in such dealing, vnles a woman should be made an Asse and a Beast, to beare euery Knaues wrong. *Enter Falstaffe and Bardolfe.* Yonder he comes, and that arrant Malmesey-Nose *Bardolfe* with him. Do your Offices, do your offices: M. *Fang*, & M. *Snare*, do me, do me, do me your Offices.

Fal. How now? whose Mare's dead? what's the matter?

Fang. Sir *Iohn*, I arrest you, at the suit of Mist. *Quickly*.

Falst. Away Varlets, draw *Bardolfe*: Cut me off the Villaines head: throw the Queane in the Channel.

Host. Throw me in the channell? Ile throw thee there. Wilt thou? wilt thou? thou bastardly rogue. Murder, murder, O thou Hony-suckle villaine, wilt thou kill Gods officers, and the Kings? O thou hony-seed Rogue, thou art a honyseed, a Man-queller, and a woman-queller.

Falst. Keep them off, *Bardolfe*. *Fang.* A rescu, a rescu.

Host. Good people bring a rescu. Thou wilt not? thou wilt not? Do, do thou Rogue: Do thou Hempseed.

Page. Away you Scullion, you Rampallian, you Fustillirian: Ile tucke your Catastrophe. *Enter. Ch. Iustice.*

Iust. What's the matter? Keepe the Peace here, hoa.

Host. Good my Lord be good to mee. I beseech you stand to me.

Ch. Iust. How now sir *Iohn*? What are you brauling here? Doth this become your place, your time, and businesse? You should haue bene well on your way to Yorke. Stand from him Fellow; wherefore hang'st vpon him?

Host. Oh my most worshipfull Lord, and't please your Grace, I am a poore widdow of Eastcheap, and he is arrested at my suit. *Ch. Iust.* For what summe?

Host. It is more then for some (my Lord) it is for all: all I haue, he hath eaten me out of house and home; hee hath put all my substance into that fat belly of his: but I will haue some of it out againe, or I will ride thee o' Nights, like the Mare.

Falst. I thinke I am as like to ride the Mare, if I haue any vantage of ground, to get vp.

Ch. Iust. How comes this, Sir *Iohn*? Fy, what a man of good temper would endure this tempest of exclamation? Are you not asham'd to inforce a poore Widdowe to so rough a course, to come by her owne?

Falst. What is the grosse summe that I owe thee?

Host. Marry (if thou wer't an honest man) thy selfe, & the mony too. Thou didst sweare to mee vpon a parcell gilt Goblet, sitting in my Dolphin-chamber at the round table, by a sea-cole fire, on Wednesday in Whitson week, when the Prince broke thy head for lik'ning him to a singing man of Windsor; Thou didst sweare to me then (as I was washing thy wound) to marry me, and make mee my Lady thy wife. Canst y deny it? Did not goodwife *Keech* the Butchers wife come in then, and cal me gossip *Quickly*, comming in to borrow a messe of Vinegar: telling vs, she had a good dish of Prawnes: whereby y didst desire to eat some: whereby I told thee they were ill for a greene wound? And didst not thou (when she was gone downe staires) desire me to be no more familiar with such poore people, saying, that ere long they should call me Madam? And did'st y not kisse me, and bid mee fetch thee 30.s.? I put thee now to thy Book-oath, deny it if thou canst?

Fal. My Lord, this is a poore mad soule: and she sayes vp & downe the town, that her eldest son is like you. She hath bin in good case, & the truth is, pouerty hath distracted her: but for these foolish Officers, I beseech you, I may haue redresse against them.

Iust. Sir *Iohn*, sir *Iohn*, I am well acquainted with your maner of wrenching the true cause, the false way. It is not a confident brow, nor the throng of wordes, that come with such (more then impudent) sawcines from you, can thrust me from a leuell consideration, I know you ha' practis'd vpon the easie-yeelding spirit of this woman.

Host. Yes in troth my Lord.

Iust. Prethee peace: pay her the debt you owe her, and vnpay the villany you haue done her: the one you may do with sterling mony, & the other with currant repentance.

Fal. My Lord, I will not vndergo this sneape without reply. You call honorable Boldnes, impudent Sawcinesse: If a man wil curt'sie, and say nothing, he is vertuous: No, my Lord (your humble duty remembred) I will not be your sutor. I say to you, I desire deliuerance from these Officers being vpon hasty employment in the Kings Affaires.

Iust. You speake, as hauing power to do wrong: But answer in the effect of your Reputation, and satisfie the poore woman.

Falst. Come hither Hostesse. *Enter M. Gower.*

Ch. Iust. Now Master *Gower*; What newes?

Gow. The King (my Lord) and *Henrie* Prince of Wales
Are neere at hand: The rest the Paper telles.

Falst. As I am a Gentleman.

Host. Nay, you said so before.

Fal. As I am a Gentleman. Come, no more words of it

Host. By this Heauenly ground I tread on, I must be faine to pawne both my Plate, and the Tapistry of my dyning Chambers.

The Authorship Question; or, Will the Real William Shakespeare Please Stand Up?

Why has Shakespeare, more than any other author, attracted so many doubters?

by Jenijoy La Belle

Ignatius Donnelly sought to prove Sir Francis Bacon's authorship through cryptographic analysis—uncovering the elaborate code behind which Bacon supposedly hid his identity. Reproduced here from Donnelly's *The Great Cryptogram* (1888) is a page from the Shakespeare first Folio (1623) that Donnelly used as a work sheet.

Over the last 200 years many theories have surfaced that the plays and poems generally attributed to William Shakespeare from Stratford-on-Avon were actually written by someone else. Most of these theories propose that there was some sort of conspiracy or hoax and that the true author of *Hamlet, King Lear, Macbeth*, and all those other masterpieces found it necessary to hide his or her identity by producing and publishing the plays under the name of a minor theatrical manager and actor of no significant talent. Many of these theorists, and perhaps even some of their readers, assume that we know next to nothing about the man from Stratford and that what we do know gives no indication that he was capable of writing great dramas. But, as a matter of fact, as the scholar Alfred Harbage has put it, "we have more reliably documented information about Shakespeare than about Aeschylus, Sophocles, Euripides, Aristophanes, Plautus, Terence, all medieval English playwrights combined, and all but a few of those of the Renaissance. . . . No playwright's life was then written up, and the most remarkable thing about Shakespeare's is that our record of it is as full as it is. . . . The identity of theatre writers [in the 16th century], like that of . . . television writers now, was a matter of public indifference."

One piece of information about Shakespeare sometimes taken as negative evidence is his education or lack thereof. He did not go to a university—and probably attended only grammar school. This raises an important point that affects many anti-Stratfordian arguments—a lack of historical perspective. We're all familiar with grammar schools today and what they teach, but an Elizabethan grammar school would have provided an education roughly equivalent to a modern bachelor's degree in classical literature. Latin was the principal subject taught. Thus, the implication that Shakespeare was an uneducated country boy is very probably wrong. And calling him an "illiterate butcher," as one anti-Stratfordian does, is patently absurd.

Another assumption one frequently encounters is that the Shakespearean plays show such insight into various aspects of human experience that their author must have been a sailor, a soldier, a statesman, a lawyer, an astronomer, a medical doctor—each theorist of course picks his own particular profession. But I think the one opinion about the great plays that even the anti-Stratfordians would assent to is that they are great *plays*, and they were written by someone who understood the living theater, stage performance, and the creation of dramatic plot and character. What sort of person would be most likely to know how to create such works? I suggest that it would be someone intimately familiar with the procedures of the theater of his day, someone who knew about acting, and someone who was a professional playwright. This is exactly what we know William Shakespeare from Stratford did for a living. He was an important member of an important theatrical troupe—roughly equivalent to a repertory company today. He was respected within the industry, he made a good deal of money at this profession, and no one in his own time or for nearly 200 years seriously questioned the authorship of his plays. Conse-

"If Bacon wrote Shakespeare, then Shakespeare (or someone else) wrote the works of Bacon."

quently, the notions that the name Shakespeare is a meaningless veil or that the man from Stratford was an ignorant rustic incapable of writing plays are contrary to the facts.

Let me now turn to a brief history of the authorship controversy. One of the earlier theorists was Colonel Joseph C. Hart, an American, who in 1848 set forth his opinions in a book entitled *The Romance of Yachting*. In the course of relating his adventures crossing the Atlantic, Hart digresses on the subject of Shakespeare. He doesn't know *who* wrote the plays, but he indignantly claims that Shakespeare was "a vulgar and unlettered man" who purchased other people's works and added naughty bits to spice them up. He said he could "easily discover" the parts of the plays Shakespeare wrote by their "filth."

Actually Hart makes a valid point without realizing it. Shakespeare did indeed borrow almost all of his plots from other authors, ranging from classical writers like Plutarch to contemporaries such as Thomas Lodge. The "spicing up," however, includes not just ribald jokes and bawdy puns, but the transformation of prose tales into theatrical events and the virtual invention of complex psychological characterizations for both real and imagined figures whose lives are recounted in outline in his sources.

The most popular authorship theory in the 19th century was that the Shakespeare canon was written by the sly and mighty Elizabethan politician and philosopher Sir Francis Bacon. One of the earliest proponents, William Henry Smith, stakes his claims on the supposed fact that Bacon "had the requisite learning and experience" to write the dramas—even though Bacon is not known to have had any connection with the theater or any experience in play writing. Bacon was indeed a writer and a man of enormous erudition, but his philosophical, legal, and political tracts (often in Latin) bear no similarities to Shakespeare's plays. As the literary scholar George Lyman Kittredge pointed out years ago, "If Bacon wrote Shakespeare, then Shakespeare (or someone else) wrote the works of Bacon." Smith, however, points out that both Shakespeare and Bacon use some of the same vocabulary—such as the word *inkling*. This is a type of argument frequently used to prove authorship, and if two writers can be shown to have used an extensive list of the same words that no one else in the period used, then we might have the beginnings of a good argument that the same person wrote under both names. But simply to show that two authors used the same word commonly used by a great many people of the time is hardly the basis for an attribution—even if the word strikes the modern ear as unusual.

In 1888 the American lawyer Ignatius Donnelly brought to the Baconian hypothesis the full machinery of cryptology—finding in Shakespeare's plays "the most ingenious and elaborate cipher ever presumed to have been constructed by the mind of man." Through this cipher (or secret code) Bacon was indicating his authorship of the plays. Further, Donnelly claimed he had found evidence that Bacon wrote practically all the dramas of the Elizabethan era (almost 800 plays), plus the essays of Montaigne—in French. How Sir Francis also had enough time to compose works under his own name and help govern England remains something of a mystery.

The cryptographic approach proved popular. One believer, a physician from Detroit, extended the theory to the notion that Bacon not only begot the Shakespearean plays, but was himself the son of Queen Elizabeth. Indeed, the Baconian theory has in this century continued to stake out new ground, including the "discovery" that Bacon wrote *Don Quixote*, parts of the King James Version of the Bible, and Edgar Allan Poe's "The Raven." The absurdity of these vast claims is a by-product of the fact that if one looks hard enough and invents an elaborate enough system, one can create ciphers out of any extensive body of writing. Professional cryptographers have discredited the Baconians by showing that their basic procedures can be used to validate obvious impossibilities—for example, that Theodore Roosevelt wrote the Gettysburg Address and that Francis Bacon wrote parts of the Yale University Catalogue for 1909. Perhaps Sir James Barrie

In his book *The Poets' Corner* (1904), Max Beerbohm caricatured Francis Bacon furtively handing the manuscript of *Hamlet* to William Shakespeare. Reproduced by permission of the Huntington Library, San Marino, California.

has the final word on the Baconian hypothesis. Barrie said, "I know not, sir, whether Bacon wrote the words of Shakespeare, but if he did not it seems to me he missed the opportunity of his life."

Another popular theory is that Shakespeare's works were written by a group of collaborators. In spite of her name, Delia Bacon did not strictly follow the Baconian pattern, but in her interminable book of 1857 proposed a collaborative effort led by Sir Walter Raleigh. The unfortunate lady died two years later—"violently insane." Yet, her idea of what the scholar Samuel Schoenbaum calls "a secret society of master wits" did not die. Indeed, the group or syndicate theory had a resurgence in the 1930s, with a cast of characters that included not only a great many Elizabethan courtiers and playwrights but also an anonymous cabal of Jesuits. Actually these proposals do have a certain appeal, for they would seem to account for Shakespeare's infinite variety and do accord with legitimate scholarly suppositions about how Shakespeare may have developed his plays through working together with other members of his theatrical company. However, the Groupists consistently exclude the supposedly illiterate Shakespeare from their proposals—or assign him a very minor role.

In recent years, the favored candidate for the anti-Stratfordian forces has been Edward de Vere, 17th Earl of Oxford—a man of influence and many talents, although nothing in his extant works indicates that play writing was among those talents. The first to set forth the Oxfordian attribution in detail was the admirably named J. Thomas Looney. In his 1920 volume, *"Shakespeare" Identified*, Looney claims that the author of the Shakespeare canon had nine "special characteristics," including "an enthusiasm for Italy" and "a love of music," and, of course, Oxford's life revealed these very characteristics. Looney confesses to one impediment to his attribution. The earl died in 1604—vexingly early in light of the standard chronology of the plays. *The Tempest*, for example, is generally dated to 1611. But Looney triumphantly leaps this hurdle by the simple expedient of asserting that *The Tempest* is a poor effort and could not possibly have been written by the author of the earlier plays. To back up his claims, Looney also published a volume of Edward de Vere's poetry—although in fact some of those verses were not his at all but are works known to be by skilled poets such as John Lyly and Walter Raleigh.

Among the many adherents of the Looney theory was Percy Allen who, during séances conducted by a spiritual medium of "unimpeach-

Sometime around 1800 William Blake painted this intriguing tempera portrait of Shakespeare, based on the famous Droeshout engraving published in the Shakespeare Folio of 1623. Reproduced by permission of the City Art Gallery, Manchester, England.

able integrity," was able to converse with Bacon, Oxford, and Shakespeare. These worthy ghosts revealed de Vere, the Earl of Oxford, as the man who shook the spear. This seemingly incontrovertible proof has been questioned, however, because this same medium had earlier found for the Baconian Alfred Dodd that his favorite was the true author.

The tendency for anti-Stratfordians to be long-winded reached new extremes in a 1952 volume (running to nearly 1,300 pages) by the American lawyer Charlton Ogburn and his wife, Dorothy. As usual, these Oxfordian claimants begin by branding Shakespeare as an "uneducated, unlettered, undistinguished, . . . virtually unknown" lout. The Ogburns find a host of what they call "identity-clues" in the plays—all pointing to de Vere. For example, Rosalind's statement in *As You Like It* that "men are April when they woo, December when they wed" "recalls the fact that Oxford, born in April, wooed when very young and was quite cool by the time of his December wedding." Need I point out that the ability of imaginative literature to "recall" to our minds incidents in our own lives or in the lives of others does not provide solid evidence for authorship. Yet, the Oxfordians soar on apace with such tomes as the 1984 volume, *The Mysterious William Shakespeare: The Myth and the Reality*—almost 900 pages of detailed information knit together with magisterial illogic by Charlton Ogburn's son, Charlton Jr. I have recently learned that there is a Charlton Ogburn III—perhaps waiting in the wings to continue the family tradition?

The actual reason that the Baconian and Oxfordian attributions have never received attention in the academy is that these theories have no merit.

Locally, the supposed "mystery" of authorship has been kept alive in the pages of the *Los Angeles Times* by its arts editor, Charles Champlin. As far as I know, Champlin has not come down firmly in print for de Vere, but his articles on the "Debate Over the Bard" make it seem as though this is a legitimate scholarly issue and that academics are ignoring it for no good reason. The actual reason that the Baconian and Oxfordian attributions have never received attention in the academy is that these theories have no merit. Nor do the proposals for dozens of other rival claimants. The Derbyites advocate William Stanley, sixth Earl of Derby, who at least has the requisite initials (W. S.). Stanley's promoters emphasize the knowledge of court etiquette in the dramas, knowledge which they maintain only a courtier of distinguished ancestry could have acquired. Since Derby didn't die until 1642 (26 years after Shakespeare's death), it's surprising he did not crank out a few more plays. Yet another candidate is Roger Manners, fifth Earl of Rutland. The Mannerists assert that the plays merely echo episodes in Rutland's life. Others champion Christopher Marlowe—even though there is good evidence he was slain in 1593, years before many of the plays were written. But such a minor detail as death is no hindrance to a theory-spinner. The Marlovians simply insist that their pretender did not die, but went into hiding in Northern Italy where he wrote the works now credited to Shakespeare. (I am reminded of some lines from *Macbeth*: "The time has been / That when the brains were out, the man would die, / And there an end. But now they rise again, / . . . And push us from our stools.") The one positive thing that can be said for the supporters of Marlowe is that, in the game of "Choose Your Own Shakespeare," they have at least fixed upon a considerable dramatist and poet instead of enlisting yet another earl.

As a feminist, I suppose I should mention a few of the theories involving women. Queen Elizabeth has been proposed as the author of the plays—as has the Countess of Pembroke, the Countess of Rutland, Mary Queen of Scots, a nun named Anne Whateley (who probably didn't even exist), and Shakespeare's own wife, Anne Hathaway. The arguments supporting these candidates are tissue-thin and often require elaborate scenarios and speculations about Shakespeare's love life, political intrigue in England's court, and other matters for which there is no historical evidence. Often these arguments are preceded by the implicit phrase, "Isn't it possible that . . .?" The only reply is, "Yes, it is possible . . . ," but there is an almost infinite list of possibilities that never happened. The "isn't it possible" argument is simply a rhetorical ploy intended to shift the burden of proof to those unconvinced by an attribution. But the burden must always rest on those who make such ascriptions.

The one positive thing that can be said for the supporters of Marlowe is that, in the game of "Choose Your Own Shakespeare," they have at least fixed upon a considerable dramatist and poet instead of enlisting yet another earl.

However weak and fallacious the anti-Stratfordian arguments have seemed to literary scholars, the Shakespeare authorship issue is a historical phenomenon worthy of study and explanation. Everyone loves a good mystery, and conspiracy theories often capture the popular mind. But why has *Shakespeare*, more than any other author, attracted so many doubters? Nobody produces volume after volume on who "really" wrote the plays of Thomas Middleton, Cyril Tourneur, or a host of other Elizabethan and Jacobean playwrights about whose lives we know less than we do about Shakespeare's. Claims that someone else wrote his plays began in the early 19th century. This was the same period in which bardolatry—the worship of Shakespeare as a transcendent, almost superhuman genius—also began. As Thomas Carlyle wrote in 1840, "there is actually a kind of sacredness in the fact of such a man being sent into this Earth." And the German poet Heinrich Heine once stated, "God himself naturally has a right to the first place, but the second certainly belongs to Shakespeare." I think that the two phenomena, bardolatry and reattribution, are intimately connected. If whoever authored the plays was one of the greatest minds who ever lived, then how could he have been a mere commoner? The exaggerations of bardolatry—for example, the claim that Shakespeare was a world-class expert in a dozen or so fields—have tempted some people to imagine that the author must have been a nobleman of wide experience and high education.

Another quality in the plays themselves can also stimulate speculations on authorship. As the Romantic poet John Keats pointed out, Shakespeare was as capable of creating an evil character as a good character, taking "as much delight in conceiving an Iago as an Imogen." This mobility and multiplicity of personality can lead to a sense that there is no single describable mind operating behind all the plays. Thus, theories of collaborative authorship, however unsupported by the facts, accord with Shakespeare's acknowledged variety. The wealth of theories supporting well over 60 different claimants springs not from solid historical evidence, but from 19th- and 20th-century perceptions about Shakespeare's artistry.

If we stand back a little from the details of the anti-Stratfordian arguments—all those thousands of pages of pointlessness—a few features emerge common to the vast majority. Almost all share a snobbish class-consciousness. Rather than proposing that the Shakespeare canon was written by some other professional playwright, the Looneys, Ogburns, and their many minions always select candidates of aristocratic birth (the bluer the blood the better) or political position. Here again, the worship of Shakespeare's talent leads some to translate artistic ability into literal nobility. But, as Harbage has pointed out, Shakespeare had precisely the social background one would expect of a popular playwright. Indeed, many of the other giants of English literature had similar middle-class origins: "Chaucer was the son of a vintner, Spenser the son of a linen draper, Donne the son of an iron-monger, Milton the son of a scrivener, and so it goes. . . . To be the son of a Stratford glovemaker was not poetically disabling."

Another assumption common to the authorship doubters is that there is no such thing as imaginative and fictive literature written for the purpose of entertainment. Let me explain. Theorist after theorist reads the plays as though they were puzzles—both concealing and revealing secrets about authorship. Such readers believe that the plays are neither fictions nor dramatic recastings of clearly indicated historical events, but are veiled observations about contemporary happenings and people. For example, the younger Ogburn takes it as a firm principle that "the dramatist's first intention . . . seems to have been to write a parable of the times." This notion, of course, drains the plays of their aesthetic qualities, their power to make us laugh or cry. The logical extreme of such an approach is reached in the works of the anagram, acrostic, and cipher schools. The cryptologists do not investigate the literary features of Shakespeare's works and compare them with the literary characteristics evinced by the writings of the proposed candidates. Instead, all literature is reduced to an allegory of authorship. As Schoenbaum has written, "Surely it is madness . . . to believe that the hilarity of Falstaff, the agony of Othello, and the rage of Lear serve merely the puerile requirements of a game of words or numbers: telling an impossible tale of courtly intrigue, conveying signatures or broken fragments of thought. For this the lyricism of *Romeo and Juliet*, the ripeness of *Antony and Cleopatra*? For this the poet's vision, the playwright's craft?"

That the theorists have a very limited respect for the powers of the imagination is further shown by their constant harping on the fact that the plays are filled with all sorts of places, people, and events that the man from Stratford could not have personally experienced. One example of this sort of anti-imaginative—indeed anti-intellectual—argument will suffice. In his boldly titled book *Bacon Is Shake-speare* (1910), Edwin Durning-Lawrence's logic runs as follows:

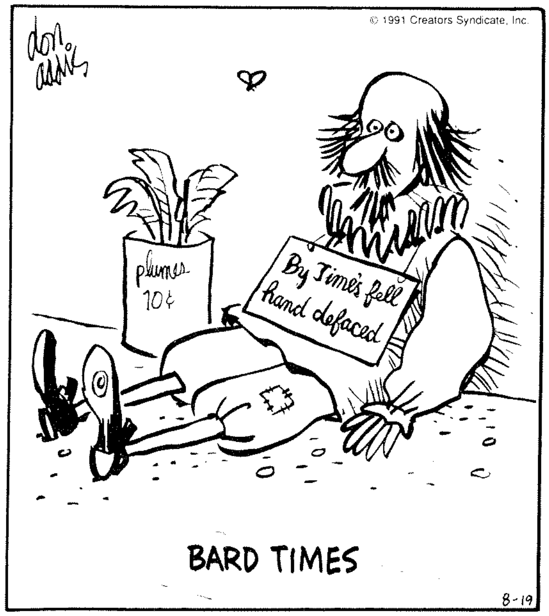
1. There are French soldiers in *Henry V.*
2. Shakespeare could never have seen a French soldier.
3. Bacon, while in Paris, had considerable experience of French soldiers.
4. Ergo, Bacon wrote *Henry V.*

Of course, if the anti-Stratfordians applied this kind of logic consistently, they would have to conclude that *no* Elizabethan could have written *Julius Caesar* and that modern science-fiction stories about life on Mars must have been written by people who actually visited the red planet.

Of course, if the anti-Stratfordians applied this kind of logic consistently, they would have to conclude that no *Elizabethan could have written* Julius Caesar *and that modern science-fiction stories about life on Mars must have been written by people who actually visited the red planet.*

What Shakespeare has to say about Italy often figures large in heretical arguments. The theorists begin by exaggerating the knowledge of that country evinced in the plays, and then conclude that Shakespeare, who never visited Italy, could not have written so insightfully about it. But, not surprisingly, the Earl of Oxford had spent time in Italy and knew it well. In fact, what the Shakespeare plays tell us about Italy is mostly a series of commonplaces that one could derive from any of several books of the period. Further, Shakespeare's dramas contain some basic geographical errors that would be odd mistakes for the well-traveled earl to have made. If de Vere wrote the plays, it seems more than a little strange that he would place a sailmaker in inland

BENT OFFERINGS **By Don Addis**



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Bergamo, describe a nonexistent waterway between Milan and the sea, and have characters board a ship in landlocked Verona. And why does Bohemia have a seacoast if the author of the plays was an educated aristocrat who traveled on the Continent? In short, the geographical knowledge shown in Shakespeare's plays provides no evidence of authorship. And the same could be said for a host of other realms of learning—such as sailing, warfare, law, and medicine. When stripped of their rhetoric and dramatic artistry, Shakespeare's thoughts on these subjects may be wise, but they are not original contributions to the Renaissance body of knowledge in these disparate fields. The author of Shakespeare's works was a great writer—but he was not a great navigator, lawyer, or physician.

Besides bashing Shakespeare the man, the anti-Stratfordian forces enjoy denigrating academic scholars. The Baconians, Oxfordians, and their devotees take the fact that no respected academic *literary* scholar has ever believed in their fantastical theories as an indication not of the weakness of those theories, but of the dull wits of academics. Some go a step further and add to their conspiratorial proposals about authorship a modern conspiracy among academics to deny all claims against the man from Stratford. The nonbelievers believe that the community of professional scholars has some sort of profound investment in Shakespeare's authorship. I suspect that shopkeepers in Stratford-on-Avon have such an investment, but I have never understood how academics do. Indeed, if I could come up with valid and significant evidence that someone other

Indeed, if I could come up with valid and significant evidence that someone other than Shakespeare wrote his plays, I could achieve instant fame and fortune in my profession.

than Shakespeare wrote his plays, I could achieve instant fame and fortune in my profession. Plus, I could appear on the "Phil Donahue Show." But, alas, none of us has ever found a shred of such evidence upon which to build an argument.

I realize that to come to this conclusion and to argue against the multitude of authorship proposals will have no effect on those who deeply believe that Bacon or de Vere or someone else wrote Shakespeare. The theorists are sincere, they are dedicated, they are irrepressible, and they will take whatever I say as proof of my own pigheadedness, not of theirs. But their views are, as William Shakespeare of Stratford wrote, "a tale / Told by an idiot, full of sound and fury, / Signifying nothing." □

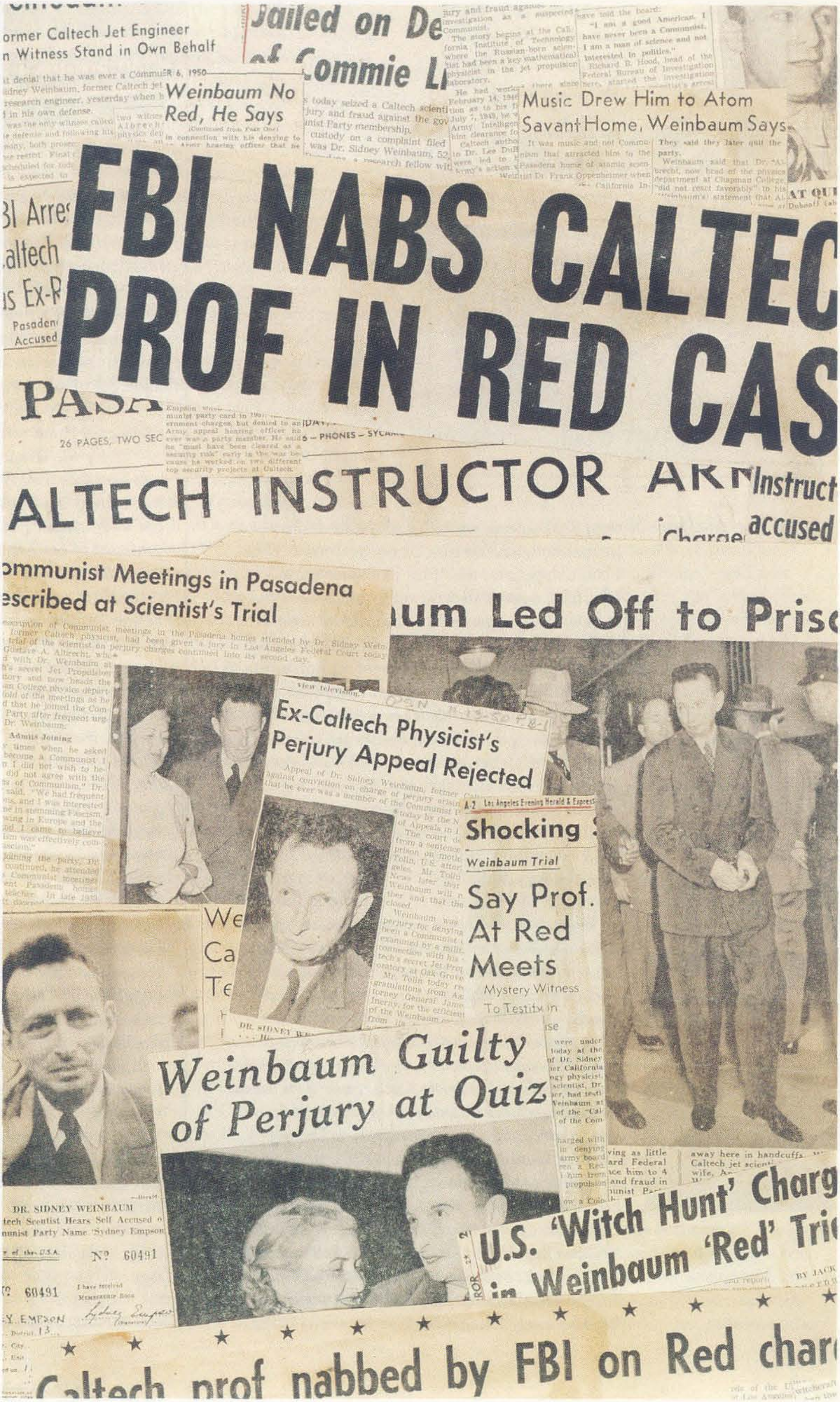
Jenijoy La Belle has been professor of literature since 1988. She received her BA from the University of Washington in 1965 and her PhD from UC San Diego in 1969, when she joined the Caltech faculty. She has published extensively on William Blake, but she does indeed teach Shakespeare to Caltech undergraduates. This article is based on a talk originally delivered to the Friends of the Caltech Libraries last April and to the Seattle chapter of the Alumni Association in July. Historical information in the article is based in part on the following works: Edwin Durning-Lawrence, Bacon Is Shake-speare *(1910); H. N. Gibson,* The Shakespeare Claimants *(1962); William F. and Elizebeth S. Friedman,* The Shakespeare Ciphers Examined *(1957); Alfred Harbage,* Shakespeare Without Words *(1972); George McMichael and Edgar M. Glenn, eds.,* Shakespeare and His Rivals *(1962); S. Schoenbaum,* Shakespeare's Lives *(1970); and Frank W. Wadsworth,* The Poacher from Stratford *(1958).*

Calculating Shakespeare

Even Caltech was involved, at least once, in the Shakespeare-authorship question. Sidney Weinbaum, who worked during the 1930s in Linus Pauling's lab doing quantum mechanical calculations of molecular bonds, tells the following story in his oral history (another more notorious chapter follows on page 30):

"At that time, they decided they will have to use electric calculators instead of hand calculators. And so every firm wanted to sell their electric calculators, and two of them gave them to us free to try out.

"There was a man in Los Angeles who was sure that Shakespeare was not written by Shakespeare but by whoever it was—I don't remember now. And a Caltech professor of mathematics, Clyde Wolfe, was doing calculations for him; one of his specialities was theory of probability. So he was looking at repetition of words and things like that, to show that it was not the same as the known writings of Shakespeare. I understand that this man had about six or eight calculating machines, and he had a little swivel chair in the center there, so Wolfe could swivel his chair and go from machine to machine. However, when we got the electric calculators, they were much more modern than what he had. So he somehow found out about it, and he came to take a look and to see how they worked. Well, a few weeks passed by. One day I came back to work, and the machine was stuck; it wouldn't work. So I called the company; 'That's impossible; it was in perfect order.' The company representative came back and he tried it out; it didn't work. He said, 'What did you do with it? Did you try to take it apart or something like that?' Well, they gave us a different machine. Months passed by, and I met Wolfe on the campus. And he says, 'I came one day to try out your machines. Nobody was there, so I just worked for a while. And then I wanted to know how it is put together, so I took it apart, and then I put it back together.' So the company was right."



Former Caltech Jet Engineer
On Witness Stand in Own Behalf
Jailed on De
of Commie Li
Weinbaum No
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Savant Home, Weinbaum Says
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Oral History

Sidney Weinbaum
Politics at Mid-Century

You know, people now really cannot realize what those days were like.

Local newspaper accounts of Weinbaum's arrest and trial must have been a public-relations nightmare for Caltech, where he had continued to work as an instructor in chemistry after losing his clearance at JPL. Caltech hired a lawyer to defend Weinbaum before the military board but took no official action on his behalf thereafter. Several faculty members then raised funds for his defense.

Sidney Weinbaum was born in 1898 to a middle-class Jewish family in Kamenets-Podolsk in the Ukraine. As a schoolboy he was interested in mathematics and also became an accomplished pianist and chess player. His university education at Kharkov Institute of Technology was interrupted by the Russian revolution, but a job inspecting sugar beet plantations near the border enabled him to flee first to Germany and then to Poland in 1921. An aunt in Los Angeles and a cousin enrolled at Caltech urged him to keep going, and after being accepted as a student at Caltech (and refused Polish citizenship), Weinbaum emigrated in 1922. He taught piano to pay his way and organized chess teams that included Professors Harry Bateman and Ralph Smythe (Weinbaum was LA chess champion twice in the twenties). In 1924 he earned his BS, and after a series of draftsman jobs and returning briefly to Russia to marry, he came back to Caltech in 1929 to work as a technical assistant for Linus Pauling, whom he had known earlier in advanced math courses. Caltech awarded him a PhD in physics in 1933.

During the 1930s Weinbaum, along with a number of his better-known colleagues, was active in leftist causes that returned to haunt him during the "Red scare" that began in the late 1940s. Although several Caltech people were accused of Communist Party membership, only Weinbaum went to prison for a substantial period of time; one was jailed briefly for contempt, while others cooperated with the FBI or left the country.

In August 1985 Mary Terrall interviewed Weinbaum for the Caltech Archives. Portions of that oral history dealing with his political activity and eventual arrest and trial are excerpted here, beginning with his work for Pauling doing the complicated calculations of crystal structure.

Mary Terrall: Pauling was working on x-ray crystallography at that time?

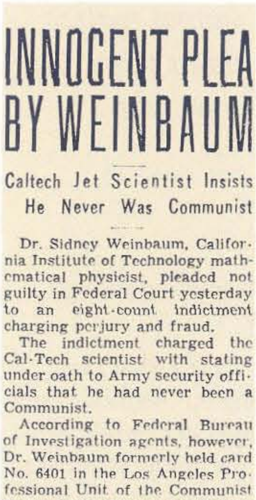
Sidney Weinbaum: Yes. That's what I was going to do. He was working on chemical bonds. Because the crystals are formed in a certain definite way, you can calculate the distances between the atoms. Then why is it in some crystals the distance is more, in other crystals less? So that's experimental data; if you develop a theory for that, you can check and see whether it is correct. Or, if you already have a theory, then you can say the distance should be so-and-so in this crystal, within a certain error.

Pauling had just come back from a fellowship in Europe [1926–27]. I think he worked with Arnold Sommerfeld; I don't remember where he worked in Europe. But anyway, those were the days when quantum mechanics appeared. And Pauling became interested in whether quantum mechanics could not also be applied to chemistry, to calculate the bonds. In that day only very simple things were done— things that had just one electron in them—but he was interested in molecular problems. And he had a theoretical physicist, Boris Podolski, working on it. He was a Caltech PhD and came to Pauling's lab after spending a year or two in Russia. You know, in the thirties there were no jobs in the U.S. The problem requires some very extensive algebra. So, besides doing crystal structure, I was also checking his algebra.

MT: When you decided to enter a degree program, how did it happen to be in physics rather than in chemistry?

SW: Because my background was not in chemistry and I wasn't particularly interested in it. Also, I was interested more in theoretical things. I graduated in physics and engineering, not in chemistry. And in the years in between, there were big changes in physics, from quantum theory to quantum mechanics, that I missed. And here I came back, and suddenly I got into that and it was very interesting to me. I sort of got acquainted with particular phases of quantum mechanics. And as I say, the main thing was that actually I was doing work in physics.

MT: Was Pauling politically active in the thirties?


INNOCENT PLEA BY WEINBAUM

Caltech Jet Scientist Insists He Never Was Communist

Dr. Sidney Weinbaum, California Institute of Technology mathematical physicist, pleaded not guilty in Federal Court yesterday to an eight-count indictment charging perjury and fraud.

The indictment charged the Cal-Tech scientist with stating under oath to Army security officials that he had never been a Communist.

According to Federal Bureau of Investigation agents, however, Dr. Weinbaum formerly held card No. 6401 in the Los Angeles Professional Unit of the Communist Party, under the name of Sydney Empson.

The not guilty plea was entered after Judge Ben Harrison had denied motions of Attorney Ben Margolis to dismiss the indictment on technical grounds.

TRIAL AUG. 22

Margolis formerly represented a group of local Communists who were convicted a year ago on contempt charges for refusing to answer questions before the Federal Grand Jury regarding local Communist activities.

Judge Harrison set tentative trial date of August 22 for Dr. Weinbaum.

Until a year ago, the Russian-born scientist was a research fellow and a key figure in the Army's highly secret jet propulsion laboratory at Cal-Tech.

Following his not guilty plea, Dr. Weinbaum said he was "utterly bewildered" by the charges and insisted that he had never been a member of the Communist Party.

Pending trial he is at liberty under $5000 bond.

SW: At first he was not politically active at all. Even later, I didn't have any political discussions with him. Did I tell you how he got interested in politics? This I heard; I don't know it first hand. Eventually the Paulings had a big place in Sierra Madre or wherever it was, and they had a Japanese gardener who worked for them. And when the war started, and all the trouble started with the Japanese, he came to Pauling very distraught, and Pauling said, "You know, you have no family; you can use that shack, and if you wish, you can live there." So he did. The house stood on a little hill, and the garage was at the bottom. Mrs. Pauling came down one morning to take the car out, and there were all kinds of four-letter words—and "you Jap lovers," and things of that sort. So they called the sheriff, and the sheriff came and took a look at it, and said, "Well, isn't that true?" I was told that that was the beginning of Pauling's political awareness.

But I was quite interested in politics. That's basically why I got into trouble. I circulated very successfully two petitions in Caltech during the middle thirties or early thirties; I don't remember the dates. One was for recognition of Russia; and the other was against the Criminal Syndicalism Act. And for that second one I had very good backing, because Theodore Soares, who was a retired theologian on the campus in the thirties, had an office in Dabney, the humanities building. When somebody gave me a leaflet about this thing, I saw he was among the people who sponsored it. He belonged to a certain parish, and I even went to him to ask whether he knew anyone who might be willing to sign this petition. And in a couple of days I got from him a fairly long list of people. In Caltech, I certainly didn't go to people like Millikan, and I didn't even go to Pauling in those days for his signature, because he was directly my boss, so I didn't want to do it. And, as I say, at that time I didn't even know where he stood politically. I knew that he was fairly liberal, but you never can tell. For example, [Alfred] Sturtevant was very liberal, but one of these petitions he refused to sign; I don't remember which one it was.

MT: You said you knew the Paulings socially. So your friendship would have been more around cultural things than politics?

SW: Yes. And children, food, things of that sort. We went to their place. I didn't have a car, so it was easier for them to come to us. Several times a year we would get together. But our real social life was in the biology department. Calvin Bridges became a close friend. He was politically very liberal.

In those days Caltech needed money very badly. When Einstein was there, they had a dinner for the people with money. It was a thousand-dollar dinner; Einstein was the guest of honor. And they invited all the top people on the faculty. Calvin Bridges was not on the faculty, but he was also invited. He sat between two ladies, and all during the dinner he discussed the question of the necessity to redistribute the wealth in the United States. He probably was never invited again.

MT: Going back to your graduate work, did you take a class from [Richard C.] Tolman? Didn't he offer a class in quantum mechanics in the chemistry division?

SW: I don't remember. Maybe it was before I came. But I had a very good relationship with Tolman. Also, Tolman was the first man I always went to for his signature on many of these things, because Tolman was a very liberal person.

These political activities never came up against me. But the particular accusation [of Communist Party membership] that was made in 1950, came up for the first time, I would say, about 1941. The war had already started in Europe, and I know that I had just started working for Bendix Aviation. By the way, I was recommended by Professor Sorensen for that job. Anyway, a friend of mine—well, I don't have to hide the names now—Frank Malina, came to me quite perturbed. Frank Malina was a very close friend. He told me that he was at a big party in the aeronautics department, and Clark Millikan had gleefully told him that they were informed that he and I, and another two or three names, were members of the Communist Party. So I went to see a lawyer friend of mine and said, "What can I do?" I was sure that with an accusation like that they were going to refuse me clearance. But no! I was cleared throughout all these years; from '41 to '49, when the trouble


FBI ARRESTS CALTECH SCIENTIST FOR PERJURY

Metropolitan Pasadena STAR-NEWS

FRIDAY, JUNE 16, 1950 FIVE CENTS PER COPY

DOWNTOWN TRAFFIC IN TANGLE

Charged With Denying Communist Party Ties

FBI Takes Dr. Sidney Weinbaum Into Custody at Pasadena Home

But I was quite interested in politics. That's basically why I got into trouble.

began, I was cleared for top-secret work. So why did it suddenly come up? I have my explanations for that.

MT: What was this accusation by Clark Millikan based on? Did you know him?

SW: Obviously the FBI gave that information to the authorities in Caltech. And Clark Millikan was way at the top, you see. And I say "gleefully" because I presume there was not too good a relationship between Frank Malina and Clark Millikan. Anyway, I never expected anything to happen to me so much later.

So I got my PhD. I just worked over my thesis a little bit, and it was my first published paper. That was the first application of quantum mechanics to molecular problems—not just when there is one electron, but when there is a whole molecule. And then the same thing was repeated with the helium ion; that also had only two electrons. Pauling had some other ideas, and I spent a lot of time on them, but the amount of work proved to be too great.

MT: Did you continue working on this, then, after you got your degree?

SW: Yes. I continued working with Pauling on this type of problem or any other problem that required mathematics. I was at that time already a research fellow. I came in '29 and stayed until '41.

MT: Did most of the people that you knew also have Caltech connections?

SW: No. My first more or less interesting connections were those that I made through chess when I came here—some very interesting friends, and some just acquaintances. Then, besides that, I met some friends through [Paul] Epstein. And I also knew the people in the English department pretty well. I went regularly to the group that met at [Clinton] Judy's house. It lasted only a few years; I don't remember why it disbanded. So I knew Judy very well. Any time there was something musical going on—for example, when one of these modern composers, Krenek, gave a talk at Caltech, Judy invited my wife and me to dinner. My contribution to these things at Judy's house was a talk on modern trends in music.

MT: What was the political atmosphere around Caltech, and the climate here, in the thirties?

SW: I would say it was indifferent, as I found out when I went around with the petition for recognition of Russia. I was amazed how little people knew about Russia, about the revolution, about what the situation was. And not just one, but several of these people in the faculty thanked me for talking to them about this. And even our close friends, with the exception of Calvin Bridges, were that way.

MT: So did you find yourself arguing with people?

SW: No. I never argued with people. First of all, they were not interested. And my political interests became finally in support of Roosevelt. That's where I met a number of Communists and a number of them became friends. Because actually, in all these Democratic organizations, Los Angeles Examiner * Wed., Sept. 6, 1950 Sec. I

DEFENSE ENDED BY WEINBAUM

Dr. Sidney Weinbaum ended his Federal perjury trial defense late yesterday after steadfastly denying he ever was a Communist Party member, or even knew a Red cell existed on the California Institute of Technology campus.

Day-long arguments to the jury by the prosecution and attorneys for Dr. Weinbaum, a 52-year-old Russian born scientist, will open this morning.

U. S. Judge Ben Harrison announced he will deliver his instructions to the jurors tomorrow.

Weinbaum, in short, clipped phrases, sought to convey the impression to the jury of 11 women and one man that Government witnesses were his enemies for various reasons, in his final testimony.

FORMAL DENIAL——

The former Caltech faculty member took the stand after U. S. Atorney Ernest A. Tolin dismissed a perjury accusation and another of fraud on which Weinbaum was indicted.

This left Weinbaum still facing three counts of perjury and one of fraud, based on his denials that he ever was a Communist Party member.

Under questioning of Defense Attorney Ben Margolis Weinbaum formally denied he ever had been a Communist or had used the party alias of "Sydney Empson."

He told of his political interests in the 1937-39 period when the Government charges he was a Communist.

"I was disturbed over the growth of Fascism and the persecution of science and the Jews and felt that the rest of the world should unite to

the people who did the work were the Communists. The Young Democrats in Pasadena, for example. The leadership either was Communist or was somebody who knew Communists. Because otherwise there was no organizational ability and no desire to put any time in. At best, people would come to a meeting. Some work had to be done.

MT: Did you get to know Frank Oppenheimer in this context?

SW: The whole Oppenheimer thing for me is very interesting. Frank Oppenheimer may be one of the reasons why a lot of people got into trouble. Because like all rich people, well, they don't give a damn about anything.

He joined an open group. He was absent-minded enough that when he sent a suit to be cleaned, he had his Party card in that suit. I think he was pretty open among students, not necessarily saying that he was a Party member, but to the extent that when there were big meetings in Los Angeles—he had a truck in those days; to show how proletarian he was, he had a little truck—he would take some of the students in the truck to these meetings, and things like that. Well, naturally, he became a friend after I met him. The most amazing thing was to find him among the witnesses for the government case against me.

We also became, through Frank, very friendly with Robert Oppenheimer. Robert spent very little time in Caltech. A couple of times we invited him and he came to dinner. He used to drop in quite often, because we lived very close to Caltech, so on his walks he would just stop to say hello. When the thing happened to me, I thought that a few people should know about it. And I wrote to them so that they would know, from the tone of my letter, that I was not going to say anything that might be prejudicial to them.

MT: You were talking about Oppenheimer and how he could afford to be so open about his involvement. Wasn't it also true, though, at that time that no one had any idea what was going to happen later?

SW: But you see, actually, maybe the authorities didn't even know about Frank Oppenheimer; anyway, Robert Oppenheimer was such a respected person that they might not look that way at his brother. So far as Robert Oppenheimer is concerned, I think that all the books and the series on television and so on missed the most important thing about him—of a man who thought that he was God.

Later on he said that he had been a fool; he practically beat on his chest about what a fool he had been. You see, this was such a tragedy for him. He used to look down on everybody. Though nobody spoke about this openly, in Caltech most of the professors didn't like him because he always behaved as if he were so much above them. And, by the way, when he became close to the Communist Party, his manner did change. When he gave a talk, you could understand what he was talking about. Before that, if he gave a talk I think he made it a point to talk so that maybe one or two people would understand him, because he didn't care about the rest of the people.

When I thought that I had only lost my job in the Jet Propulsion Laboratory, before I was actually arrested, I wrote to a few people, and Robert Oppenheimer was among them. I thought that Robert may find a possibility to suggest a job for me.

Well, the next weekend, the doorbell rings. And here he was with Mrs. Tolman. In other words, he came with a protector for the interview. And we talked together. I couldn't talk completely openly, but it was clear to Robert Oppenheimer that there was no danger to him from me. I never heard from either Frank or Robert again. Never! And they knew that I was the sole supporter of my family. I know that my family would not have accepted any help from them. But they didn't make an attempt, because, among other things, they were scared. They wanted to protect themselves as much as possible.

The thing with Oppenheimer was that he

Why was I the victim at that particular time? And why was it I and not somebody else? Things were happening in the East, all the arrests of spies and so on.

never got a Nobel Prize, because he never contributed anything new. He right away understood a new idea and knew how to go further with this new idea and so on. But this was such a big loss for him. These people that he'd looked down upon, some mere experimental men or something like that, got a Nobel Prize but he did not. So when the opportunity to build an atomic bomb came, that was his great contribution. And when they took his clearance away, what did he need that clearance for anymore? He had made his contribution.

MT: How did it happen that after the war you came back to JPL?

SW: Actually it was [H.S.] Tsien who was responsible for my getting the Jet Propulsion Laboratory job, because [Pol] Duwez was the head of the materials section at JPL, and he was looking for a mathematician. Also, he was a very fine cellist.

MT: Did you have to get clearance to get this job originally?

SW: Well, this is the whole thing. I think I have mentioned that I expected, on account of these rumors that had been circulated in the past, that I wouldn't be cleared for it. But I had no trouble at all. During the war I even spent two weeks at the Wright field in Dayton, Ohio. There they recheck these things, but nothing happened.

But this is the question: Why was I the victim at that particular time? And why was it I and not somebody else? Things were happening in the East, all the arrests of spies and so on. And I think that the FBI here felt that it should show some activity—so they went through their records. Some of the people were already too important, like Frank Malina. But here was a man like myself, who was, first of all, Jewish, and second, still had a mother and sister in Russia. And I knew that something was brewing, because even some of these lawyers remembered that the so-called perjury things have a three-year statute of limitations. Suddenly in 1949 I got a telephone call from the JPL office that they were sending me another application to fill out. And when I asked why, they just got mad on the phone. So I did it, but I found it very strange.

MT: So had you filled out something similar to this in '46, when you first came to JPL?

SW: You see, it was three years old.

MT: Had they asked you these questions in '46?

SW: Yes, the same things. You had to show all the organizations that you used to belong to. The thing is that in general, it was just like the loyalty oath at the university in Berkeley. Political things were not supposed to be asked. But they selected me as somebody that maybe pressure could be put on. Their approach was this: "We know that you like your family very much and that you would like to support your family. And if you work with us and tell us who are your friends, though you cannot work anymore at the Jet Propulsion Laboratory, we can arrange it so that you will get a satisfactory job someplace else." I had an offer: if I just name names, everything will be smoothed out.

MT: So it was with this new form that you filled out that they were able to accuse you of perjury?

SW: Yes. But they had to formulate the things in many different ways, because the prosecuting attorney said that they didn't quite know what they would be able to prove. It had been tested legally only once before. So it was all a purely legal kind of thing. The main thing that they were asking me was to name the people with whom I had been associated. And when they saw that I didn't go along, they obviously decided that I was the proper person for them to start a court case here. It was unfortunate that they didn't have any real spying case or something of that sort here.

MT: So in '49, they took away the clearance that you already had?

SW: First, they took the clearance only to top secret. I didn't appeal, because I didn't care if I didn't have that. For example, I asked for the reports that I wrote to be declassified, and they appeared afterwards in the *Journal of Applied Physics*. I felt that something was brewing, but I never expected it to come to this kind of thing.

MT: So it was after the hearing in '49 that you lost your job?

SW: And the Institute appealed my case to the military board. They said, if I don't appeal, I cannot work in Caltech anymore. This is where the trouble comes with Caltech. They were under pressure. So the charge of perjury came from my testimony before the military board—because I denied the charges before the military board. Professor [Earnest] Watson was a witness for me and also Verner Shomaker.

There was always a lot of incorrect information—that's putting it mildly—about me. The only thing that personally worried me in all that time was that there were two or three things that came up that couldn't be known about me

8 Part I—THURS., AUG. 31, 1950 ★ Los Angeles Times

TESTIFIES—Dr. Frank Oppenheimer, Caltech graduate who told court he did not recall if Dr. Sidney Weinbaum had ever attended Communist meetings at his home. Times photo

WEINBAUM TRIAL

Continued from First Page

numbered 60491 and a green-covered book with the same number stamped on it.

"Are these like yours?" asked Tolin.

"Yes, they are the same thing—it is the same type of book," the witness replied.

Used by Weinbaum

"This book shows the name Sydney Empson," Tolin pointed out to the witness. "Do you know anyone of that name?"

"Dr. Weinbaum used that as his party name," the witness testified.

Rosanoff said that he was told by Dr. Weinbaum that his party responsibility consisted of keeping his eyes and ears open and encouraging anyone who talked along the party line and if they were sufficiently interested to call them to the attention of other officials.

He said that his own party card was in the Caltech unit which was known as Professional Unit 122 of the Communist Party of America. He said that following the time he joined the party early in 1938 he had discussed numerous possible party prospects with Dr. Weinbaum and others. One of these was no

WITNESS—Richard A. Rosanoff, who testified Dr. Weinbaum admitted Red ties. Times photo

And he said in his letter that the worst that he could think of me was that maybe I voted for Roosevelt.

personally except among very close friends. And I was wondering whose hand I was still shaking who . . . Up to today I do not know who the person was.

MT: But when you were actually arrested, that came as a surprise to you?

SW: Yes. But at the same time, I was more or less prepared for a thing like that. But one doesn't think about these things; one hopes that it won't happen.

MT: Just to go back to the trial for a few minutes, you said that you didn't know during the hearing who it was that was talking to the FBI. But, in fact, there were these people who testified against you. The major portion of the case really rested on this testimony, right? There was Gus Albrecht [previously at Caltech and JPL], Frank Oppenheimer, and Jacob Dubnoff [a senior research associate at Caltech]. What is your interpretation of why these people talked? Do you see it as just being pressure from the FBI?

SW: Well, I knew that Dubnoff was a Communist, and Oppenheimer, and many others that I was friendly with in those days. But the charge was worded that I had been a member. The other charge was that I got some people to join. And the third was that I abetted the Party or something. Well, the third one may have been right. But you see, the whole thing was not that. They didn't give a damn; they just wanted to have a case, to put the FBI on the map. And also, they thought that I would crack, like Dubnoff for example. The day before Dubnoff's testimony, it appeared in the newspaper that he refused to testify; and the next day, he was the first witness!

But none of these people claimed that I had been a Party member. What I say is this: if I were, one way or the other, then they lied. If I was, then they knew that, they had to know; then, by going all around, but not naming it, they are the ones that perjured themselves. But this is also quite true, that whether I belonged or not was not the problem.

One name that came up was that of the wife of Rudolf Schindler, the famous architect. They said she visited our house and stayed for a long time; they saw her car parked outside. Certainly she did. We were very friendly with her. I don't remember now what was the origin of our meeting. Speaking about the people that I knew—Richard Neutra, you know, the famous architect? Well, his wife played the cello and sang. They were friends of some distant relative of mine, and I have accompanied her a number of times. Actually they lived for a while in a house that Schindler built. The only time I visited the Neutras was in that house, before I even knew of Mrs. Schindler's existence. So, they said, didn't I know that she was a Russian agent? You know, imagining that woman to be an agent was like—I don't know. So it is very, very funny.

MT: Well, it's true that maybe the point wasn't whether you had been a member or not; but, in fact, that was what they had to prove, in order to get this conviction.

SW: So you see, the way the things were worded, even if I wasn't a member, they put the word *abetted*, because it covers the situation and I was certainly working with them. So, if that's a bad thing, in that way, that was true.

MT: In order to convict you of perjury, they had to prove that you had been a member of the Party though.

SW: But it was not only this thing, but also these two other things. In the atmosphere of those days, there was very little question about being convicted. The only explanation is that at that time the climate was such that if you don't have a case of some sort—these are the sort of things I didn't think of at that time.

MT: So, what kind of a defense could you have, then? Did you call witnesses in your favor?

SW: No. But that was an interesting thing. My lawyer wanted to read the letters that had been submitted to the military board. And the district attorney got up and objected to that, saying, "Let these people appear here as witnesses." However, in his speech, he had quoted some things from the military hearings. So the judge, very reluctantly, I must say, said that he had to allow this. And so my lawyer read some of the selected letters. One was from a man by the name of Miller. He was some kind of a technical electronics man in the seismological laboratory; he was a chess player who played years before on my team. During the war he built up a factory and made a lot of money. I wanted to have a letter from someone working for the military, and I asked him for a recommendation letter. And he said in his letter that the worst that he could think of me was that maybe I voted for Roosevelt.

Frank Oppenheimer did very foolish things. You know, he had a hearing before a congressional committee. Oppenheimer obviously didn't have a very good lawyer, or the lawyer was mostly trying to protect Oppenheimer. When they asked Frank about a number of people in Berkeley, he claimed every time that they were not Communists—that he knew that they were not in the Party. But when the names of Malina, and Tsien, and myself came up, then he refused to answer. By this, you see, he singled us out. Well, because somehow nothing about his brother had yet come up in Caltech— maybe it was on account of that.

MT: Did you see the trial as kind of a foregone conclusion, the outcome of it?

SW: Yes. My lawyer warned me that there was little chance. The district attorney called for ten years, because he grouped the counts into two different bunches. But the judge said that they are all based on just one thing, so the maximum was five years, and he gave me only four. I think I told you the story of the marshal who told me that he was sure that I would get a suspended sentence. So the judge said, "You know, I didn't even give him the maximum. I had to give him four years because he's going to get parole after a short time, and I want to make sure that he is not going to leave this country and give his services to our enemies. By that time, science already will develop further." In other words, my knowledge would be already obsolete. And again, there was that offer, that if I go and tell about people, I could get a suspended sentence. But my scientific career was finished anyway, you see. You know, people now really cannot realize what those days were like.

MT: I know for myself, just reading the newspaper clippings—even though I know something about the period—it's shocking.

Weinbaum Case Jail Term Test

Highest Court May Rule on Refusal To Testify

Whether Communists, ex-Reds or suspected Communists can get special treatment from the courts by refusing to testify regarding their present or past affiliations appeared today to be headed for a ruling by the highest courts of the country.

An appeal was being framed today from the decision of District Judge Ben Harrison to send Dr. Eugene Brunner, research chemist, to jail for six months for contempt in refusing to answer questions in the federal court perjury trial of Dr. Sidney Weinbaum.

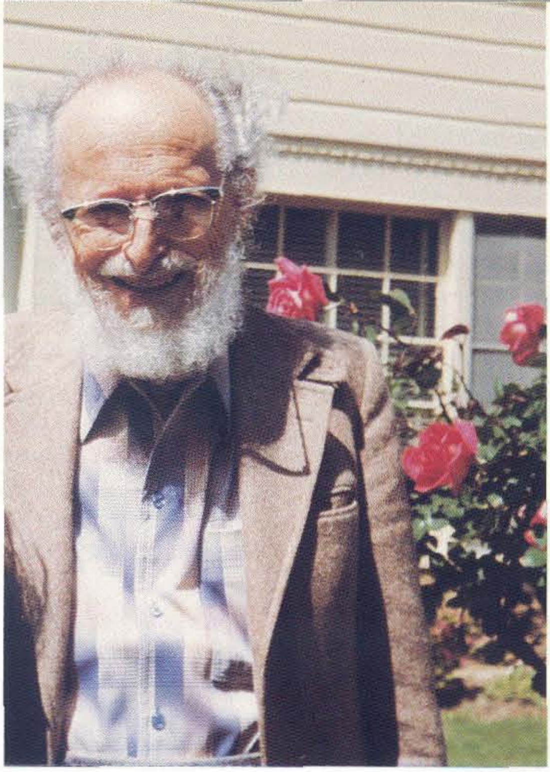
Brunner, 39, formerly a graduate student at Caltech, was called as a prosecution witness in the trial of Dr. Weinbaum, former physicist in the jet propulsion laboratory at the institute, and refused flatly to answer these two questions:

"Between 1937 and 1939 were you a member of the Communist Party in Pasadena? During the period, did you ever see Dr. Weinbaum at Communist meetings?"

BAIL IS DENIED

Judge Harrison then denied a motion by Brunner's lawyer, William Esterman, that Harrison disqualify himself. He also denied the attorney's request to fix bail pending appeal "because I find that this contempt was deliberate and wilful."

Earlier Dr. Jacob Dubnoff, senior research assistant at Caltech, admitted on the witness stand that he had been treasurer of the "Caltech branch" of the Communist party prior to 1940. He said that his "party name" was "John Kelly," and that he had collected dues from other Pasadena Reds, but he "couldn't remember" whether Weinbaum had been one of them.



Sidney Weinbaum last year at his Santa Monica home.

SW: I didn't read everything. The things I read about myself were actual things that happened in the court, sort of a summary without too much detail. And also, one or two articles in the beginning that told about me in general, that were nice and favorable. There was nothing prejudicial in any way. But seeing that headline in the newspapers, that Weinbaum faces four years in prison—that wasn't very pleasant. No, those were not very pleasant days. A few telephone calls, some abusive. A letter when I was in prison, waiting to be sentenced, was very, very abusive.

When you get to prison, by the time you get through admission, the prison is already dark. And so they take you and give you a pillow or something like that, and push you into a two- or four-man cell. And I had to lie on the floor with my head to the toilet. You see, some of us were only transients, because we were federal, and that was a county prison. One of the trusties who had obviously been there for a long time came over and said, "Oh, I have the two most famous prisoners in my place." Who was the other one? It was a man who had tried to blow up a plane because his wife deserted him or something like that. I talked to that man quite a bit, but he was not quite there. You see what "fame" is; for the trusty we were both famous.

Now looking back, I must say that I consider I led an interesting life in many respects.

One of the better-educated guards—officers they called them—told me I was there when Mickey Cohen, the underworld king, was there. This guard said, "All these people who are here, when they come out, you know what they will be boasting about? They will say, 'I knew Mickey Cohen, and I knew Dr. Weinbaum.'" So I had very good company.

Not that there were not some disagreeable things that would happen; but I had to learn to handle these things. And there were not too many personal things that I had trouble with, either with the inmates or the prison.

When the time for my parole came, everybody that I talked to among the guards and some of the younger people said, "Oh, you're certainly going to get parole." Well, I knew otherwise. Because when I appeared before the parole board, they started to ask me who my friends were. The chairman of the parole board said, "Why should we give you parole if you don't know how to cooperate with us?" So, you see, it was a matter of punishment and nothing else.

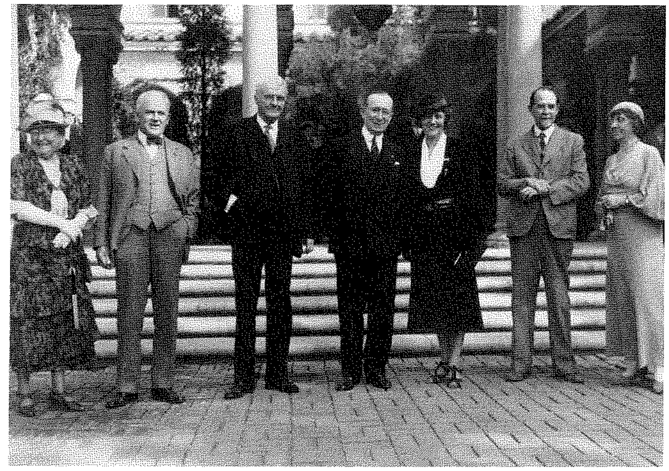
When I was refused parole, a man who was serving 25 years for being involved in a very famous robbery of a truck that was going to Lockheed with the payroll was working in the officers' dining room. When I was denied parole, I noticed that there was a little package on my bed. When I went in, it was a very nice ham sandwich, the kind we didn't get, and a note was attached: "You may have no standing with the parole board, but you have a standing with me." The reason for that was that I didn't succumb to the FBI. And I was never again interviewed by them while in prison.

I don't know if I mentioned to you that the inmates even elected me as their representative for six months. So I came out of prison ready to become emotionally stabilized; and I did. And I can talk about these things. And also, somehow I tried to fit things that happened to me into the life of the country in the particular time, and also find out why I react emotionally in one way or another. One lives a long life.

Now looking back, I must say that I consider I led an interesting life in many respects. I met so many different people. And where I got my greatest education was in prison. Prison was the thing that is responsible for my living to be an old man. I came out a different person. □

Weinbaum served three years of his sentence with a year off for good behavior. On his release, an old friend from his chess-playing days offered him a job in a dress business. He also met his second wife, Betty, through a chess-club connection; they settled in Santa Monica and lived happily ever after. "I cannot complain," he said near the end of what he considered a very rich life. He had little to do with Caltech, but did make at least one official visit—to be inducted into the Alumni Association's Half-Century Club. Weinbaum died September 1 after a short illness at the age of 93.

Letters



In this photo from "The Thomas Hunt Morgan Era in Biology" in the last issue of ***E&S***, the man standing third from left between Robert Millikan and Guglielmo Marconi was misidentified as Allen Balch. He is in fact Harry Chandler, publisher of the ***Los Angeles Times*** and another of Caltech's early benefactors and trustees. The 1933 occasion, besides welcoming the Marconis, was in celebration of Morgan's Nobel Prize. Morgan is second from right.

Editor: I have just read Judith Goodstein's chapter on the history of the Caltech biology division in the last issue of *Engineering & Science*. I have, of course, immediately ordered a copy of the book.

I am writing to make a few comments on the question of anti-Semitism. As I am sure you know, that was a prevailing attitude in the 1920s, and it is by no means extinct now. When I was chairman of physiology here at the University of Michigan, I was criticized for having too many Jews in the department. I gave up science 10 years ago, and I have occupied my time writing history of physiology and medicine. In going through the university archives in the Bentley Historical Library, I have encountered the warning that So-and-so is Jewish so watch out. There is the complimentary remark that although he is Jewish he is all right. Michigan is full of families descended from German immigrants, and I have encountered also the remark that someone who has a German name is not Jewish, as a recommendation.

As for T. H. Morgan, in 1933 I was an undergraduate at Caltech, and I spent one afternoon a week working with Morgan in the laboratory. On my 21st birthday my chief task was to fend off reporters, for that was the day the notice of the Nobel Prize reached Morgan. Morgan's assistant was Albert Tyler, who was also my teacher in a couple of courses. Once when Morgan and I were alone he made an entirely gratuitous, rather snide remark to the effect that you could always tell a Jew by the way he walked. That was stimulated by hearing Albert Tyler approach down the hall.

There is another side to this story. I heard (and I have no recollection, however, where I heard it) that Morgan had been so impressed by Jacques Loeb that he concluded that only a Jew could be a good biochemist. The result was that he looked for the brightest young Jewish biochemist he could find, with the result that he brought Henry Borsook to Caltech. It's a good story, whether or not it is true.

Apropos your story about Morgan's remark at the Royal Society, the story I heard was that Morgan had been provided with a list of good British neurophysiologists among whom he might find a recruit. One on the list was Jack Eccles [later to win a Nobel Prize], who was not asked. Eccles told me when I was his student at Oxford that he would have accepted the job if it had been offered. The Wiersma-Van Harrefeld team really wasn't very good, though Wiersma was by far the better of the two.

All this is without documentation, anathema to a historian.

Horace W. Davenport (BS '35, PhD '39)

The real Mr. and Mrs. Allen Balch, who financed the Athenaeum and part of Kerckhoff Laboratory, are shown above. Is Mrs. Balch also standing next to Millikan in the top photo? We're no longer certain, though it looks like her, and we would appreciate hearing from anyone who knows.

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/* ================================================================ */
INITIALIZE
/* ================================================================ */
GYRO'STAT    = '—'
STOP'GYCHG   = TRUE                          /* !!2226h */
STOP'?DCON   = TRUE

/* ================================================================ */
EVENT LOGIC NODES
/* ================================================================ */

GYR'HTR    = ?HG FF ?HGR;

GYR'HTR'TME = STORE CLOCK IF GYR'HTR;

GYRO'ON   = STORE TRUE IF  GYRO'STAT='ON '
        ELSE FALSE IF GYRO'STAT='OFF';

GYRO'BOTH'ON = GYRO1'ON AND GYRO2'ON;

GYRO1'TIME = STORE CLOCK IF GYRO1'ON;

GYRO2'TIME = STORE CLOCK IF GYRO2'ON;

GYRO1'ON = INS'ELEC1 AND GYRO'1 AND DECODE(TIMER,6) = 'GYRO1'ON'
         FF NOT (INS'ELEC1 AND GYRO'1);

GYRO2'ON = INS'ELEC2 AND GYRO'2 AND DECODE(TIMER,6) = 'GYRO2'ON'
         FF NOT (INS'ELEC2 AND GYRO'2);

GYRO'STAT = STORE 'ON ' IF GYRO1'ON OR GYRO2'ON
         ELSE 'OFF' IF NOT GYRO1'ON AND NOT GYRO2'ON;

GYRO1'DLY = STORE CLOCK + ::40 IF INS'ELEC1
         AND GYRO'1 AND ?IN1P
         DELAY 'TIMER:GYRO1'ON';
GYRO2'DLY = STORE CLOCK + ::40 IF INS'ELEC2
         AND GYRO'2 AND ?IN2P
         DELAY 'TIMER:GYRO2'ON';

GY'CHG = ?GYON OR (?MODE AND ((STATE'AACS = 16 AND NO'PROP P'AACS'ST $;$ 16)
         OR STATE'AACS = 22)) OR (?SUN AND STATE'AACS = 11);

STOP'GYCHG = NOT GY'CHG;

DO'GYCHG = NOT STOP'GYCHG AND NOT GY'CHG;

GY'CHGNODE = STORE DECODE(?GYON,6) IF ?GYON
          ELSE 'ON'  IF GY'CHG AND NO'PROP STATE'AACS = 16
          ELSE 'OFF' IF GY'CHG AND (NO'PROP STATE'AACS = 22
              OR NO'PROP STATE'AACS = 11)
          ELSE ' ' IF NOT ?GYON AND NOT GY'CHG;

GYRO'1 = ((DCON'GY1 OR (GY'CHGNODE = 'ON' AND GY'CHG)) AND (NO'PROP STATE'AACS $;$ 12))
          FF ((((?DCON6 AND H#0008) = H#0000) AND ((?DCON7 AND H#0008) = H#0008)
         AND ?DCON) OR (GY'CHGNODE = 'OFF' AND GY'CHG));

GYRO'2 = (( DCON'GY2 OR (GY'CHGNODE = 'ON' AND GY'CHG))
        AND (NO'PROP STATE'AACS ; 12))
       FF
         ((((?DCON6 AND H#0004) = H#0000) AND ((?DCON7 AND H#0004) = H#0004)
         AND ?DCON) OR (GY'CHGNODE = 'OFF' AND GY'CHG));
```

Do What I Mean

Some processors quickly run ahead of the others. Soon the front runners start getting messages that are the spacecraft equivalent of "turn left three blocks ago."

One spaceflight engineer's worst nightmare came true on August 29, 1988, when he sat down at a console in the Soviet space program's Flight Control Center at Kaliningrad, on the outskirts of Moscow. He typed in a routine command to PHOBOS 1, which had been launched toward Mars that July. He unwittingly omitted one character at the beginning of the command, transforming it into one that turned off the spacecraft's attitude-control system instead. The computer verified that the command was properly encoded, and duly forwarded it. Without attitude control, the spacecraft began to tumble, its life-giving solar panels no longer fixed on the sun. By the time humans discovered the error several hours later, the onboard batteries had run dangerously low. The comatose spacecraft failed to respond to frantic signals from the ground, and expired before it could be reoriented.

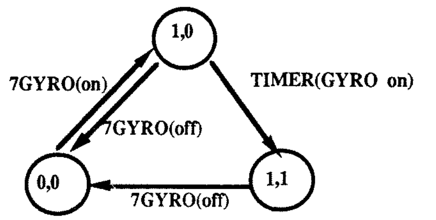
Spacecraft are robots, and they do exactly as they're told. As the fate of PHOBOS 1 shows, not only must the commands—called "sequences"—be instructions that the robot is capable of executing, they must not cause it to do itself harm. A spacecraft's onboard computers are fully occupied as it is, so the task of ensuring that sequences have their intended effects—and no others!—must be done on the ground. To this end, spaceflight engineers draw up a book of "flight rules" that the sequences mustn't violate—the owner's manual, as it were, that codifies how the spacecraft is to be operated.

At Caltech's Jet Propulsion Laboratory, sequences are generated by an elaborate process. The science-team members negotiate among themselves for instrument time—almost everyone wants the cameras simultaneously during close flybys, for example—and the flight team figures out how to run the spacecraft while accommodating as many of the science team's requests as possible. The combined teams draw up a master schedule containing general instructions such as, "Take five ultraviolet pictures of Neptune," or, "Get a navigational fix from the star tracker." This eventually leads to sequences of detailed "machine-language" instructions that actually aim the camera at Neptune, or collect data from the star tracker. And every step of the way, humans and computers check the process against the flight rules.

For the Magellan, Galileo, and part of the Voyager missions, one of the last stops for a sequence is a program called CHECKER, which simulates the whole spacecraft, executes the sequence step by step, and notes when flight rules would be broken. But CHECKER is far from perfect. Says Joan Horvath, assistant sequence team chief for TOPEX/Posei-

Opposite: A portion of the flight rules for Galileo's attitude-control gyros in their "human-readable" format.

Right: A gyro and its flight rules as simulated by Alkalaj and Schneider. The circles correspond to the gyro's possible states: lower left is gyro off; top is gyro on and warming up; lower right is gyro on and warm. (The heater has its own diagram.) The paired numbers indicate the system's overall status. Thus (1,0) means GYRO ON, DATA NOT RELIABLE. "7" designates an attitude-control system.



State: {(X,Y) | X=gyro power on, Y=reliable data) }

```
                                RULES
whenever (gyro_on -> true)    if (time(gyro_on)-time(htr_on) <1 hour) => error
whenever (gyro_on -> false)   if (aacs_mode == inertial) => error
whenever (gyro on ==true)     if (htr on -> true) => error
```

don and former Magellan sequence design engineer, "Parts of CHECKER date back to 1973. It runs on an old UNIVAC computer, and it's hard to use. It's also difficult to add new flight rules to the software, because they're diffused through the program, rather than being spelled out in one spot. But you always wind up adding rules, because unexpected things happen during a mission. Magellan's star tracker turned out to be oversensitive, for instance, so we wound up having to greatly increase the number of calibrations. For each calibration, we had to point the spacecraft at the guide star, tape-record the calibration data, then point the spacecraft back toward Earth and play the data back. This caused lots of extra tape movements, not to mention all the additional work required of the attitude-control systems to turn the spacecraft back and forth. So the way we do it now, a lot of the flight rules are on checklists in binders rather than in the software, and people do as much checking as possible by hand. It's like programming was back in the days of punch cards, when you'd play with your deck until you got it just right, and then hand it in to someone in a white coat who'd run it for you. We need a sequence checker that people could use as they're working, like a calculator or a word processor, instead of trying to get everything perfect before putting it in the computer."

For the last three years, Horvath's group—which has included a series of Summer Undergraduate Research Fellowship (SURF) students, supported by JPL's Director's Discretionary Fund—has been working on an approach to bring sequence checking into the nineties. The group is the first to design sequence-checking software for parallel computers, whose multiple processors run different parts of a program simultaneously, speeding up the computation. The group's latest effort is a program called SAVE, developed this past summer by SURFer Karl Schneider, a senior in engineering and applied science.

SAVE uses a conceptual shorthand, developed by Leon Alkalaj, a member of the technical staff in JPL's Advanced Computer Systems and Technology Section, that represents how the spacecraft's components interact. For example, an attitude-control gyro is always either on or off at any given moment, as is its heater. And the heater must have been turned on at least an hour before the gyro for it to be warm enough to give reliable data to other spacecraft systems. SAVE can be thought of as containing a map of all these links, with a little flag stuck into each component to indicate its state—ON, OFF, DATA RELIABLE, and so on. The flight rules constantly scan the map, watching for patterns of flags that spell trouble. This approach also allows the flight rules to be displayed in a box on the computer's screen, where they can be altered or new rules added.

SAVE parcels out the spacecraft's components to the processors. It then sorts through the sequence, dealing all commands relating to a particular component to the processor running that component. Each processor then steps through its own subset of commands. When a command executed by one component affects another component, a message is automatically sent to the affected component.

But each component's workload varies, and so some processors quickly run ahead of the others. Soon the front runners start getting messages that are the spacecraft equivalent of "turn left three blocks ago." One solution to this dilemma, widely used in other applications, would be to backtrack the recipient processor to where the command should have been executed, and pick up from there. However, this approach—called Time Warp—rapidly gets cumbersome when applied to the sequence-checking problem.

Instead, Alkalaj's and Schneider's program "knows" when a component will receive a message, even if that component didn't ask for information to be sent to it. When the component reaches that moment, the processor stops and waits until the message arrives before proceeding with the next command. This approach was developed by K. Mani Chandy, professor of computer science at Caltech, and J. Misra of the University of Texas at Austin.

At the moment, SAVE is simulating very simple spacecraft on a little five-node computer in Horvath's lab. But the program is written to be compatible with the world's fastest supercomputer, the Touchstone Delta, recently installed on campus. Horvath and Schneider have applied for time on the Delta, where they plan to check real sequences from the spacecraft Galileo's recent encounter with the asteroid Gaspra. If SAVE passes this test, they plan to propose that SAVE be used on other JPL projects, such as the upcoming CRAF/Cassini mission to Saturn. □ —*DS*

Cleared for Takeoff

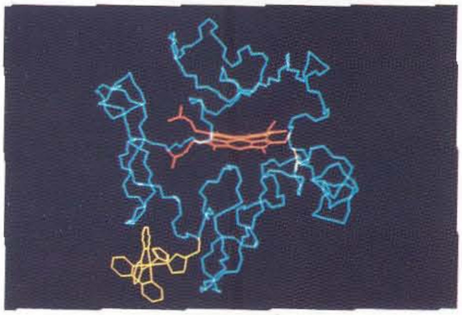
Some members of the collaboration: (back row, from left) Morten Bjerrum, Winkler, Gray, David Beratan, DiBilio, Juri Germanas; (front row) Jorge Colón, Gary Mines, Chang, Debbie Wuttke, Danny Casimiro, and Zhong Huang.

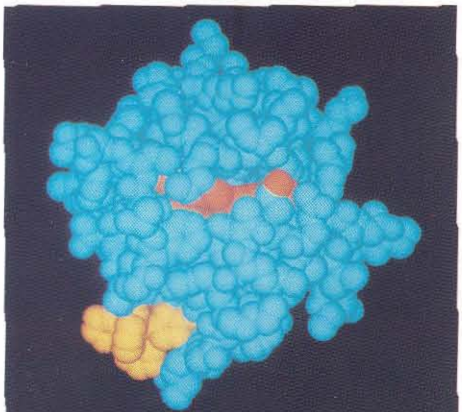
Electrons do more frequent flying than most corporate executives. Electrons commuting from molecule to molecule power life's basic processes. And many important metalloproteins, such as the cytochromes that help power our cells, keep the metabolic economy humming by dispatching electrons from a metal atom at the heart of the molecule to various sites on its periphery. Harry Gray, Beckman Professor of Chemistry and Director of the Beckman Institute, and Jay Winkler, a Member of the Beckman Institute, are studying these intramolecular electron transfers in hopes of discovering how the rate of electron transfer varies with the distance to the destination and with the molecular terrain along the way. Proteins are made of smaller molecules called amino acids, and the physical contours and electrical properties of the particular amino acids on the route traveled by the electron can greatly affect its passage. The researchers' eventual goal is to make another transfer—to apply the rules behind nature's exquisitely engineered metabolic machinery to the design of similar chemical machinery that would turn out made-to-order substances (pharmaceuticals, plastics, or what have you) on an industrial scale.

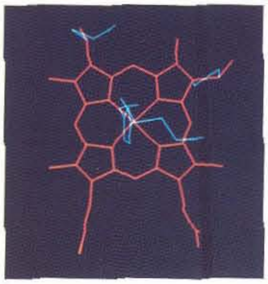
The researchers act as molecular travel agents. First, the electron's reservation is confirmed by replacing the amino acid at the electron's destination with another amino acid—histidine—to which a ruthenium atom can be attached. The ruthenium atom provides a landing site for the electron and undergoes a spectroscopically detectable change upon its arrival, allowing its time of flight to be measured. These new electron-transfer proteins are built to spec in collaboration with members of Professor of Organic Chemistry John Richards's group.

Electrons in living cells take wing in response to processes that are hard to duplicate in the lab, so the researchers use a laser pulse to excite the central metal atom, causing it to emit an electron. But most of the biologically important metals stay excited for mere trillionths of a second—not long enough to emit an electron. Several researchers at other institutions have successfully substituted zinc—whose excited state lasts for several thousandths of a second—for iron, around which hemoglobin, the cytochrome family of proteins, and a slew of other molecules are built, but this technique doesn't work for other metals. An iron-containing protein has its metal atom mounted in an elaborate bit of scaffolding called a heme complex, around which the protein is assembled. When researchers popped the iron atom out of the framework and slipped a zinc atom in, the protein obligingly reassembled itself around the modified heme complex. But atoms of other metals are directly bound to the amino acids that

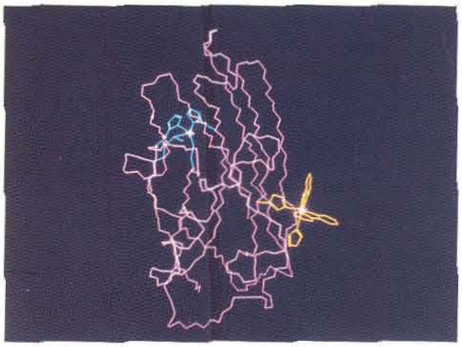
Left: Winkler measuring fluorescence from a sample in the Beckman Institute's laser center. Below: Two views of a cytochrome molecule. The heme complex is red, the amino acids are blue, and the histidine–ruthenium assembly is orange. The upper view shows only the "backbone" of the structure. The lower view shows all the atoms. Note how the heme complex, which lies perpendicular to the page from this perspective, is buried in the middle of the protein.







Above: The heme complex. The iron atom lies at the intersection of the two diagonal lines in its center. The light blue lines show points of attachment to the surrounding protein, including two directly above and directly below the iron atom, relative to the plane of the page. Electrons can also travel to the protein via the two "legs" projecting toward the bottom of the image, which are more loosely connected to the protein. Below: There are only four connections (light blue) to the copper atom (purple cross) in an azurin molecule.



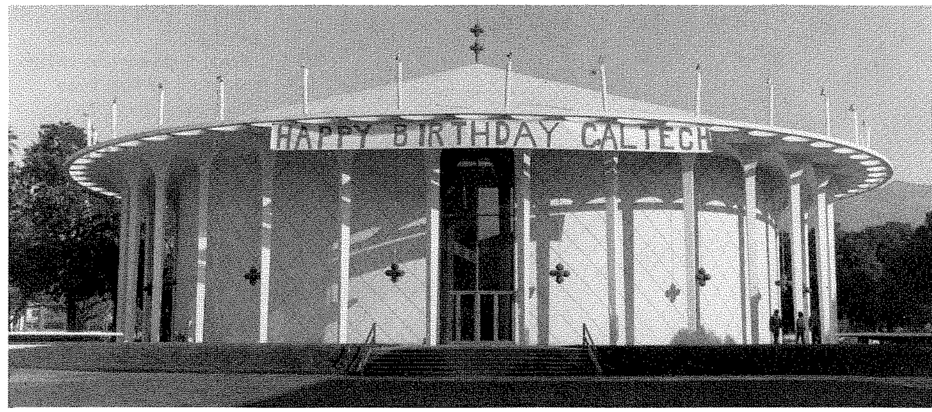
make up the protein. The metal's identity determines the shape of the protein around it to such an extent that replacing the metal with zinc distorts the protein to the point of altering its behavior.

This past year, postdoc I-Jy Chang figured out how to do the experiment with the original metal left in place, by turning the laser on the ruthenium atom instead. When the right substituents are added to it, ruthenium's excited state lasts about 50 billionths of a second, just long enough to clear an electron for takeoff. So instead of prodding the zinc atom, the researchers excite the ruthenium atom, sending electrons from the outskirts in toward the center. And if the experiment demands that the electrons be outbound, the researchers can add a chemical reagent that makes the ruthenium atom electron-deficient, causing it to steal electrons from the central atom when excited.

Now that electrons can be booked onto any itinerary that the researchers want to study, postdoc Angelo DiBilio and grad student Ralf Langen are applying the technique to azurin, an intensely blue, copper-containing protein found in bacteria. Azurin is a particularly nice protein to study, because the amino acids enfolding the copper atom attach to it at only four specific points. Thus an electron has only four possible routes to or from the copper atom. The ruthenium atom's placement determines which path the electron follows, allowing each one of the four to be studied unambiguously. Heme, by contrast, has an elaborate honeycomb structure, and the metal-heme complex resembles a golf ball pushed halfway through a chicken-wire fence. There are many possible journeys an electron could make through this complex, and it's almost impossible to chart with certainty the course the electron actually traverses.

The group has just discovered that an electron's speed depends on its route, and the specific amino acid attached to the copper atom appears to make the difference. Two of the attachment points are histidines, one is a cysteine, the fourth is a methionine. Cysteine is the express route; the methionine route appears to be a puddle-jumper, taking several hundred times longer. The measurements were made with the ruthenium runway sited some distance from the copper center, so the proof isn't ironclad yet. The scientists plan to move the runway closer to the center, but they know from experiments with cytochrome that the electrons start traveling too fast for the current spectroscopic system as the distance between takeoff and landing shrinks. Up to 15 atomic diameters as the electron flies can be covered in less than ten billionths of a second. A new system that will enable the group to follow electron transfers in trillionths of a second is being built. □ *—DS*

Random Walk



Best in the World

Newsweek magazine has named Caltech one of the ten best schools in the world. Its December 2 cover story on education described "pockets of excellence across the globe" in various levels and disciplines, noting that "Americans have the most successful system of higher education, especially postgraduate programs; the California Institute of Technology represents the best of that tradition."

Although Newsweek claims that Japan is tops in science education at the elementary- and secondary-school level, the article credits U.S. success in science and engineering graduate education to the establishment of centers of research in the universities after World War II, with the result of "unparalleled educational opportunities for students fortunate enough to attend those centers—such as the California Institute of Technology." The article goes on to praise Caltech's size, the creativity resulting from its lack of academic boundaries, and its focus on fields in which it can excel.

"Caltech has come to epitomize excellence in higher education," the article concludes. An editorial in the *Los Angeles Times* called *Newsweek*'s accolade "the last birthday present of [Caltech's] centennial year."

LIGO Funded

The National Science Foundation's 1992 budget, as approved by Congress and signed by the president in October, includes first-year funding for the Laser Interferometer Gravitational Wave Observatory (LIGO). A joint project of Caltech and MIT, it will cost an estimated $211 million over the next five years. LIGO will consist of two detectors, built at least 1,500 miles apart; each is L-shaped with arms 2.5 miles long. The two detectors will operate as a single observatory, which will attempt to register and measure gravity waves—ripples in space-time predicted by Einstein's theory of general relativity. LIGO was featured in the cover story of the Summer 1991 *E&S*.

As November 1, 1991, dawned, Beckman Auditorium was revealed as a giant birthday cake, a gift from some anonymous students. Later in the day, President Everhart used a sword to cut the real centennial birthday cake during a party for members of the Caltech community, who showed up in thousands for the occasion. This marked the finale of Caltech's celebrations of it's 100th year.

Honors and Awards

John Hopfield, the Roscoe G. Dickinson Professor of Chemistry and Biology, was chosen the 1991 California Scientist of the Year by the California Museum of Science and Industry.

Hans Hornung, the Johnson Professor of Aeronautics and director of GALCIT, has been elected a foreign member of the Royal Swedish Academy of Engineering Sciences.

Edward Lewis, the Thomas Hunt Morgan Professor of Biology, Emeritus, has won the 1991 Albert Lasker Basic Research Award, for his pioneering genetic work with *Drosophila.*

Edward Stone was among 20 U.S. scientists to receive the National Medal of Science in September. Stone is professor of physics and director of the Jet Propulsion Laboratory, as well as Caltech vice president.

Yu-Chong Tai, assistant professor of electrical engineering, was named one of 20 outstanding young researchers to receive a David and Lucile Packard Foundation Fellowhip in Science and Engineering. The fellowship carries a research stipend of $500,000 over five years.

Kip Thorne has been named the Richard P. Feynman Professor of Theoretical Physics. The chair was established by a $1.5 million gift from Michael Scott, BS '65.