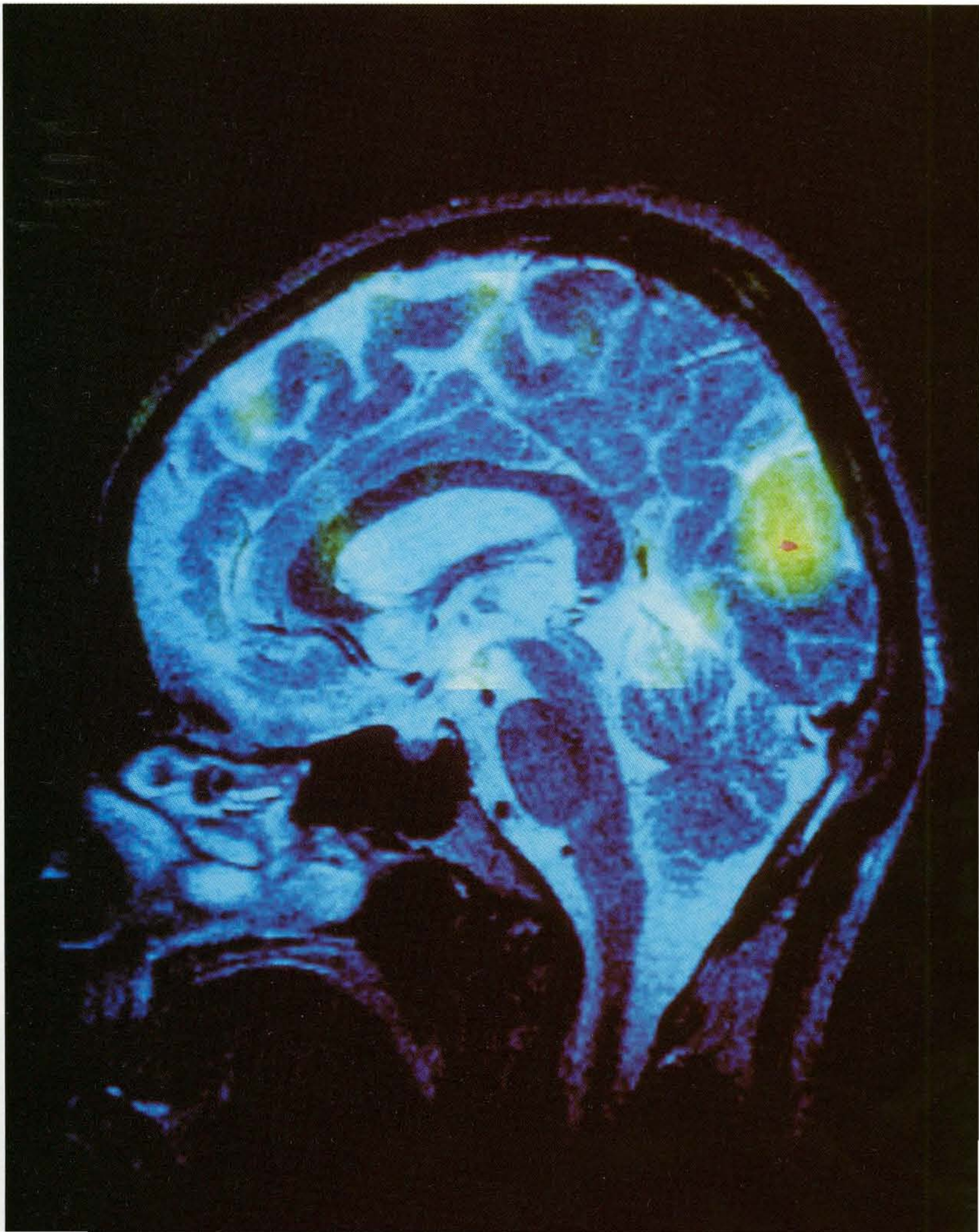
California Institute of Technology

Engineering & Science

Spring 1993



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Russia's Constitution

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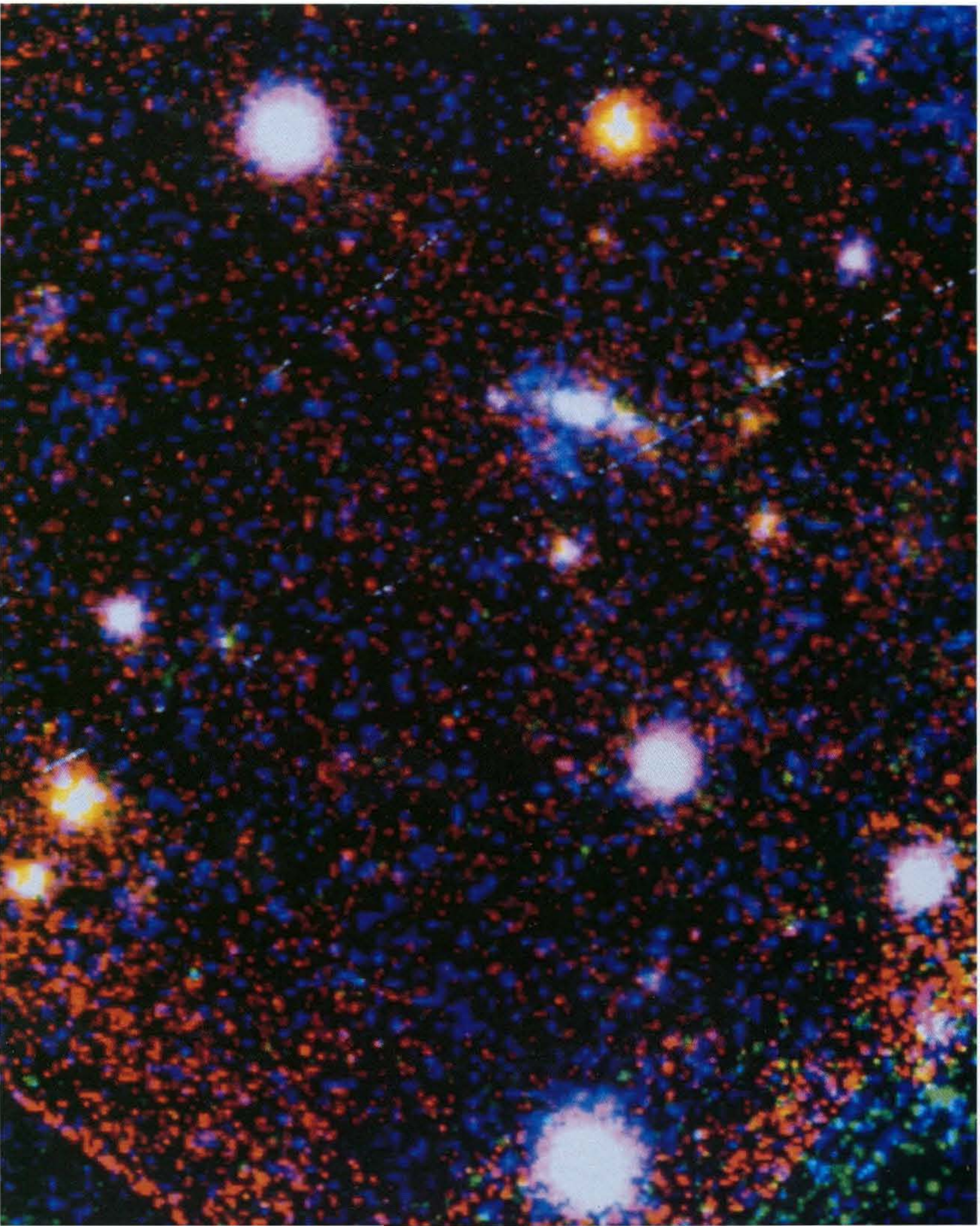

The most distant galaxy yet discovered, euphoniously christened 4C41.17. This image was created by combining two infrared pictures taken by the Near-Infrared Camera on the ten-meter W. M. Keck Telescope, and a third picture taken in visible light by the four-meter Kitt Peak telescope. The image shows the galaxy (the flat, white object in the upper center) and its surrounding field of stars as we would see them if our eyes were sensitive to infrared as well as visible light. The blue wisp just below the galaxy may be a region of hot, newly formed stars. The three red objects to the galaxy's right, and the red object below it, are faint companions visible only at infrared wavelengths. Caltech and the University of California jointly operate the Keck Telescope, the world's largest optical/infrared telescope.

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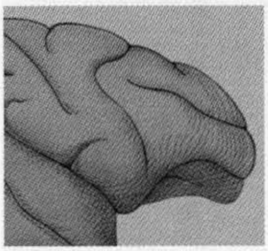

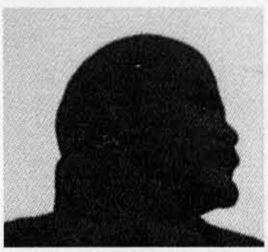

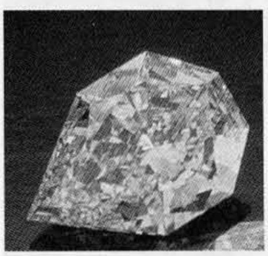

On the cover: A vertical PET/MRI image of the brain of Caltech's John Allman, Hixon Professor of Psychobiology and professor of biology. PET (Positron Emission Tomography) shows active brain areas, which absorb positron-emitting oxygen-15 from the blood, in yellow and red; MRI (Magnetic Resonance Imaging) makes detailed anatomical maps. Superimposing PET and MRI shows which brain structures are active in a particular task. Allman was looking at a flashing stimulus, activating visual areas at the back of the brain.

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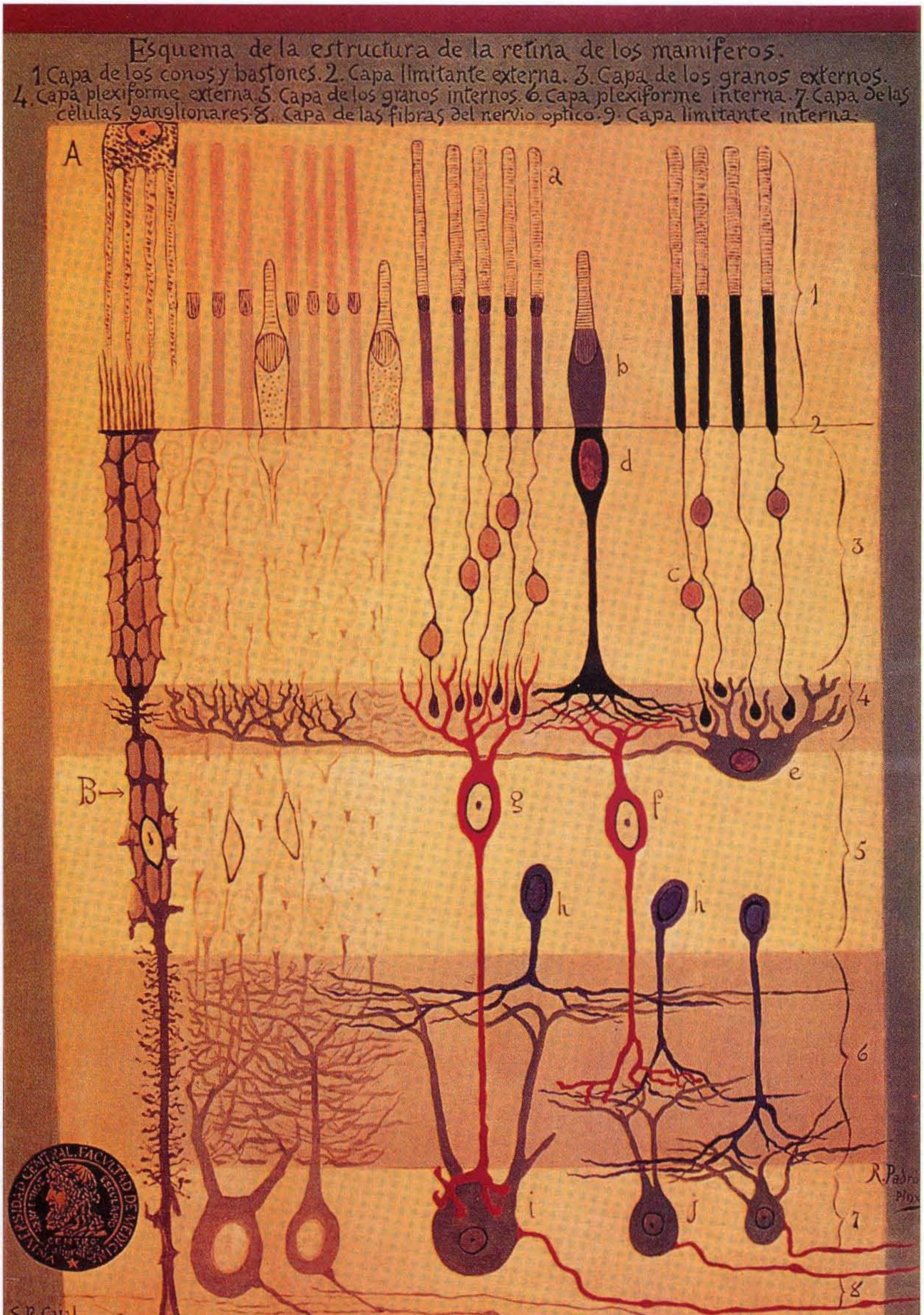
Esquema de la estructura de la retina de los mamiferos.
1. Capa de los conos y bastones. 2. Capa limitante externa. 3. Capa de los granos externos. 4. Capa plexiforme externa. 5. Capa de los granos internos. 6. Capa plexiforme interna. 7. Capa de las células ganglionares. 8. Capa de las fibras del nervio optico. 9. Capa limitante interna.
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A. Celulas pigmentarias. B. Celulas epiteliales
a. bastones. b. conos. c. núcleo de los bastones. d. núcleo de los conos. e. célula horizontal grande. f. bipolar relacionada con los conos. g. bipolar relacionada con los bastones. h. células amacrinas. i. célula ganglionar gigante. j. células ganglionares pequeñas.

When Looking Is Not Seeing: Towards a Neurobiological View of Awareness

I'll try to explain how we can begin to attack the problem of consciousness in a reductionist, scientific manner.

by Christof Koch

Where seeing begins. This cross-sectional drawing of nerve cells in the retina was made by Santiago Ramón y Cajal, who shared the Nobel Prize for medicine with Camillo Golgi in 1906 for their studies of the nervous system. From the light-detecting rod and cone cells at the top to the optic-nerve fibers at the bottom, the retina is about 0.01 inch (0.25 mm) thick.

Three fundamental problems intrigue scientists today. The first is the physicist's dream, namely, to unify all the known forces in the universe into one single theory, and there's a candidate being worked on here at Caltech called superstring theory. This theory—whatever it proves to be—together with the initial conditions prevailing at the birth of the universe 15 billion years ago, would explain why the universe is in the bad shape it's in now. The second problem is the biologist's dream—to explain how a single cell, over five weeks, five months, or five years, becomes a plant, a bug, or a person—the problem of development. There are lots of people at Caltech working on that problem, too. The third has been the domain of philosophers and psychologists until now. It's the problem of the brain—how do we perceive? Not only we humans, but monkeys, cats, and even such lowly creatures as the fly and the sea slug—animals we squish underfoot sometimes. How do we perceive, and react to, our surroundings in a way that makes sense?

Solving this problem is really preliminary to solving the problem that drew many of us into neurobiology in the first place, but which we can't talk about. It's the evil C-word, where C stands for consciousness. Over the last 60 years, particularly in this country, there has been a very strong movement by the behaviorists—B. F. Skinner and friends—to outlaw consciousness. They say that consciousness is not really a scientific concept. You can't test it, so you should just leave it out of your experiments altogether. But we all know that we *are* conscious, so I'll try to explain how we can begin to attack the problem of consciousness in a reductionist, scientific manner.

There are some house rules to this game, in order to not get stuck early on. The first one is: don't attempt any formal definition of "consciousness." We roughly know what we're talking about, and for any definition you give describing a "conscious" being, I can give a counterexample involving sleepwalking, or REM sleep, or anesthesia, or zombies, or something. So, without defining it more precisely, "consciousness" is the state that I hope you're in now. You aren't asleep yet—that may come as you read—and that's the state I mean.

Rule two is that we are going to assume that higher animals—particularly monkeys, but probably also cats and dogs—have some form of consciousness. If you look at the brain structure of our closest cousins, the great apes, it is very similar to ours. Our brain is bigger, but the complexity is comparable. There's no reason to assume that they don't share some degree of the consciousness that we have. It's a corollary of this rule that a language system is not required for consciousness. From there, it becomes a question of which animals are not conscious, and that again is best left for when we know much more about consciousness. The sea slug, I would say, is probably not conscious, but where consciousness begins is diffuse.

And I'll disappoint you with rule three. I'll not deal with the most interesting aspects of consciousness—such things as free will and qualia. Qualia are subjective properties such as

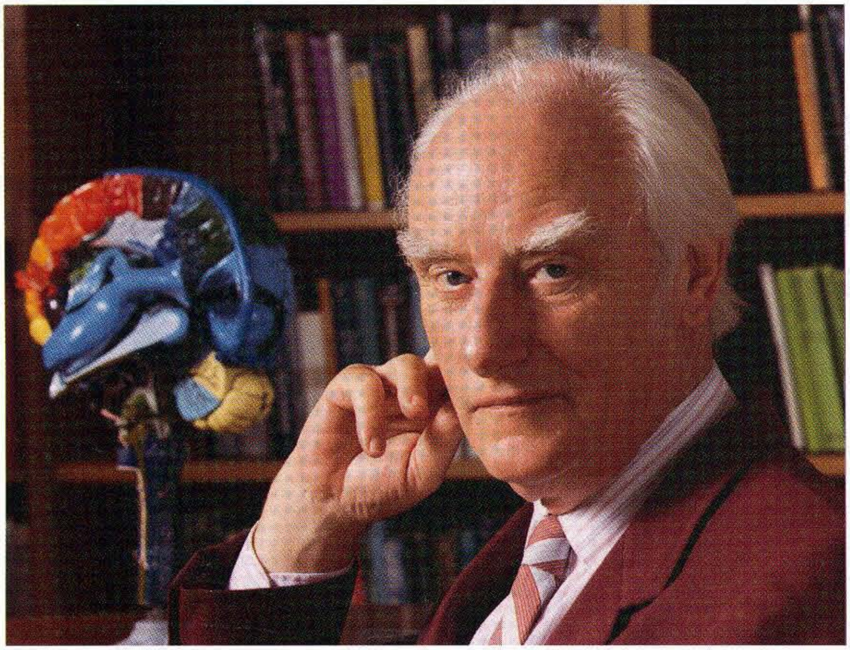
If I'm lost in some problem, I can drive home and realize suddenly that I'm in my garage, but I don't have any awareness of how I arrived there. Yet I had to stop at red lights, make left turns only when there was no oncoming traffic, and, in general, act in an intelligent manner.

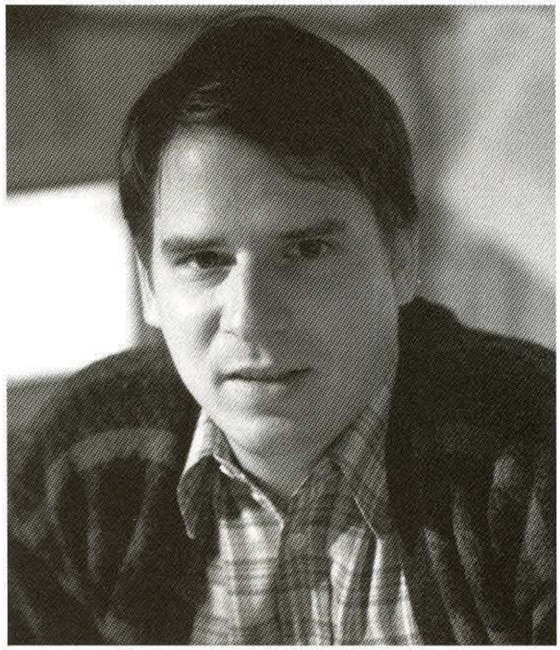
blueness or pain, considered separately from the objects that manifest them. If I have a really bad toothache—I've already taken two aspirin and I'm lying in bed and the tooth starts pounding away—I might start thinking, "Why is this actually *bad?* Why is this awful feeling associated with pain?" The pain comes from a nerve cell—a neuron—in my tooth sending a message to my brain, producing electrical activity in some brain cell. But why should that activity be bad? If a neuron two inches away is activated, it causes me pleasure. And if a neuron two inches in the other direction is activated, I smell a rose. How can the electrical activity of any cell, how can any physical cause, give rise to these feelings of blueness, of awfulness, of pleasure, of smelling a rose? This is a very subjective question, of course, because the feeling *I* have of pain might be quite distinct from the feeling *you* have of pain. All I know is if I hit you on the foot with a big hammer, you are going to cry out. I can infer from your reaction that you probably have the same awful feeling that I would have under the same circumstance, but I really can't test it. And so these problems are best left out. They can only be addressed within philosophy, and may never have any scientifically testable explanation.

And no amount of psychoanalysis, of lying on the couch and paying 100 bucks an hour, will ever tell me how I see color. Maybe I can uncover why I married my wife, but low-level things—how I see color, how I hear or smell—are not amenable to any amount of introspection. In fact, we probably don't have any conscious access to most of our brain. Psychological theories about consciousness or other mental phenomena sometimes have very good elements, but they work from the outside. The only way to find out how the human brain really works is to open up the black box—in this case the brain—and do experiments. You put in electrodes; you do biochemistry; you apply the entire gamut of scientific procedures.

Having said that, I will now discuss the unconscious, a notion first proposed by Nietzsche, and popularized by Freud, Jung, Adler, and others. Over the last 20 years, cognitive psychology has made great progress in understanding a variety of aspects of the unconscious mind, especially the two called "automatic processes" and "knowledge without awareness." All of us do both of these things all the time. Driving a car is a good example of an automatic process. The first time you did it, it took all your concentration. You had to consciously pay attention to everything—staying in a lane, looking in the mirror, and shifting gears if you had a manual transmission. But now, a few years down the road, you drive completely automatically—you can even be thinking of something else. If I'm lost in some problem, I can drive home and realize suddenly that I'm in my garage, but I don't have any awareness of how I arrived there. Yet I had to stop at red lights, make left turns only when there was no oncoming traffic, and, in general, act in an intelligent manner. It happens to me all the time. Another example of an automatic process is mirror writing, like Leonardo da Vinci did in his notebooks. Most people can be trained to read and write mirror writing. It's difficult, and takes quite a while, particularly writing it. But if you *do* it, if I pay you as an experimental subject, you can acquire it in a couple of months. And then you do it effortlessly—it's just like reading normal writing, which, incidentally, is also an automatic process. The other aspect, knowledge without awareness, is knowledge that's available to the brain, but not the mind—you know something, but you're not aware that you know it. One example is subliminal advertising, which was very controversial in the 1960s. The effect is not nearly as strong as most people believe, but it exists. I can flash the words *Buy My Book* on a screen so fast that you're unable to recognize them, yet something in your brain will know. But it won't make you go out and buy my book, unfortunately. A lot of the social judgments that govern our day-to-day interactions—why you like or dislike someone, why you look up to some people and down on others—have been extensively studied. They, too, bypass awareness—you like someone "instinctively," and you can't explain why.

How do we test this morass of feelings to which we have no access? Knowledge without awareness has been studied most rigorously in a class of patients who have prosopagnosia—they're unable to recognize faces. They've had a stroke, or a virus, and some part of their brain is gone. (The study of brain-injury patients has been very fruitful for neuroscience, because you can see which part of the brain has been damaged, and you can find out what mental ability has been affected, and then you can infer—if you are careful—what the missing part of the brain does.) The title character of Oliver Sacks's *The Man Who Mistook His Wife for a Hat* was a prosopagnostic. If you show him a picture of his wife of 25 years, he says he doesn't know who she is. But if you measure the skin conductance of the palm of his hand while you ask him the same question—essentially the principle on which lie detectors are based—you'll see a big change. Something in his brain has recognized her, even though he's not





Crick (left) and Koch (right).

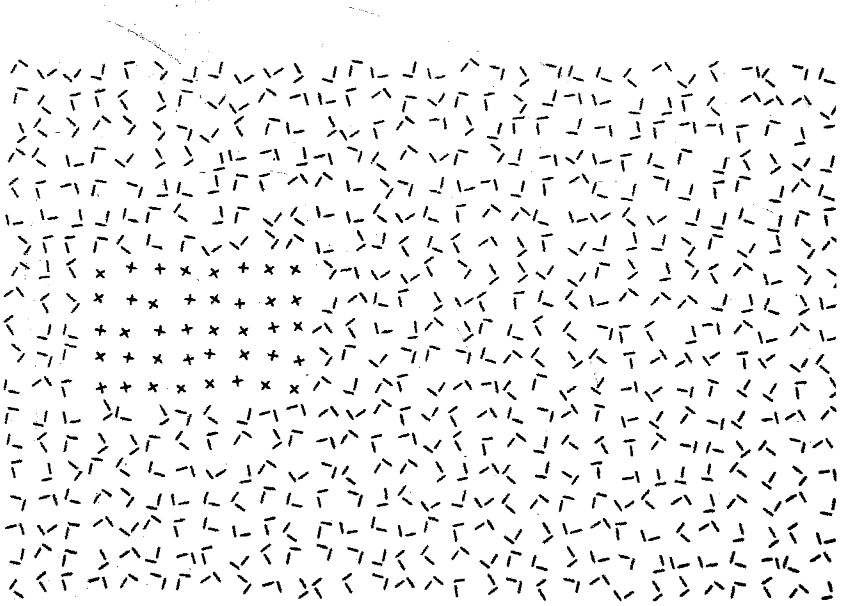
I can flash the words Buy My Book *on a screen so fast that you're unable to recognize them, yet something in your brain will know. But it won't make you go out and buy my book, unfortunately.*

aware of who she is. If you show him a random face he's never seen, he also says, "I haven't seen this before," and this time the skin conductance doesn't show any significant change. You can show him pictures of famous presidents, or movie actors, and very often you will see that, although he claims he doesn't know any of them, there's a big change in skin conductance.

There's another famous case of knowledge without awareness, from which my title, *When Looking Is Not Seeing*, comes. There's a group of patients, first discovered in England, who have what's called blindsight. They are typically older males who have had a stroke in the back of the cerebral cortex, where the visual part of the brain is, so they're unable to see anything in, say, the left part of the visual field. (They don't see anything to the left of what their eyes are focused on.) And so the doctor holds up a finger in the patient's left field of view, and asks, "Do you see anything?" And the patient says, "No, I don't. I'm blind there." "Well, do you see my finger?" "No." "Can you see it moving?" "No! Why do you ask these questions? I'm *blind!*" "Just tell me, does my finger move to the left or to the right?" Eventually the patient says, "OK, I'm just going to guess. It's moving to the left." And he's correct every time. Although all these patients adamantly insist that they don't see anything, they can correctly "guess" the direction of motion. You can move a bright light around, and they will automatically track it with their eyes, although they claim they have no knowledge of it. You can ask them to point at things, and they'll point approximately at the object, although they don't have the same visual acuity that we have. They can identify colors. They cannot do everything—for example, they can't tell shapes. If you hold up a square and ask them if it's a square or a circle, they truly seem to be guessing. This class of patient very vividly demonstrates that people can "know" something without being *aware* of this knowledge.

So what does it mean to be aware? Why am I aware of certain things and not others? How can my brain have information that I'm not aware of? Over the last three or four years, Francis Crick of the Salk Institute in La Jolla and I have outlined a framework that we think will ultimately explain this problem reductively, at the neuronal level. You probably all know of Crick, who won the Nobel Prize for his work with Jim Watson on the double helix. As for my own background, I'm a theoretical neuroscientist, not an experimental biologist. My first, and only, contact with experimental animals was when I was a programmer at the Max Planck Institute for Biological Cybernetics in Tübingen, Germany, where I later got my PhD. It was perhaps three o'clock in the morning and I was hacking some code, when a fly buzzed by with a little numbered sign glued to its back like a shark's dorsal fin. The fly had escaped from a lab where they worked on its visual system. The experience shocked me, and I've remained with computers ever since. To explain our theory, I'll proceed on three descending levels—psychologically first, on the level of the whole brain; then down to brain areas; and finally to single nerve cells.

Crick and I postulate that awareness, at the



This texton pattern contains a background of L-shaped textons, in which two regions of dissimilar texture are imbedded. The region composed of +-shaped textons leaps out at you—can you find the other one?

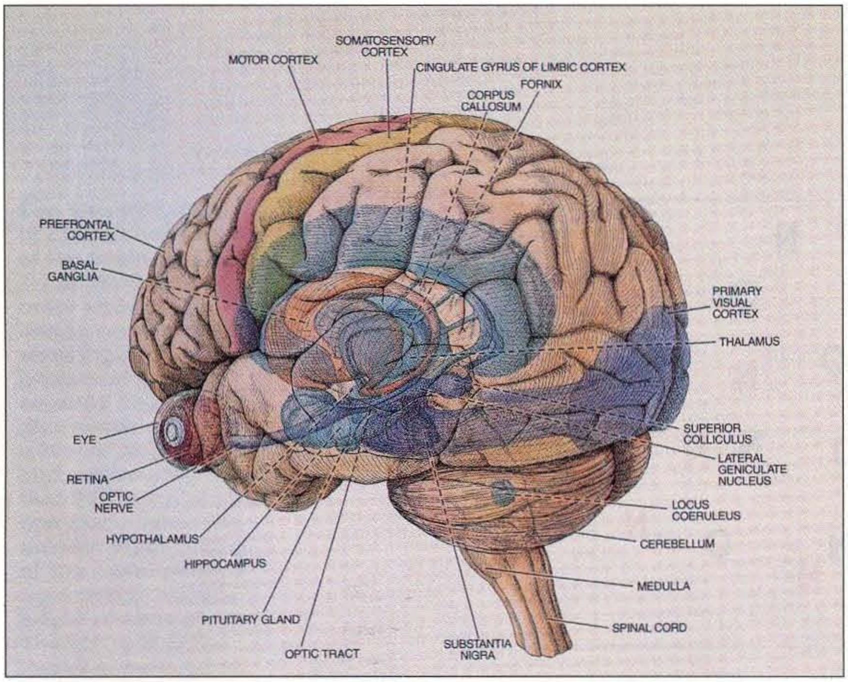
You have to scan the image, object by object, using your mental searchlight.

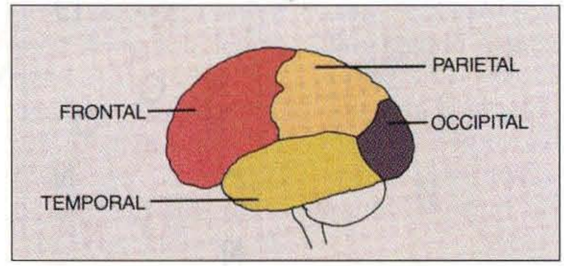
psychological level, involves two things: attention and short-term memory. These processes have been linked to consciousness for quite a while—William James described the phenomenon of attention and its relation to awareness 100 years ago. We believe that whenever you are consciously aware of something, this really means that your unconscious brain has focused your attention on that thing and put it into your short-term memory, where your conscious brain has access to it.

Attention operates in all sensory modalities, but we know it best, by far, in vision. You can think of it as a searchlight. If everything is dark, you can see only what the searchlight is shining on. Only when it illuminates the something is further analysis possible—who do you see? what are they doing? and so on. We believe something similar operates in the visual system, independent of eye movement. You can fix your eyes on one location, yet focus your attention on another; and you can shift the searchlight around. I'll try to demonstrate that with a test devised a decade ago by psychologist Ann Treisman at Berkeley. On the upper right corner of page 9, there's a figure containing the letters N and O. When you finish reading this paragraph, focus your eyes on the small drawing of the brain in the upper right-hand corner of page 7, close your eyes, turn the page, and open your eyes for just an instant—the blink of an eye, just as fast as you can make it. Then answer the question: Was there a green object? There will be many red objects, and there will either be no green objects or one green object. (The reason to glance so quickly is to avoid eye movement. It takes about a fifth of a second to initiate eye movement, so if your glance is quicker, you don't have a chance to move your eyes.) Go ahead and try it right now.

There were roughly two dozen red objects, called distracters, and there was one green target object. It turns out that the time it takes you to find this target is independent of the number of distracting objects. Whether there are 100 red objects and one green, or two red and one green, you'll still pick the green one out instantly, anywhere in your visual field. That is an example of what we call parallel processing. You don't use the searchlight of attention to do it. Even if I hadn't told you to look for a green object, you would still have seen it instantly.

Now, I'll show you something for which you need the searchlight. Using the same procedure as before, I want you to focus on the figure above on this page, turn to page 8, and look for red Os. Try it now. The length of time it takes to do this more difficult task depends on the number of distracters, so our assumption is that you have to scan the image, object by object, using your mental searchlight. So doubling the number of distracters roughly doubles the time it takes you to find the target. Bela Julesz, a visiting professor of biology at Caltech, has found that you need 30 to 50 milliseconds—roughly one-twentieth of a second—to inspect each item with your searchlight (compared to the fifth of a second it takes to move your eyes) and tell whether it's a red O. If it's not, you move your searchlight to the next target. Psychologists believe that you need the attention searchlight for this task because you need to look





Above: A mind's-eye view. The human brain consists of left and right hemispheres (the left one is shown here) divided by a deep fissure and connected by the corpus callosum. At the brain's base lies the cerebellum, which coordinates movement, and the medulla, which controls "autonomic" fucntions such as digestion, breathing, and heartbeat. The limbic system, deep within the brain, is the seat of emotion and long-term memory. Most activity related to thought and perception occurs in the brain's convoluted surface, or cortex. The primary visual cortex, where most visual processing occurs, is at the back of the brain. Right: The cortex's crumpled surface is divided into four lobes by particularly deep folds. Again, the left hemisphere is shown, in the same orientation as the large drawing.

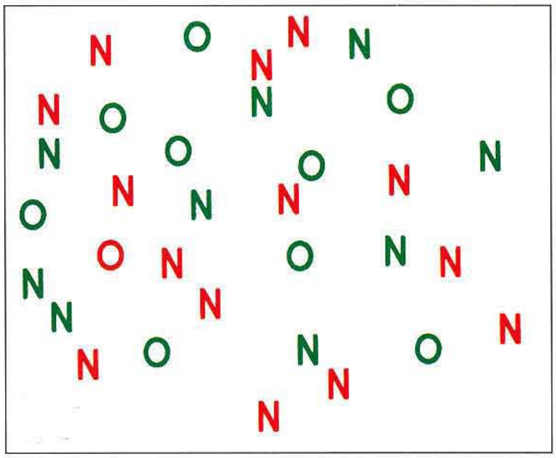
for the simultaneous occurrence of two features—the color red and the letter O.

This searchlight has nothing to do with the scanning movements your eyes make when you look at something. In the 1960s, a Russian, A. L. Yarbus, showed how people's eyes scan an object. He put a little suction cup with a mirror mounted on it on a volunteer's eyeball. The mirror reflected a beam of light onto a photographic plate, making a record of how the eye moved. He discovered that when you look at something, for instance a photograph of a face, your eyes are in constant motion. You might glance at the person's right eye first, then the left, then your gaze might move to the right ear, sweep around the edge of the face and back to the right eye again, then on to the nose, and so on. Under normal circumstances, you usually move your eyes to the same location that you move your attention, but you don't have to.

On page 6 is a demonstration that doesn't require speed. It's a texton pattern devised by Julesz. A texton is a unit of texture—a photon of texture, if you will—and in this case each texton is made up of two line segments that form either an L, a T, or a +. You'll immediately see one pattern—a region made up entirely of +'s embedded in a sea of L's—without having to scan the image. Do you see a second pattern as well? Seeing the second pattern—a region made up entirely of T's, which are texturally very similar to the L's—requires focused attention. You can't see it at a glance. So in this case you can see parallel processing and the spotlight of attention demonstrated in the same figure.

We believe something similar to the spotlight operates whenever you concentrate on one sense. You can listen to a tune, or you can close your eyes and attend to where your finger touches your leg. I'll only talk about vision from now on—that's what I know best—but the spotlight metaphor holds for the other senses as well.

Crick and I postulate that the second component of awareness is short-term, or immediate, memory. Everyone is familiar with long-term memory, which is divided into different sorts. Autobiographical memory is the one most important to us—I know where I was yesterday, or a year ago. Semantic memory is remembering facts, like what the capital of England is. These forms of long-term memory are conscious. There are also unconscious forms, such as procedural memory—skills, such as playing golf, or doing mirror writing, that you learn by practice over time. You usually don't have conscious access to procedural memory, which is why learning such skills is so difficult. The short-term memory underlying awareness is something else. If I give you a telephone number, say 359-6811, you'll remember it for a couple of seconds until something else distracts you. Or, if you need to remember it until you get home, you say, "359-6811, 359-6811, 359-6811, 359-6811." You can keep on rehearsing it indefinitely, but if you don't, it disappears. Short-term memory stores high-level information. If you're a chess player, I can show you a game in progress very briefly, and you can tell me the pieces' positions. But this is true only up to a point, because in general, this memory only holds seven things, plus or minus



Since the searchlight can inspect any one item on the tray in 30-50 milliseconds, and conscious perception takes hundreds of milliseconds, we have the subjective impression that we can be aware of all the items on the tray at once. Placing a new item on the tray causes an older item to get shoved off it.

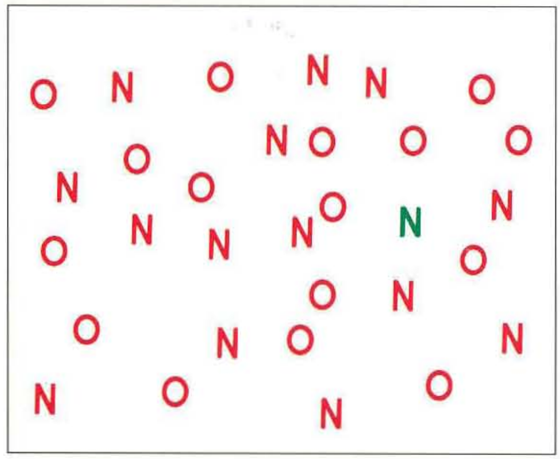
two—seven digits, seven names, seven chess positions. You could think of this memory as a serving tray with a limited capacity. The searchlight plays over the items on the tray one at a time, and as an item is illuminated, we become conscious of it. Since the searchlight can inspect any one item on the tray in 30-50 milliseconds, and conscious perception takes hundreds of milliseconds, we have the subjective impression that we can be aware of all the items on the tray at once. Placing a new item on the tray causes an older item to get shoved off it.

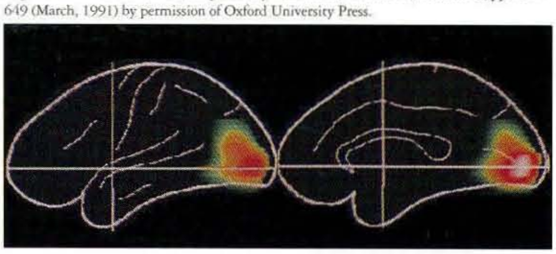
Short-term memory is very robust, and hard to damage. But there are a number of drugs used routinely during surgery that take out long-term memory. These drugs include the family of benzodiazepines—of which Valium is the best-known member—and scopolamine. But there's no drug we know of that blocks short-term memory. There are emergency-room patients who have been in serious accidents and are too badly injured to receive heavy anesthesia, so they receive benzodiazepines that relax them and induce a profound antegrade amnesia. This means that the patient now has a moving time-window of roughly two to three minutes: they forget everything that happened more than a few minutes ago, including the pain they feel during the surgery. Yet they can respond meaningfully to the requests of emergency-room personnel and can sometimes even talk, so they are conscious in the normal sense of the word, but when the drug has worn off 45 minutes later, they don't remember a thing. And there are patients who've lost their autobiographical and semantic memory systems (these two together are known as the declarative memory system) due to cancer, or surgery, or Alzheimer's disease, or an epileptic seizure. There's a patient called H. M., both of whose temporal lobes were surgically removed in the 1950s as a treatment for profound epileptic seizures. His last explicit memories are of events that happened before his operation, well over 30 years ago. He's been in the hospital ever since, and he still doesn't consciously remember his nurses and doctors. But he's perfectly aware and lucid. So long-term memory enriches our lives incredibly, but you don't need it to be aware. All that's necessary for base-level awareness is short-term memory and attention.

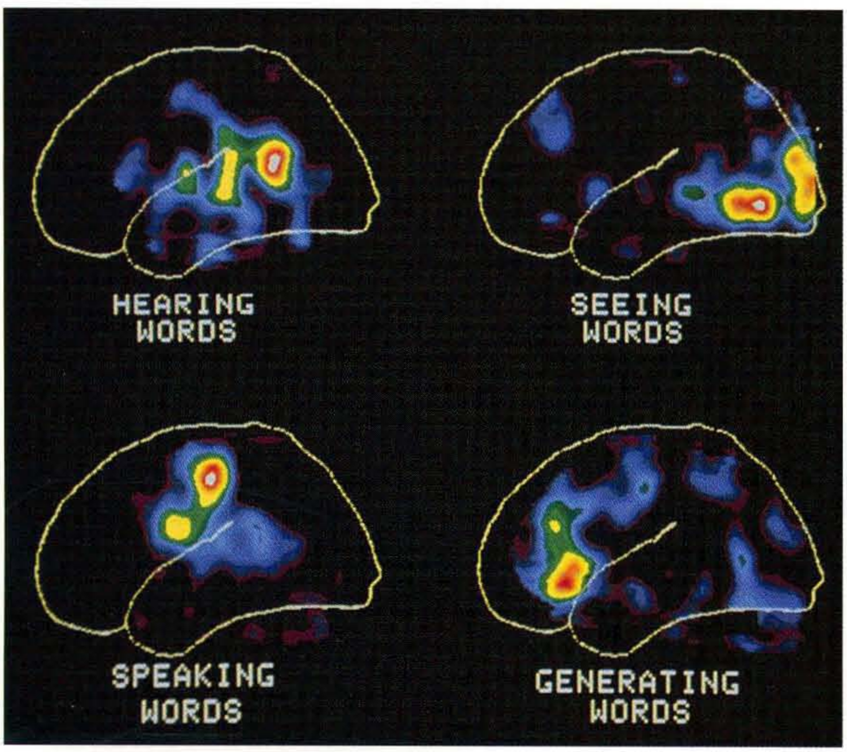
Now I'm going to show you what it means to be aware at the level of brain areas. The brain is made up of many dozens of subparts, called cortical areas, that range in area from the size of your thumbprint to a credit card. Each cortical area has a different function—for seeing color, for seeing depth, for hearing, for talking, for storing people's names, and so on. On the magazine's cover you saw a combined PET/MRI image of the brain of Caltech's John Allman, the Hixon Professor of Psychobiology and professor of biology. When you superimpose a PET image on an MRI image, you can see which brain structures are active in a particular task. In this case, John was looking at a flashing visual stimulus. The first area activated, upon arrival of visual information from the retina, is located at the back of the brain in the occipital lobe—an area called V1, for "visual area one." V1 does "early filtering"—it does the first stages of the processing needed to detect

Top left: PET image of the left hemisphere of the brain, showing areas involved in color vision. The image was made by showing a subject a pattern of colored squares and rectangles reminiscent of a Mondrian painting, and subtracting from that PET scan another one made when the subject was looking at the same pattern rendered in equally bright shades of gray. The left half of the image shows areas activated on the cortical surface, while the right half shows the interior areas.

Below: PET images showing brain areas active during various verbal skills.



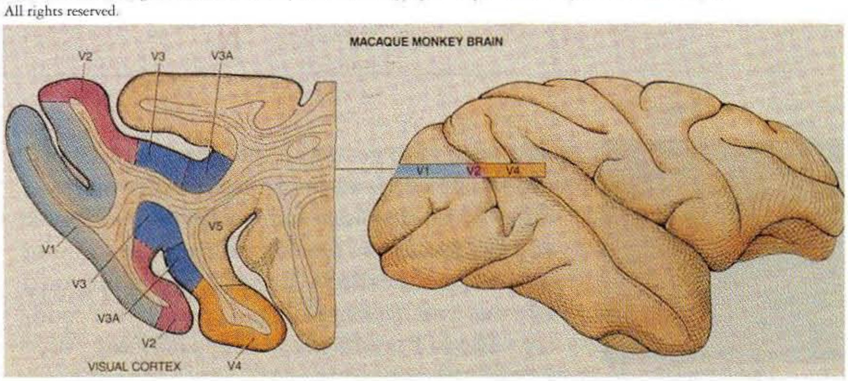


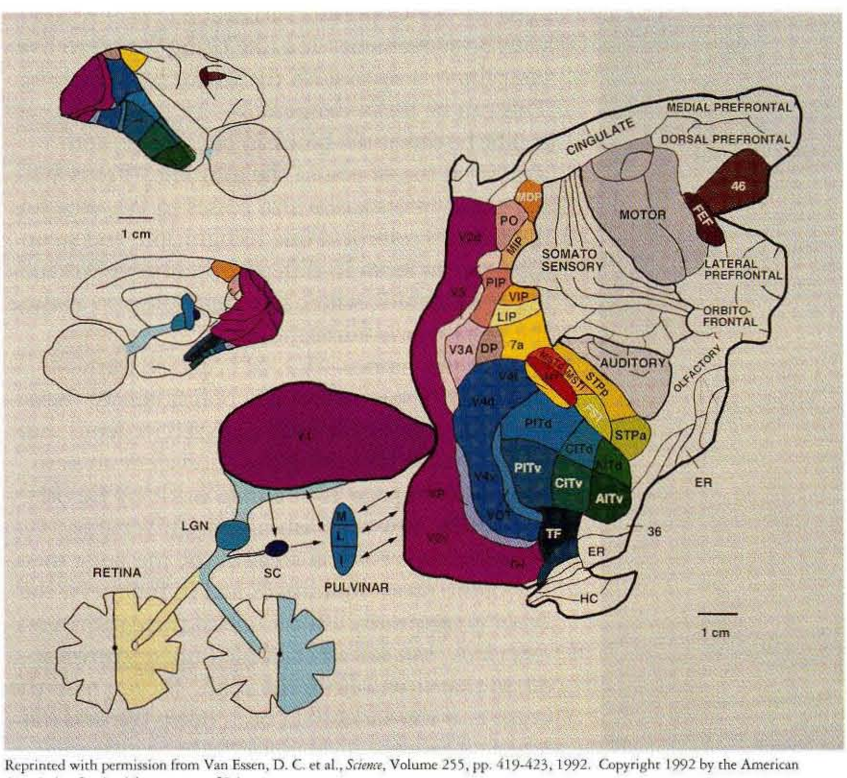


motion, and to get depth information by comparing the stereo views we get from our two eyes. From there, the visual information is distributed to other locations. One of the areas the stimulus goes to next is called V5 or MT, depending on whether you're from England or from this country. The MT area is involved in analyzing motion. Patients whose MT area has been damaged see the world as a succession of still images, with no movement. It's like being forced to live your life in a disco with the strobe light flashing. This can be really dangerous—for example, a car might be down the block in one image, and almost on top of you in the next one. From V1, the visual information also passes to V4, which is involved in color and hue recognition, and so on. There are at least 30 different brain areas, including V4 and MT, whose sole function is to analyze the visual world surrounding us.

All these names—V1, MT, and so forth—really only apply to monkey brains, where these areas' functions have been analyzed in detail, but we believe we are seeing the equivalent areas in humans. On page 10 is a map made by David Van Essen, now at Washington University, showing these cortical areas in the macaque monkey brain. Both in humans and in monkeys, the brain is essentially a sheet one to three millimeters thick, but it's all crumpled up, or convoluted, so that it will fit in the skull. So you map the brain as if the cortex had been taken out and flattened. The typical macaque brain has the surface area of one of those enormous cookies they sell in malls—160 square centimeters. Each of the two hemispheres of our brain corresponds in extent to

How a macaque sees the world. All diagrams are of the brain's right hemisphere.
Top: A cross section through the visual cortex (left) made at the level shown (right). The visual areas are shaded. Note how the cortex's deep folds mean that the brain is practically all surface. Neurons lie in the surface layers; the interior consists of connective and supportive tissues. The eyes are to the right.
Bottom: Map of the unfolded cortex, optic nerve, and both retinas. The insets show how the map relates to the hemisphere's exterior.

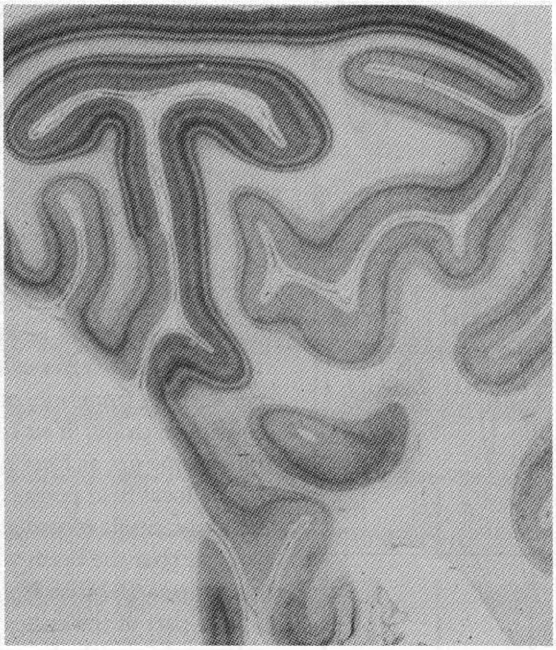


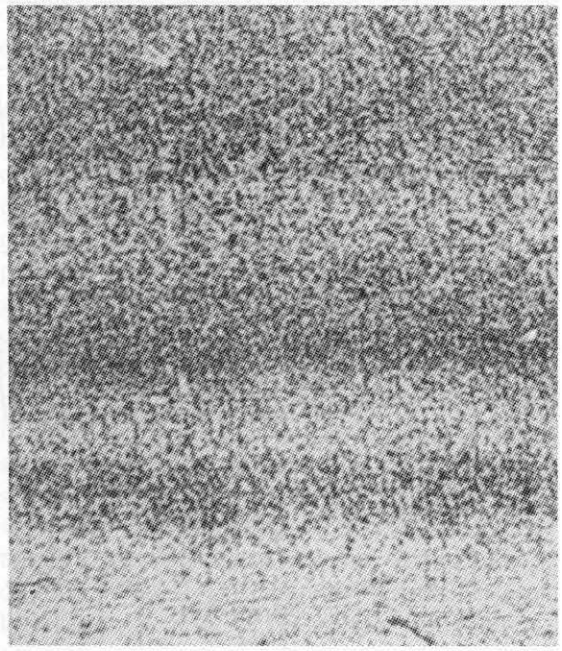


a large pizza an eighth of an inch thick and 14 inches in diameter—something like 1,000 square centimeters. And each of the cortical areas on this map contains a few tens of millions to a few billion neurons.

Let's go down one more level, and look at individual nerve cells. There are roughly a quarter of a million nerve cells and two billion synapses below one square millimeter of cerebral-cortex surface. (Synapses are the connections between neurons.) This is much more densely packed than anything we can do in silicon chips today. Seventy-five percent of those cells are pyramidal cells, so called because their cell bodies supposedly look like pyramids. Each pyramidal cell has lots of input wires, called dendrites, that branch out and extend all through the cortical layer. The cell also has one output wire, called the axon, which makes synapses with the dendrites of several thousand other cells. Neurobiologists believe that memories are encoded in the synapses, and two billion synapses per square millimeter of cortex can hold a lot of memories.

If you insert an electrode into an anesthetized animal's—or human's—brain, you can record the electrical activity of nearby nerve cells. Each nerve cell is turned on only by a particular set of stimuli—objects in the environment that the cell likes to respond to. Visual-cortex neurons like visual things. In V4, the color area, for example, there are neurons that only fire if they see objects with a reddish hue, other neurons that fire for blue, and so forth. And each neuron looks only at a small chunk of the visual field, so a specific "red" neuron will only fire when there's a red





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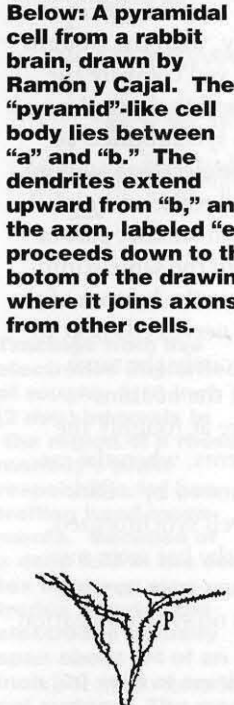
Right: A cross sectional photomicrograph of the macaque visual cortex. The purple dots are nerve-cell bodies, which have been stained to make them visible. Far right: Part of the same region close up. The area photographed is about 1/8 inch (3 mm) from top to bottom. Below: A pyramidal cell from a rabbit brain, drawn by Ramón y Cajal. The "pyramid"-like cell body lies between "a" and "b." The dendrites extend upward from "b," and the axon, labeled "e," proceeds down to the botom of the drawing, where it joins axons from other cells.



object in the part of the field it's responsible for. In a higher part of the visual cortex, certain neurons are only turned on if they see a face. So if you show a face to the monkey, assuming the face is in the part of the visual field that corresponds to where you inserted the electrode, you will see a nerve cell producing electrical activity in the form of pulses.

So every time I see an event, that event corresponds to electrical activity all over the brain. If I look at my friend Bill, say, his face is represented in the brain area where my face neurons are located, the hue of his face is processed in V4, the fact that he's moving around is represented in MT, my memories of him correspond to activity in the temporal lobes, and if he talks, his speech activates my auditory cortex.

Yet if I look at Bill I see a coherent whole. If he speaks to me, his voice isn't disembodied—it comes from his mouth. The color sticks with his face as it moves. And I know that it's Bill's face that's moving, not the background. How this works is a big, big mystery called the binding problem. If some perceived event outside corresponds to electrical activity all over the brain, how can I put it together in a single homogeneous image? Why don't we see the world like a cubist painting, all broken up? Nobody has ever found an area in the brain where everything comes together. A lot of people imagine that somewhere there's a control room, where a little homunculus sits in front of a TV screen so he can see, with speakers so he can hear, and he pulls the levers that make us do things. If you remember Woody Allen's film *Everything You Always Wanted to Know About Sex*, that's exactly the metaphor I mean. This control room doesn't exist—all the brain's activities are highly distributed.

The problem becomes even worse when you consider that while I'm looking at Bill, there are other people behind and next to him who are also moving and talking, and their faces and voices are being registered in these same brain areas by other neurons, yet I'm not confusing Bill or any of his attributes with those of the other people next to him. How is that possible? How come I don't get Bill's voice coming from the man behind him?

All the neurons that correspond to the object I'm attending, like Bill, must carry a common label that the brain recognizes. This label identifies for the brain all the associated neurons that are responding to different aspects of the same object out there in the perceived world. Sometimes this labeling doesn't work—for example, it's frequently a problem with witnesses in criminal cases. The witness sees something very fast—perhaps only for a tenth of a second—and remembers, "There was a man with glasses and a raincoat." And it turns out that there was one man with glasses, and another man with a raincoat, but because it happened so fast, the binding got mixed up, and a feature of one object became attached to another object. This is known as "illusory conjunction." There's also a rare clinical syndrome called disjunctive aphasia, where patients are unable to put things together. If they see two people, they'll mix up the faces, particularly if the people are the same color. Their visual fields contain two overlapping regions of

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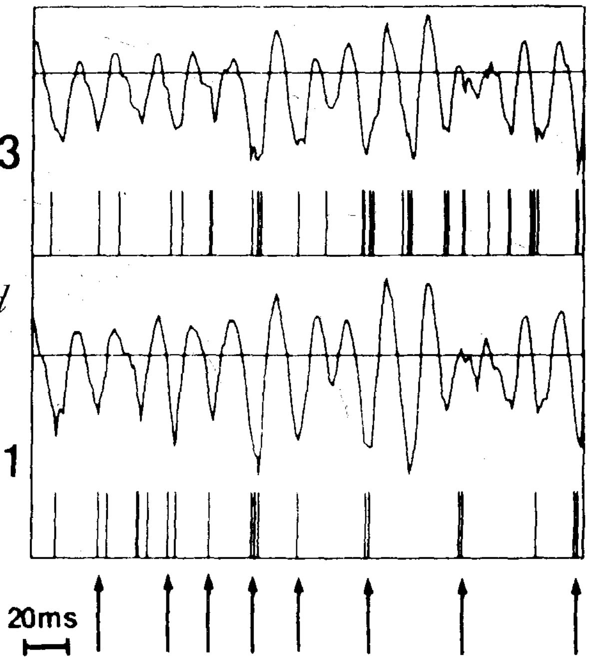


Data recorded from two electrodes, numbered 1 and 3, implanted about 0.03 inches (800 microns) apart in a cat's visual cortex. The cat was looking at a moving bar oriented 112° from the vertical. Because of the way the data is recorded, neurons generate negative peaks when they fire. The scale bar is 0.02 seconds long.

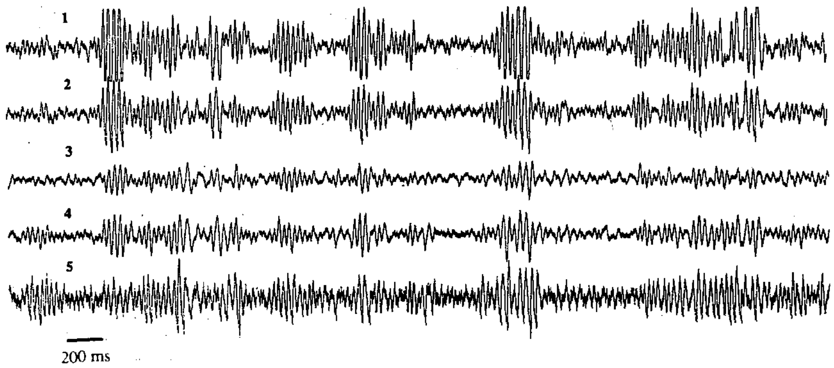
This synchronized oscillation could be the neuronal trace of consciousness.

the same color, and they can't make the separation that one colored region belongs to one face, and the other region of the same color belongs to the other face. The world does look like a cubist collage to them.

What is special about the labeled nerve cells? Is there a set of special consciousness neurons—C-neurons? If so, then every time these C-neurons become activated, you're aware of the thing they correspond to. This poses a problem, because there are an infinite number of things you *could* be aware of, and this theory implies that you'd have to have a special set of neurons for each one—you'd have to have "grandmother neurons" in order to recognize your grandmother. Crick and I think that the awareness neurons are not special, but that they behave in a special way. Every neuron in the cerebral cortex has the potential to be involved, to some extent, in awareness. It's how they respond to a stimulus that matters.

Crick and I think that there's a special pattern of electrical activity that relates to awareness. It's not just that a lot of nerve cells are all firing. If you have an epileptic seizure, *every* nerve cell in your brain is firing, but you're unconscious. The above figure came from a group in Germany, headed by Wolf Singer, working together with Charles Gray, who is now at the Salk Institute. It shows the electrical activity in a cat's visual cortex when the cat saw a moving bar of light. You can think of it as being like the brain waves you see on an EEG, the electroencephalogram that doctors record. People had known about brain waves before, but not about this particular type. It's a high-frequency activity—about 40 cycles per second (cps)—and it only seems to occur under special circumstances. The two halves of the figure show the recordings from two electrodes located about a thirtieth of an inch apart—twenty to thirty cell bodies' distance, roughly. In this case, a single elongated bar of light on a dark background was moving across the receptive fields of neurons at both electrode sites. In each half of the figure, the wavy line on top shows the local field potential—the activity of several thousand nearby neurons, summed together. The series of spikes below it shows the activity from one, or a few, nerve cells closest to the electrode. You can see how the spikes or pulses line up, at least roughly, with the troughs in the local field potential. If you compare the field potentials recorded at the two electrodes, you see that the brain wave is synchronized at both sites. In other words, the electrical activity in one part of the cortex has a precise and global relationship with the activity in another part of the cortex that is responding to the same stimulus. Furthermore, the arrows at the bottom of the figure show where individual nerve cells next to the two electrodes fired at precisely the same instant. In other words, all of the neurons responding to the stimulus fire at roughly the same time. In other experiments, when the cat saw two pieces of the bar separated by a dark zone, the waves were not as well synchronized, even though the two parts of the bar were moving as one. And if the two parts start moving in different directions, there's no synchronization at all.

Crick and I think this is the crux of it. It's a bit of a leap, because the cat in those experiments was lightly anesthetized, but we think that this synchronized oscillation could be the neuronal trace of consciousness. In other words, we think that if you are aware of an event, *all* the nerve cells involved in the perception of that event anywhere in the brain fire at the same time. That is, they fire in a synchronized manner. Other events that you are not aware of—like the sound of traffic outside your window—excite other neurons simultaneously, but these neurons fire randomly. They may even fire at the same rate, but they don't fire in synchronization. The evidence for synchronized neuronal activity is relatively unambiguous in cats. Some of my experimental colleagues also see it in monkeys, as shown in the figure on the opposite page, made by Eberhard Fetz and Venkatesh Murthy at the University of Washington. The traces were recorded from five different electrodes as the monkey was taking raisins from the experimenter's hand. At each electrode, you see big waves consisting of lots of



Tracings from five electrodes implanted at roughly 1/16 inch (2 mm) intervals in the region of a rhesus monkey's brain responsible for controlling hand movements. Because of a deep fold in the cortex between electrodes 2 and 3, the electrodes actually span about 3/4 of an inch (20 mm) of cortical surface. The monkey was reaching for raisins held out of its field of view, a task that required it to feel along the experimenter's arm to locate the raisin and thus to focus its attention on its hand.

smaller spikes, and the big waves are roughly synchronized from electrode to electrode. There's less evidence in man, but a 40 cps oscillation, called an evoked potential, has been found in the auditory domain. You put two electrodes over the temporal lobes of your brain—at your temples, basically—and listen to clicks through earphones. After several hundred clicks, you'll see a few pulses of a 40-cps wave. This wave disappears in deep anesthesia. It does not disappear in sleep, and it does not disappear under light anesthesia. It is being used now in a clinical context by some anesthesiologists to check whether patients are truly under—truly anesthetized—or whether they have merely been paralyzed and rendered amnesic, somewhat like the benzodiazepine recipients I mentioned earlier.

Our theory can easily be tested experimentally. (When I say easily, I mean that conceptually it's quite simple, but actually setting it up would be rather time-consuming.) One way to do it would be to have a monkey looking at a display of red and green bars, some of which are moving to the left and the rest to the right. These figures would be very similar to the Treisman figures I showed you earlier, with the added component of motion. The fact that the bars are either red or green would cause neurons to fire in V4, and the fact that the bars are in motion would make neurons fire in MT. You train the monkey to find the odd bar—if there's only one red bar moving left, for instance—and then you look for synchronized brain waves from V4 and MT. There are subtleties, of course—you'd have to be sure that the odd bar was in the proper part of the visual field to be registered by the nerve cells next to the electrodes—but it could be done.

In conclusion, what are we saying? First, we are saying that we think the time is right to try to start to approach the problem of consciousness—and what it means to be aware of something—in a scientific, reductionist manner at the neuronal level. Crick recalls that 40 years ago, people talked endlessly about the definition of a gene. Rather than worrying about such "meta" questions, Crick, Watson, and their colleagues concentrated on the materialistic foundation of the genetic substance—DNA—and discovered its double-helix structure, and there have been spectacular advances in molecular biology ever since. Second, we argue that to be aware of something you need to attend to it, and you need to put in into short-term memory. At the level of the neuron, this corresponds to a special type of bioelectrical activity. If the neurons firing in this special manner happen to be associated with the "pain" system, you feel pain. Unconscious phenomena—automatic processes, like driving while thinking about something else; or knowledge without awareness, like blindsight—also cause neurons to fire, but not in this special manner. Thirdly, our theory of "awareness" can be tested using today's technology.

If I may draw a comparison between neuroscience and physics, the brain is the most complicated object we know of in the universe. Galaxies are much larger, but they obey a few very simple laws, so their behavior is comparatively easy to predict. By contrast, neuroscience is still in the pre-Galileo stage. The detailed laws that govern the brain's behavior are still unknown, and theories of brain function have a terrible track record. If our model is proven wrong, it won't surprise us greatly, but at least in the process we will have helped clarify the issues that need to be addressed by the next round of theories. □

Christof Koch discussed the neuronal basis of consciousness in a Watson Lecture in March, 1992, on which this article is based. Born in Kansas City, Missouri, and educated in Canada, Morocco and Germany, Koch came to Caltech in 1986, and is now an associate professor of computation and neural systems. When he's not thinking about thinking, Koch designs and builds silicon-based vision systems for robots that mimic the neural hardware of mammalian visual systems. He was originally drawn to the subject of consciousness by the philosophical writings of Arthur Schopenhauer and Ludwig Wittgenstein and the music of Richard Wagner.

Why Don't You Just Write Us a Constitution and Be Done With It

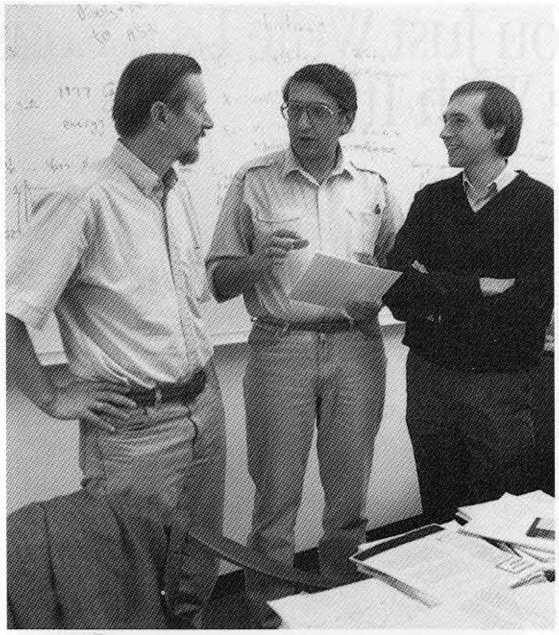
"It was interesting to actually try to write a constitution. It's not easy. There's no handbook sitting out there."

With Lenin's legacy gone, the inexperienced Russians are seeking new democratic political structures.

How do you write a constitution? The Russians need one—badly. They are still operating under Brezhnev's 1978 constitution, which was written for Russia as a republic within the Soviet Union, rather than for Russia as a sovereign state. Times have clearly changed, and the Russian Congress of People's Deputies has amended that document more than 300 times since 1991. But they have refused to amend or eliminate what many consider an especially serious deficiency—Article 104, which essentially gives the Congress the right to do anything, wiping out everything else established by the constitution and making the rest of the document irrelevant. This did not matter until 1991, but trying to live under that document in a meaningful democracy has resulted in chaos. Russia's Constitutional Reform Commission has been laboring for a year and a half and has produced several drafts, but has not come close to writing an acceptable constitution or even a draft of such a document. President Yeltsin has his own proposal, which is for the most part no better than any of the alternatives—a ponderous document that seems designed only to ensure a president with powers that parallel those of a czar.

"Why I cared about it, I'm not sure," says Professor of Political Science Peter Ordeshook, who speaks no Russian and has not, until recently, been particularly concerned with Russian politics. But in the last couple of years Caltech's Division of the Humanities and Social Sciences has played host to a number of Russian social scientists—both visiting faculty and graduate students (*Caltech News*, February 1993)—who drew the Americans into discussions of the political problems faced at home. In the immediate aftermath of the Soviet Union's breakup, says Ordeshook, everything seemed to focus on the issue of economic reform. "When people talked about social scientists going over to Russia or to Eastern Europe to make suggestions, they were really talking about economists," he says. "From my perspective, as a political scientist, I wasn't sure initially what comes first—economic reform or political reform." But after observing the political chaos there preclude sustainable economic transformation, the necessity for simultaneous political institutional change became self-evident. Unfortunately, he did not see much serious effort going toward political reform. When Russian graduate students asked Ordeshook to look at the Constitutional Reform Commission's first draft of a constitution, he promptly pronounced it "terrible." He wrote an analysis of its deficiencies for *Izvestia* (in which he diplomatically described it as "a valiant attempt") at the end of 1991, and made a couple of trips to Russia to consult with politicians and academics wrestling with the constitution problem. But as subsequent drafts of the document appeared and he did not see any improvement—"they didn't seem to be getting anywhere; they just didn't seem to have any understanding of what they were doing"—Ordeshook's frustration level rose. "It was clear they were operating from a wholly incorrect philosophy, or a wholly incorrect set of principles. Or no principles at all. I couldn't tell."

Finally Vyacheslav Nikonov, a Russian



Peter Ordeshook (left) and Vyacheslav Nikonov (right) discuss the Russian constitution with another visiting lecturer at Caltech, Fuad Aleskerov, a Moscow economics professor and research department director at the Institute of Control Sciences of the Russian Academy of Sciences.

"We didn't write this thing with the idea that we're going to go over there, and people are going to read it and say, 'Wow, this is brilliant; let's use it.'"

counselor for the International Fund for Economic and Social Reforms (and following the 1991 coup, second in command of the KGB), who was visiting Caltech in the 1992 fall quarter and had to listen to Ordeshook's grumbling, invited him to put up or shut up—specifically, to write a series of newspaper articles on what the democratic process was all about, a sort of "pre-Federalist Papers" for ordinary Russians. A subsequent discussion with the editor of *Nezavisimaya Gazeta,* Russia's largest independent newspaper, to ascertain interest in such a series, evoked from the editor the half-kidding comment, "Why don't you just write us a constitution and be done with it." So, in addition to the essays, Ordeshook decided to write a constitution. He drafted a colleague, Tom Schwartz, professor of political science at UCLA, another visitor to Caltech, to collaborate on the task.

"It was interesting to actually try to write a constitution," said Ordeshook. "It's not easy. There's no handbook sitting out there." But he found himself intrigued and challenged by what he calls "probably the greatest political engineering-design problem of the 20th century. For a political scientist, it involves some really fundamentally interesting questions—particularly in institutional design. It's an empirical challenge to try to figure out what the right institutions are for this rather crazy place." Ordeshook isn't a bit sanguine about the Russians actually adopting his draft but is glad to have undertaken the challenge. "We didn't write this thing with the idea that we're going to go over there, and people are going to read it and say, 'Wow, this is brilliant; let's use it.' But we hoped to draft something that at least presented a structure in which people could see the philosophy behind a constitution."

Ordeshook and Schwartz sat down and wrote their draft in three weeks in January. Even the American founding fathers took the whole summer of 1787 to complete their enterprise, which is actually the world's oldest surviving written constitution. But if the two professors did not have a handbook, they did at least have a model in that document. "Aside from the fact that it has a completely different history and a completely different economy, and there's 140 million of them and, when we began as a country, only 3 or 4 million of us, America is still probably the most relevant model for Russia," says Ordeshook. "It's not a perfect model for many reasons, but none of the other stable democracies—Germany, France, Italy, Switzerland—comes close to approximating the match between the U.S. and Russia. Its size and ethnic diversity are similar to ours, and the fact that it's a federation with regional and local interests means that there will be a continuing tension between Moscow and the regions over jurisdiction and authority, just as there has been here."

The Ordeshook/Schwartz draft runs to less than 10 pages, double spaced, and 40 articles—approximately the same length as the U.S. Constitution. Compared to the 69 pages and 133 articles of the Commission draft, and 50 pages and 130 articles for Yeltsin's, this sounds rather skimpy. But Ordeshook believes, "A constitution should say *no more than necessary* to start the

Article 13: The state is responsible for promoting these objects:

- *Adequate income for all, including wage earners, the unemployed and disabled, widows and orphans, veterans, victims of repression, and retired persons.*
- *Primary, secondary, and vocational education for all, and higher education for all according to ability.*
- *The viability of families.*
- *Medical care for all.*
- *Housing for all.*
- *Compensation for damage done illegally to one's health, dignity, good name, or property.*
- *Environmental and ecological safety.*
- *Preservation of the natural and cultural heritage of the Russian Foundation.*
- *The safety and healthfulness of the workplace.*
- *Promotion of the arts and sciences.*
- *Development of industry and transportation.*
- *Promotion and efficient regulation of commerce.*
- *Protection of ethnic, social, national, and religious minorities.*
- *Safety against crime.*
- *Protection of consumers against fraud, unsafe products, and anti-competitive practices.*
- *Protection of proprietary and contractual claims.*
- *Defense of the state and this Constitution.*
- *Democratic self-government in every federal subject.*
- *The ready means to petition the state for a redress of grievances.*

The state shall not act but in support of these objects.

Russians expect social guarantees to be included among the state's responsibilities. Ordeshook and Schwartz had to put them into their Article 13 but with very careful wording.

government and create a self-enforcing, adaptable, and fair process." And any constitution writer needs to begin with the idea of what a constitution is. In Ordeshook's view, it is "a document that the sovereign, the people, use to define their agent, the state, to act on their behalf, and to place limits on the state. If you begin expanding the document beyond that, it starts to muddy things up and to lose its real purpose." The Russians, in contrast, have tried to include clauses that cover just about everything a citizen could ever wish for—decreeing, for example, that parents will take care of their children and that the children will take care of aged parents. The constitutions of Stalin and Brezhnev were a "complete candy store of every conceivable right, including the right to free housing, free medical care, paid vacations, and so on." Although he would have preferred to do away with such social guarantees altogether, Ordeshook did not manage to escape the universal expectation that they should be in there. His article 13 includes such things as adequate income for all; the viability of families; medical care for all; housing for all; and environmental and ecological safety. But the trick lay in converting the article's meaning from guarantee into merely empowering the state to act in these domains. Article 13 reads: "The state is responsible for promoting these objects: . . ." "It enables the state to seek to establish, say, housing for everybody," says Ordeshook. "It does not necessarily mean that the state's *going* to do anything, but it says the state *can* do something. It is then up to the political process as directed by the institutions the constitution establishes, to determine whether and to what extent the national government will become involved with such matters."

Yet another problem is that Russians want to write a constitution, especially its provision of rights, like a business contract in which every circumstance and contingency is explicitly recognized and planned for, says the Caltech professor. "They're afraid of overly constraining the state." Instead of saying, for example, that the legislature shall pass no law infringing on freedom of the press, they want to say "the legislature shall pass no law infringing the freedom of the press except in the following cases. . ." Similarly, the Russians want to put in a constitutional provision saying that the legislature will pass no law infringing on the right of the people to peacefully assemble—except when people are trying to agitate for war, to undermine the democratic institutions of Russia or to cause enmity among groups. If the document does state a right without conditions, it's usually undermined later

"The Russians don't trust institutions. . . .They don't understand that it's not the words in a constitution that guarantee individual liberties; it's the institutions that the document establishes."

by another clause that says "these rights can only be infringed upon by law." "The net result of all of these qualifications, of course, is that you end up with no rights at all," says Ordeshook.

While the Russians are eager to expend thousands of constitutional words trying to describe specifically all the instances in which the state may be allowed to infringe upon a right, they are willing to tolerate a remarkable amount of ambiguity when it comes to defining institutional structure. This they expect the "law" to do for them. The Commission draft, for example, is very weak on constructing a separation of powers and Yeltsin's draft wholly abrogates any separation in favor of a presidential near-dictatorship. Also, neither version defines how the president and legislators are to be elected (there are numerous possibilities for a presidential election: simple plurality, regional distribution requirements, electoral college, or a simple majority vote with runoffs, which the Russians favor), or when they will take office or leave it. Ordeshook finds this approach of too much specificity in one place and too little in another, ominously inconsistent. It's in their understanding of the role of institutions, he believes, that the Russian approach completely breaks down. If institutions are designed well, then the appropriate legislation guaranteeing and qualifying rights to suit society's needs will follow. "The Russians don't trust institutions," he says. "They don't understand that it's not the words in a constitution that guarantee individual liberties; it's the institutions that the document establishes. In Marxist philosophy institutions were ephemeral things, merely dictated by the flow of events in society." Thus, rather than focus on the critical matter of political institutional design, Russians instead focus on elaborate and unenforceable statements of rights and vague principles.

Institutional design, the ultimate basis of the enforcement of rights, is what Ordeshook considers the most interesting challenge currently facing the Russians, in particular the debate over whether the country should have a presidential or a parliamentary system. Yeltsin's draft opts for an overly strong presidency. The Commission draft specifies a more modest and reasonable presidential system. An alternative constitution proposed by Anatoly Sobchak, mayor of St. Petersburg, opts for a mixed presidential-parliamentary system, which, unsurprisingly, the current members of the Russian Supreme Soviet prefer. Ordeshook comes down on the side of the presidency, but not for the same reasons that the Russians do. They claim that Russia needs a strong leader, either because it's a cultural tradition or because a strong leader is needed in these times of chaos and economic struggle, says Ordeshook. He, on the other hand, sees the presidency as necessary to prevent the political fragmentation and disintegration of the Russian Federation as a product of the nature of the political parties that will emerge eventually to compete against each other. "If you want to keep Russia whole, you have to ask the question: what kind of political party system is best, given Russia's circumstances? What you don't want is a system in which there are a lot of small ethnic regional political parties competing against each other. And that's what parliamentarianism is likely to generate—dozens of small regional parties and complete government instability. I could easily see a parliamentary Russia in which no government survived longer than six months— a replay of Poland or Italy.

"A presidential system, on the other hand, at least has the advantage of providing a chief prize for the parties to win, and this creates an incentive for parties to coalesce across regional and ethnic boundaries," says Ordeshook. Again, he finds relevance in the United States, which, he says, doesn't have merely two major political parties, but 100—50 Democratic Parties and 50 Republican Parties. This isn't surprising, because all elections except the presidency take place on a state or local level. "Delegations of *state* parties meet at a national convention every four years to nominate a president. They find it convenient to coalesce under two labels for the sake of winning this prize. Thus, although the state parties provide a natural protection for local interests, the

Ordeshook snapped this street scene of Russian capitalism on a recent Moscow visit.

"Brezhnev's 1978 constitution says: here's a constitution but *the Communist Party is the leading authority on everything."*

lure of the presidency leads the parties to negotiate many of their regional conflicts internally, before they are allowed to disrupt national politics."

The problem, though, with explaining the potential role of parties in ensuring political stability, says Ordeshook, is that Russians have no concept of what a political party is or does in anything other than the most superficial sense. "Brezhnev's 1978 constitution says: here's a constitution *but* the Communist Party is the leading authority on everything." Stalin's constitution doesn't even mention the Communist Party, which the political science professor found paradoxical. When he asked about it, he was told, "Under Stalin they didn't have to say it." In any event, with no experience in democratic process, Russians only have the example of the Communist Party when thinking about parties under any new constitution.

The failure to appreciate the nature of parties in a democracy causes Russians to fail to appreciate the fact that the U.S. state party system has provided an important protection of the enormous autonomy of the American states and consequently of the overall stability of the American federal system. Even though there has been gradual erosion, the autonomy of the individual states remains greater than in most federal countries, with the possible exception of Switzerland. The representation given to states in the Senate provides an additional protection of states. So Ordeshook and Schwartz put a bicameral legislature into their constitution, with an upper chamber similar to the American Senate and a lower chamber to be elected through single-member districts. Unfortunately, the Russian Federation is a hodge-podge of republics, oblasts, krays, and autonomous okrugs, which makes developing a fair system of representation in an upper house a nightmare. About half of the republics are populated by a majority of ethnic Russians, but the rest contain significant numbers of other ethnic groups. Oblasts, which largely derive from 19th-century administrative divisions, are between 90 and 95 percent ethnic Russian, but occasionally territories exist within them that have been carved out for ethnic minorities and given some special consideration, like an Indian reservation. These are called autonomous okrugs; an oblast with an autonomous okrug in it is called a kray. They all have different internal political structures and different degrees of autonomy with respect to Moscow—differences that the Yeltsin draft maintains (the other drafts are too ambiguous to identify their implications in this matter).

This makes for a very asymmetric federation, which is bound to lead to what Ordeshook calls "the teachers union problem: When it comes time to negotiate a contract, every teachers union in the U.S. can find some other school district that's rewarding its teachers more in terms of salary, pensions, hours in the classroom, etc. And so when new contracts are negotiated you get an escalation of demands across school districts." Or across republics and oblasts and krays in this instance. "Every republic or oblast can find some other republic or oblast that on some dimension is in a more advantageous position than it is with

PREAMBLE

We, the multinational people of the Russian Federation, to secure the peace and safety of society, to establish the legal foundations for general prosperity, to foster justice and harmony among all of us, to protect the freedom and dignity of each one of us, and to preserve the unity and patrimony of Russia, adopt this Constitution and proclaim the Russian Federation to be a democratic, federal, and social republic based on the rule of law, the inalienable rights of the individual, and the separation of legislative, executive, and judicial powers.

Right: Ordeshook (left) and Schwartz stroll in the Moscow snow, which, they discovered, is pretty much like snow everywhere.
Left: The preamble to the constitution drafted by the two Americans aludes to universal principles that should hold for *Russia as they do for* other countries.

"Two years ago, if somebody had showed up in Moscow with a constitution labeled 'Made in the USA,' it would have had enormous appeal. . . . But right now . . . it's the kiss of death."

respect to its relationship with Moscow. These leapfrogging demands are all headed in one direction, and that is toward the division of Russia," Ordeshook maintains. He and fellow constitution writer Schwartz sought to clean up some of these fuzzy boundaries, combine some of the oblasts into single units, and establish uniform degrees of autonomy and regional responsibility, but their Russian colleagues tell them this is unrealistic—for unexplained reasons.

Russia does already have a legislature, elected in 1990 under the former constitution (most of its members ran unopposed). But they were elected as the legislature not of a country but of a republic, which had little power as part of the U.S.S.R. Ordeshook likens the situation the Russians now find themselves in to a hypothetical California— a California in which the governor and the legislature didn't really have much to do. "And then all of a sudden Washington disappears, and these characters in Sacramento are left with the nuclear weapons. That's what basically happened." If this weren't a frightening enough predicament, the Russians have had no experience with democracy. Other countries in this century that started writing constitutions from scratch at least had some democratic traditions ("And Germany and Japan had the American army sitting there too.") Even China, says Ordeshook, has more experience with democracy than Russia. "Russia has absolutely none! Zero. It's really hard to imagine. Russia is almost the proverbial blank slate."

Nevertheless, the Russians are not exactly rushing to embrace an American-type constitution. Says Ordeshook, "Two years ago, if somebody had showed up in Moscow with a constitution labeled 'Made in the USA,' it would have had enormous appeal—Russians admire America more perhaps than any other country. But right now with much of the population believing that Russia's leaders are selling out their country to the West, it's the kiss of death." The two American political science professors did visit Russia again this past February to get a closer view of the problems. "Trying to understand it from here gets very murky and confusing." While there, they collaborated with an ad hoc committee of Russian academics, politicians, and businessmen who were also writing an alternative constitution—and which just happened to include Ordeshook's colleague Nikonov. "What exactly they will do with it I don't know. But we spent a couple of weeks going back and forth between our draft and the draft they were working on. We were learning some realities of the political situation, and I hope they were learning something about what it means to say, for example, 'the rule of law.' They would use such a phrase but not have the foggiest idea what it means."

They also met with a variety of other people, trying to get a sense of what different interests were. One such encounter took Ordeshook and Schwartz to a rural village outside of Moscow, a trip that came to epitomize for the two Americans their frustration with the Russian experience. Ordeshook calls it "The Parable of the Snows." They were driving back to Moscow with Nikonov, his mother-in-law (who had contacts

"But the Russians are doggedly determined to believe there's something special about Russian snow, something special about the Russian soul, something special about Russia that's not shared by anyone else."

with the village), and another Russian, who owned the car. It was snowing, and, in attempting a short cut back to the city, the car got stuck in three feet of snow on a sheet of ice. While Schwartz kept suggesting that they break up branches to put under the rear wheels and push the car out, the Russian men completely ignored him. Finally, without saying a word, they strode off into the night in the direction the car was headed, with Ordeshook in hot pursuit to find out what they were doing. They were going to the Minsk highway to wave down a truck to pull them out. "I commented that the truck would just get stuck in the snow too, but they said, 'Don't worry about this. We're Russians. We do this all the time.'" After about half an hour, a truck was flagged down, a deal negotiated, the truck started off on its rescue mission—and promptly got stuck in the snow. Ordeshook hiked back to the car, where, in the meantime, Schwartz and the mother-in-law, who spoke no English, had packed branches and other junk under the rear wheels. When the Russian men reappeared, they continued to ignore the potential of this solution, but the Americans finally insisted that the driver back the car up while they pushed. "It took about 15-20 minutes, a meter at a time, putting stuff under the wheels, and then we were out, free. We could turn the car around and go back the way we had come. Insofar as the truck is concerned, for all I know, it's still there."

Says Ordeshook: "The lesson you get out of this is that there are fundamental principles—force, friction, action-reaction, etc.—that apply universally, regardless of culture, regardless of where you are on the planet, regardless of the language you speak, regardless of whose snow it is. But the Russians are doggedly determined to believe there's something special about Russian snow, something special about the Russian soul, something special about Russia that's not shared by anyone else. They just ignore common principles and go marching off in some bizarre direction. And this is what we encountered with the constitution. They'd say, 'You just can't do that in Russia.' It was obviously one of their beliefs that there was something special about their country that negated constitutional democratic principles that applied everywhere else on the planet."

Ordeshook figures the Russians may muddle along for a while and perhaps end up with a gigantic compromise document that will look like "fish soup." He has no clear preference over any of the current "official" proposals—by Yeltsin, the Commission, or Sobchak—since they are all of such inferior quality that Russia would only embarrass itself by adopting any one of them. But he can't really see how anything coherent is going to get written and adopted. "I don't see the current Congress of People's Deputies doing it; and I'm not sure about the next Congress or any other one. It's going to be a long process." □ *–JD*

Peter Ordeshook has been a professor of political science at Caltech since 1987. He plans another trip to Russia in July—unless his criticism of Yeltsin's constitution, recently published there, is taken amiss.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Scientific Elites and Scientific Illiterates

How can our miserable system of education have produced such a brilliant community of scientists?

by David L. Goodstein

Is American science education like mining? Here Georgius Agricola illustrates the process of mining and sifting copper ore in the Carpathians in *De Re Metallica*, 1556.

Scientific papers often begin by posing a paradox, even if it is one that had not previously seemed particularly disturbing. Having posed the paradox, the author then proceeds to resolve it. At first glance, we don't seem to make much progress that way. A paradox that was previously unnoticed is now no longer unexplained. Such exercises, however, can sometimes be useful. For example, Albert Einstein's famous 1905 paper introducing the theory of relativity was very much of this form. He began by pointing out that when a magnet induces an electric current in a loop of wire, we attribute that effect to entirely different causes depending on whether the magnet or the wire is in motion. Finding this paradox intolerable, he proceeded to resolve it, giving new meanings to time and space along the way.

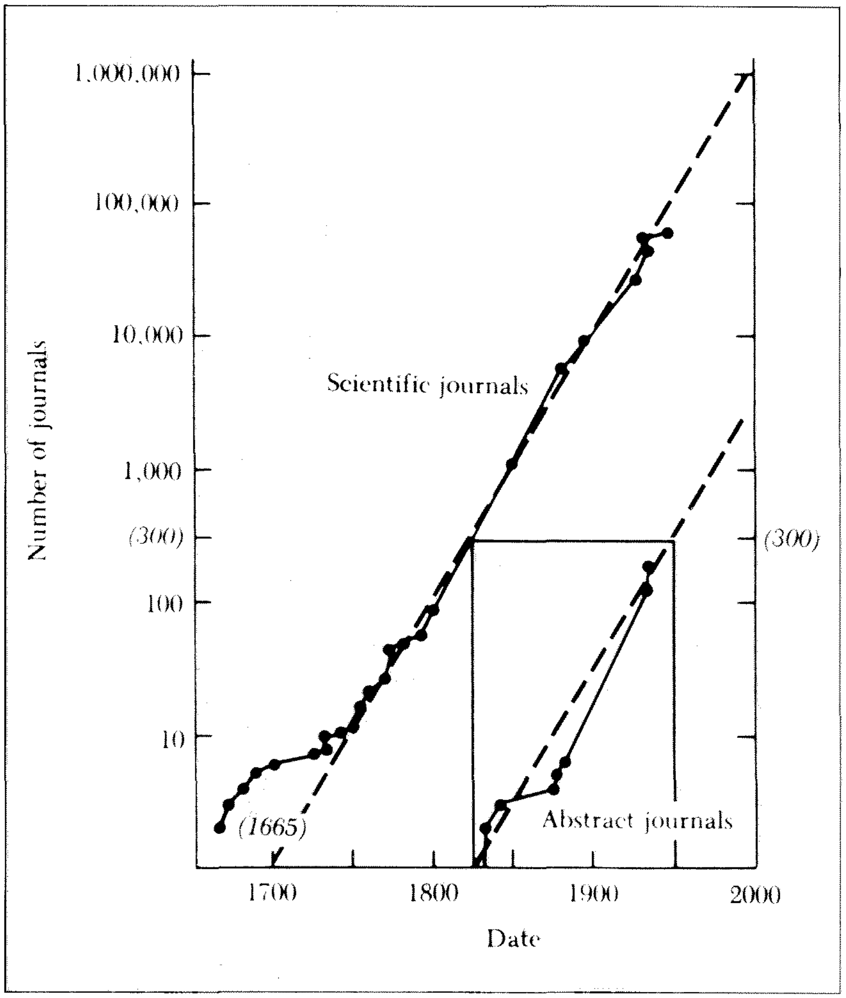
Today, with my customary modesty, I would like to follow in Albert's bicycle tracks and begin this talk by posing a paradox. The paradox is that we, here in the United States today, have the finest scientists in the world, and we also have the worst science education in the world, or at least in the industrialized world. American scientists, trained in American graduate schools, produced more Nobel Prizes, more scientific citations, more of just about anything you care to measure, than any other country in the world; maybe more than the rest of the world combined. Yet, students in American schools consistently rank at the bottom of all those from advanced nations in tests of scientific knowledge; and furthermore roughly 95 percent of the American public is consistently found to be scientifically illiterate by any rational standard. How can we possibly have arrived at such a result? How can our miserable system of education have produced such a brilliant community of scientists? I would like to refer to this situation as the Paradox of the Scientific Elites and the Scientific Illiterates.

In my view, these two seemingly contradictory observations are both true, and they are closely related to one another. We have created a kind of feudal aristocracy in American science, where a privileged few hold court, while the toiling masses huddle in darkness, metaphorically speaking, of course. But I think inexorable historic forces are at work that have already begun to bring those conditions to an end—not that light will be brought to the masses necessarily, but that *our* days at court are clearly numbered. To understand all this, and before we get more deeply mired in dubious metaphors, it may help to go back to the beginning. I mean literally The Beginning.

In modern cosmology, the accepted theory of the beginning of the universe goes something like this: at a certain instant around 10–15 billion years ago, the universe was created in a cataclysmic event called the Big Bang. It has been expanding uniformly ever since. What we do not know is whether the density of matter in the universe is great enough to reverse that expansion eventually, causing the universe to slow down, come to a stop, and then finally fall back upon itself. If that does happen, the cosmologists are prepared with a name for the final cataclysmic moment when the universe ends. It will be known as the Big Crunch.

I would like to offer a somewhat analogous

The number of founded scientific journals plotted as a function of year grew at an exponential rate between 1750 and 1950, leading to a somewhat tongue-in-cheek prediction of a million journals for a scientist to consume by the year 2000.



From Derek da Solla Price, *Science Since Babylon* (Yale University Press, 1961). Reprinted by permission.

theory of the history of science. According to this theory, science began in a cataclysmic event sometime around the year 1700. (The publication of Newton's Principia in 1687 is a good candidate for the actual event.) It then proceeded to expand at a smooth, continuous exponential rate for nearly 300 years. Unlike the universe, however, science did not expand into nothing at all. Instead, the expansion must come to an end when science reaches the natural limits imposed on it by the system it was born into, which is called the Human Race.

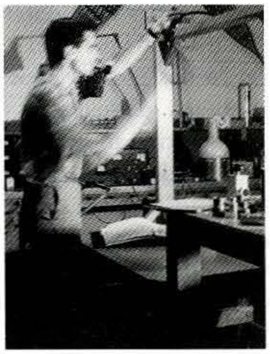
I don't mean that scientific knowledge is limited by the human race; in fact, I don't think scientific knowledge is limited at all, and I hope it will go on expanding forever. What I'm talking about here is what you might call the profession of science, or the business of science. It is my opinion that the size of the scientific enterprise, which began its exponential expansion around 1700, has now begun to reach the limits imposed on it by the size of the human race. Thus, the expansion of science is now in the process of ending, not in a Big Crunch, but in something much more like a whimper, that may or may not leave some residue of science still existing when it is all over.

I think that the beginning of the end of the exponential expansion era of science occurred, in the United States at least, around the year 1970. Most people, scientists and otherwise, are unaware that it is coming to an end (in fact, they probably never knew it existed) and are still trying to maintain a social structure of science (by which I mean research, education, funding, institutions and so on) that is based on the unexamined assumption that the future will be just like the past. Since that is impossible, I believe, we have some very interesting times ahead of us. I would like to explain why I believe all this, and what we might try to do about it.

The graph at left is borrowed from a book called *Little Science, Big Science* by Derek da Solla Price. Price may be identified as the Edwin Hubble of the expansion of science. (Hubble discovered the expansion of the universe.) The figure, a plot of the number of scientific journals founded, worldwide, as a function of year, is a suitable stand-in for any other quantitative measure of the size of science. It shows that the cumulative number of journals founded increased by a factor of 10 about every 50 years, from 1750 to 1950. This is a different, faster kind of growth than a free expansion like that of the universe. Here the rate of growth of the system keeps increasing as the size of the system increases. In other words, the bigger it is, the faster it grows.

It is a simple mathematical fact that if scientists keep multiplying faster than people, there will soon be more scientists than there are people.

Anyone observing this so-called exponential curve would conclude that science was born (roughly) in the year 1700, and that a million journals would have been founded by the year 2000. Price, who pointed out this phenomenon in the early 1960s, was clever enough to know that neither of these conclusions would be correct. On the one hand, both scientific knowledge and the scientific enterprise have roots that stretch all the way back to antiquity, and on the other hand the number of scientific journals in the world today, as we approach the year 2000, is a mere 40,000. This sorry failure of the publishing industry to keep up with our expectations often leaves us scientists with nothing to read by the time we reach the end of the week.



The author during the Golden Age of 1962.

The point is that the era of exponential growth in science is already over. The number of journals is one measure, but all others tend to agree. In particular, it applies to the number of scientists around. It is probably still true that 90 percent of all the scientists who have ever lived are alive today, and that statement has been true at any given time for nearly 300 years. But it cannot go on being true for very much longer. Even with the huge increase in world population in this century, only about one-twentieth of all the people who have ever lived are alive today. It is a simple mathematical fact that if scientists keep multiplying faster than people, there will soon be more scientists than there are people. That seems very unlikely to happen.

I think the last 40 years in the United States have seen the end of the long era of exponential growth and the beginning of a new era we have not yet begun to imagine. These years will be seen in the future as the period in which science began a dramatic and irreversible change into an entirely new regime. Let's look back at what has happened in those 40 years in light of this historic transformation.

The period 1950–1970 was a true golden age for American science. Young PhDs could choose among excellent jobs, and anyone with a decent scientific idea could be sure of getting funds to pursue it. The impressive successes of scientific projects during World War II had paved the way for the federal government to assume responsibility for the support of basic research. Moreover, much of the rest of the world was still crippled by the aftereffects of the war. At the same time, the G.I. Bill of Rights sent a whole generation back to college. The American academic enterprise grew explosively, especially in science and technology. Even so, that explosive growth was merely a seamless continuation of the exponential growth of science that had dated back to 1700. It seemed to one and all (with the notable exception of Derek da Solla Price) that these happy conditions would go on forever.

By now, in the 1990s, the situation has changed dramatically. With the Cold War over, national security is rapidly losing its appeal as a means of generating support for scientific research. To make matters worse, the country is $4 trillion in debt, and scientific research is among the few items of discretionary spending in the national budget. There is much wringing of hands about impending shortages of trained scientific talent to ensure the nation's future competitiveness, especially since by now other countries have been restored to economic and scientific vigor. But, in fact, jobs are scarce for recent graduates. The best American students have proved their superior abilities by reading the handwriting on the wall and going into other lines of work. Half the students in American graduate schools in science and technology are from abroad.

Both periods, the euphoric golden age, 1950–1970, and the beginning of the crunch, 1970–1990, seemed at the time to be the product of specific temporary conditions rather than grand historic trends. In the earlier period, the prestige of science after helping win the war created a money pipeline from Washington into the great research universities. At the same time, the G.I. Bill of Rights transformed the United States from a nation of elite higher education to a nation of mass higher education. Before the war, about 8 percent of Americans went to college, a figure comparable to that in France or England.

Now more than half of all Americans receive some sort of post-secondary education, and nearly a third will eventually graduate from college. To be sure, this great and noble experiment in mass higher education has failed utterly and completely in technology and science, where between 4 and 5 percent of the population can be identified as science and technology professionals, and the rest may as well live in the pre-Newtonian era. Nevertheless, the expanding academic world in 1950–1970 created posts for the exploding number of new science PhDs, whose research led to the founding of journals, to the acquisition of prizes and awards, and to increases in every other measure of the size and quality of science. At the same time, many great American corporations decided they needed to create or expand their central research laboratories to solve technological problems, and also to pursue basic research that would provide ideas for future developments. And the federal government itself established a network of excellent national laboratories that also became the source of jobs and opportunities for aspiring scientists. As we have already seen, all this extraordinary activity merely resulted in a 20-year extension in the U.S. of the exponential growth that had been quietly going on since 1700. It was to be the last 20 years, however. The expansionary era in the history of science was about to come to an end, at least in America.

Actually, during the second period, 1970–1990, the expansion of American science did not stop altogether, but it did slow down significantly compared to what might have been expected from Price's exponential curves. Federal funding of scientific research, in inflation-corrected dollars, doubled during that period, and, by no coincidence at all, the number of academic researchers also doubled. Such a controlled rate of growth (controlled only by the available funding, to be sure) was not, however, consistent with the lifestyle that academic researchers had evolved. The average American professor in a research university turns out about 15 PhD students in the course of a career. In a stable, steady-state world of science, only one of those 15 can go on to become another professor in a research university. In a steady-state world, it is mathematically obvious that the professor's only reproductive role is to produce one professor for the next generation. But the American PhD is basically training to become a research professor. American students, realizing that graduate school had become a training ground for a profession that no longer offered much opportunity, started choosing other options. The impact of this situation was obscured somewhat by the growth of postdoctoral research positions, a kind of holding tank for scientific talent that allowed young researchers to delay confronting reality for three to six or more years. Nevertheless, it is true that the number of the best American students deciding to go to graduate school started to decline around 1970, and it has been declining ever since.

In a steady-state world, it is mathematically obvious that the professor's only reproductive role is to produce one professor for the next generation.

In the meantime, yet one more surprising phenomenon has taken place. The golden age of American academic science produced genuine excellence in American universities. Without any doubt at all, we lead the world in scientific training and research. It became necessary for serious young scientists from everywhere else either to obtain an American PhD, or at least to spend a year or more of postgraduate study here. America has come to play the role for the rest of the world, especially the emerging nations of the Pacific rim, that Europe once played for young American scientists, and, it is said, that Greece once played for Rome. We have become the primary source of scientific culture and learning for everyone. Almost unnoticed, over the past 20 years the missing American graduate students have been replaced by foreign students. This has permitted the American research universities to go on producing PhDs almost as before.

It should be clear by now that with half the kids in America already going to college, academic expansion is finished. With the Cold War over, competition in science can no longer be sold as a matter of national survival. There are those who argue that research is essential for our economic future, but the managers of the economy know better. The great corporations have decided that central research laboratories were not such a good idea after all. Many of the national laboratories have lost their missions and have not found new ones. The economy has gradually been transformed from manufacturing to service, and service industries such as banking and insurance don't support much scientific research. Although each of these conditions appears to be transient and temporary, they are really the immediate symptoms of a large-scale historic transformation. For us in the United States, the expansionary era of science has come to an end. The future of American science will be very different from the past.

Let's get back now to the Paradox of Scientific Elites and Scientific Illiterates. The question of how we educate our young in science lies at the heart of the issues we have been discussing. The observation that for hundreds of years the number of scientists had been growing exponentially means, quite simply, that the rate at which we produced scientists has always been proportional

I believe it is a serious mistake to think of our system of education as a pipeline leading to PhDs in science or in anything else.

to the number of scientists that already existed. We have already seen how that process works at the final stage of education, where each professor in a research university turns out 15 PhDs, most of those wanting to become research professors and turn out 15 more PhDs.

Recently, however, a vastly different picture of science education has been put forth and has come to be widely accepted. It is the metaphor of the pipeline, illustrated above on the cover of an issue of *Science* magazine from last November. The idea is that our young people start out as a torrent of enthusiastic, curious minds eager to learn about the world, but as they pass through the various grades of schooling, that eagerness and curiosity is somehow squandered, fewer and fewer of them showing any interest in science, until at the end of the line, nothing is left but a mere trickle of PhDs. (The artist for *Science* didn't get the idea of a trickle quite right.) Thus, our entire system of education is seen to be a leaky pipeline, badly in need of repairs. As the cover of *Science* indicates the leakage problem is seen as particularly severe with regard to women and minorities (even the "trickle" at the end is milky white), but the pipeline metaphor applies to all. I'm not quite sure, but I think the pipeline metaphor came first out of the National Science Foundation, which keeps careful track of science workforce statistics (at least that's where I first heard it). As the NSF points out with particular urgency, women and minorities will make up the majority of our working people in future years. If we don't find a way to keep them in the pipeline, where will our future scientists come from?

I believe it is a serious mistake to think of our system of education as a pipeline leading to PhDs in science or in anything else. For one thing, if it were a leaky pipeline, and it could be repaired, then, as we've already seen, we would soon have a flood of PhDs that we wouldn't know what to do with. For another thing, producing PhDs is simply not the purpose of our system of education. Its purpose instead is to produce citizens capable of operating a Jeffersonian democracy, and also if possible capable of contributing to their own and to the collective economic well being. To regard anyone who has achieved those purposes as having leaked out of the pipeline is worse than arrogant; it is silly. Finally, the picture doesn't work in the sense of a scientific model: it doesn't make the right predictions. We have already seen that, in the absence of external constraints, the size of science grows exponentially. A pipeline, leaky or otherwise, would not have that result. It would only produce scientists in proportion to the flow of entering students.

I would like to propose a different and more illuminating metaphor for science education. It is more like a mining and sorting operation, designed to cast aside most of the mass of common human debris, but at the same time to discover and rescue diamonds in the rough, that are capable of being cleaned and cut and polished into glittering gems, just like us, the existing scientists. It takes only a little reflection to see how much more this model accounts for than the pipeline does. It accounts for exponential growth, since it takes scientists to identify prospective scientists. It accounts for the very

Rather than a pipeline, science education could be compared to a mining and sorting operation, separating from the huge mass of ore diamonds that can be cleaned, cut, and polished—into, say, physicists. At far left is the diamond treatment plant at the Auchas Mine in Namibia.

Our colleagues abroad can take what scant comfort they can find in the promise that our dilemmas in science and education are on the way, along with Big Macs and designer jeans.

real problem that women and minorities are woefully underrepresented among scientists, because it is hard for us white, male scientists to perceive that once they are cleaned, cut, and polished they will look like us. It accounts for the fact that science education is for the most part a dreary business, a burden to student and teacher alike at all levels of American education, until the magic moment when a teacher recognizes a potential peer, at which point it becomes exhilarating and successful. Above all, it resolves the paradox of Scientific Elites and Scientific Illiterates. It explains why we have the best scientists and the most poorly educated students in the world. It is because our entire system of education is designed to produce precisely that result.

It is easy to see the sorting operation at work in the college physics classroom, where most of my own experience is centered, but I believe it works at all levels of education and in many other subjects. From elementary school to graduate school, from art and literature to chemistry and physics, students and teachers with similar inclinations resonate with one another. The tendency is natural and universal. But, if it is so universal, you might ask, why is America so much worse off than the rest of the world? The answer, I think, is that in education and in science, as in fast food and popular culture, America is not really worse than the rest of the world; we are merely a few years ahead of the rest of the world. What we are seeing here will happen everywhere soon enough. Our colleagues abroad can take what scant comfort they can find in the promise that our dilemmas in science and education are on the way, along with Big Macs and designer jeans.

Getting back to America, the mining and sorting operation that we call science education begins in elementary school. Most elementary school teachers are poorly prepared to present even the simplest lessons in scientific or mathematical subjects. In many places, elementary education is the only college major that does not require even a single science course, and it is said that many students who choose that major do so precisely to avoid having to take a course in science. To the extent that this is true, elementary school teachers are not merely ignorant of science, they are preselected for their hostility to science, and no doubt they transmit that hostility to their pupils, especially young girls for whom elementary school teachers must be powerful role models.

Even those teachers who did have at least some science in college are not likely to be well prepared to teach the subject. Recently, I served on a kind of visiting committee for one of the elite campuses of the University of California, where every student is required to have at least one science course. The job of the committee was to determine how well this requirement was working. We discovered that 90 percent of the students in majors outside science and technology were satisfying the requirement by taking a very popular biology course known informally as "human sexuality." I don't doubt for an instant that the course was valuable and interesting, and may even have tempted the students to do voluntary "hands on" experimentation on their own time (a result we seldom achieve in physics). But I do not think that such a course by itself offers sufficient training in science for a university graduate at the end of the 20th century. These students, some of whom will go on to become educators, are themselves among the discards of the science mining and sorting operation.

In any case, the first step of the operation is what might be called passive sorting, since few elementary school pupils come into personal contact with anyone who has scientific training. Certainly, we all know that many young people decide that science is beyond their understanding long before they have any way of knowing what science is about. Still, a relatively small number of students, usually those who sense instinctively that they have unusual technical or mathematical aptitudes, arrive at the next level of education with their interest in science still intact.

But we too have rescued elitism from the jaws of democracy, in our superior graduate schools.

The selection process becomes more active in high school. There are about 22,000 high schools in the United States, most of which offer at least one course in physics. Physics is my own subject, and I have had some influence on the teaching of physics in American high schools because a remarkably large fraction of them use "The Mechanical Universe," a television teaching project I directed some years ago. Because I have some first-hand knowledge about physics in high schools, I'll stick to that, although I suspect what I have to say applies to other science subjects as well. Anyway, there are just a few thousand trained high school physics teachers in the U.S., far fewer than there are high schools. The majority of courses are taught by people who in college majored in chemistry, biology, mathematics, or, surprisingly often, home economics, a subject that has lost favor in recent years. I know from personal contact that these are marvelous people, often willing to work extraordinarily hard to make themselves better teachers of a subject they never chose for themselves. My greatest satisfaction from making "The Mechanical Universe" comes from the very substantial number of them who have told me that I helped make their careers successful. Their greatest satisfaction comes from—guess what?—discovering those diamonds in the rough that can be sent on to college for cutting and polishing into real physicists.

I don't think I need to explain what happens in college and graduate school, but I'd like to contribute a story of my own because I think it helps to illustrate one of my main points. By far the best course I had in college was not in physics, but rather it was a required writing and literature course known as Freshman English. The professor was my hero, and I was utterly devoted to him. He responded just as you might expect: he tried hard to talk me into quitting science and majoring in English. Nevertheless, the thought of actually doing that never crossed my mind. I knew perfectly well that if I were ever going to make anything of myself, I was going to have to suffer a lot more than I was doing in Freshman English. Physics, I was already sure, would provide the necessary suffering. The story illustrates that we scientists are not the only ones who engage in mining and sorting. But the main point is that for most of us in the academic profession our real job is not education at all; it is vocational training. We are not really satisfied with our handiwork unless it produces professional colleagues. That is one of the characteristics that may have to change in the coming brave new world of postexpansion science.

American education is much maligned, and of course it suffers from severe problems that I need not go into here. Nevertheless, it was remarkably well suited to the exponential expansion era of science. Mass higher education, essentially an American invention, means that we educate nearly everyone, rather poorly. The alternative system, gradually going out of style in Europe these days, is to educate a select few rather well. But we too have rescued elitism from the jaws of democracy, in our superior graduate schools. Our students finally catch up with their European counterparts in about the second year of graduate school (this is true, at least, in physics), after which they are second to none. When, after about 1970, the gleaming gems produced by this assembly line at the end of the mining and sorting operation were no longer in such great demand at home, the humming machinery kept right on going, fed by imported ore.

To those of us who are professors in research universities, those foreign graduate students have, temporarily at least, rescued our way of life. In fact we are justly proud that, in spite of the abysmal state of American education in general, our graduate schools are a beacon unto the nations of the world. The students who come to join us in our research are every bit as bright and eager as the home-grown types they have partially replaced, and they add energy and new ideas to our work. However, there is another way of looking at all this. Graduate students in the sciences are often awarded teaching assistantships, for which they may not be well qualified because their English is imperfect. In general, through teaching or research assistantships or fellowships, they are paid stipends, and their tuitions are either waived or subsidized by the universities. Thus our national and state governments find themselves supporting expensive research universities that often serve undergraduates poorly (partly because of those foreign teaching assistants) and whose principal educational function at the graduate level has become to train PhDs from abroad. Some of these, when they graduate, stay on in America, taking some of those few jobs still available here, and others return to their homelands taking our knowledge and technology with them to our present and future economic competitors. It doesn't take a genius to realize that our state and federal governments are not going to go on forever supporting this playground we professors have created for ourselves.

To most of us professors, of course, science no longer seems like a playground. Recently, Leon Lederman, one of the leaders of American science, published a pamphlet called *Science—The End of the Frontier.* The title is a play on *Science—The Endless Frontier,* the title of the 1940s report by Vannevar Bush that led to the creation of the National Science Foundation and helped launch the golden age described above. Lederman's point is that American science is being stifled by the failure of the government to put enough money into it. I confess to being the anonymous Caltech professor quoted in one of Lederman's sidebars to the effect that my main responsibility is no longer to do science, but rather it's to feed my graduate students' children. Lederman's appeal was not well received in Congress, where it was pointed out that financial support for science is not an entitlement program, nor in the press, where the *Washington Post* had fun speculating about hungry children haunting the halls of Caltech. Nevertheless, the problem Lederman wrote about is very real and very painful to those of us who find that our time, attention, and energy are now consumed by raising funds rather than doing research. Although Lederman would certainly disagree with me, I firmly believe that this problem cannot be solved by more government money. If federal support for basic research were to be doubled (as many are calling for), the result would be merely to tack on a few more years of exponential expansion before we'd find ourselves in exactly the same situation again. Lederman has performed a valuable service in promoting public debate of an issue that has worried me for a long time (the remark he quoted is one I made in 1979), but the issue itself is really just a symptom of the larger fact that the era of exponential expansion has come to an end.

We must find a radically different social structure to organize research and education in science.

The crises that face science are not limited to jobs and research funds. Those are bad enough, but they are just the beginning. Under stress from those problems, other parts of the scientific enterprise have started showing signs of distress. One of the most essential is the matter of honesty and ethical behavior among scientists. The public and the scientific community have both been shocked in recent years by an increasing number of cases of fraud committed by scientists. There is little doubt that the perpetrators in these cases felt themselves under intense pressure to compete for scarce resources, even by cheating if necessary. As the pressure increases, this kind of dishonesty is almost sure to become more common.

Other kinds of dishonesty will also become more common. For example, peer review, one of the crucial pillars of the whole edifice, is in critical danger. Peer review is used by scientific journals to decide which papers to publish, and by granting agencies such as the National Science Foundation to decide what research to support. Journals in most cases and agencies in some cases operate by sending manuscripts or research proposals to referees who are recognized experts on the scientific issues in question, and whose identities will not be revealed to the authors of the papers or proposals. Obviously, good decisions on what research should be supported and what results should be published are crucial to the proper functioning of science.

Peer review is usually quite a good way of identifying valid science. Of course, a referee will occasionally fail to appreciate a truly visionary or revolutionary idea, but by and large, peer review works pretty well so long as scientific validity is the only issue at stake. However, it is not at all suited to arbitrate an intense competition for research funds or for editorial space in prestigious journals. There are many reasons for this, not the least being the fact that the referees have an obvious conflict of interest, since they are themselves competitors for the same resources. It would take impossibly high ethical standards for referees to avoid taking advantage of their privileged anonymity to advance their own interests, but as time goes on, more and more referees have their ethical standards eroded as a consequence of having themselves been victimized by unfair reviews when they were authors. Peer review is thus one among many examples of practices that were well suited to the time of exponential expansion, but that will become increasingly dysfunctional in the difficult future we face.

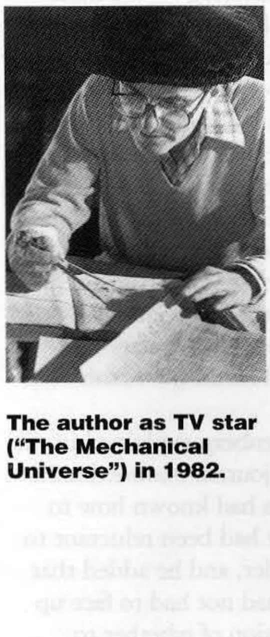
We must find a radically different social structure to organize research and education in science. That is not meant to be an exhortation. It is meant to be simply a statement of a fact known to be true with mathematical certainty, if science is to survive at all. The new structure will come about by evolution rather than design, because, for one thing, neither I nor anyone else has the faintest idea of what it will turn out to be.

And, for another, even if we did know where we are going to end up, we scientists have never been very good at guiding our own destiny. Only this much is sure: the era of exponential expansion will be replaced by an era of constraint. Because it will be unplanned, the transition is likely to be messy and painful for the participants. In fact, it already is. Ignoring the pain for the moment, however, I would like to look ahead and speculate on some conditions that must be met if science is to have a future as well as a past.

Unfortunately, we have never developed a way to bring people along as informed tourists of the vast terrain we have conquered, without training them to become professional explorers.

It seems to me that there are two essential and clearly linked conditions to consider. One is that there must be a broad political consensus that pure research in basic science is a common good that must be supported from the public purse. The second is that the mining and sorting operation I've described must be discarded and replaced by genuine education in science, not just for the scientific elite, but for all the citizens who must form that broad political consensus.

Basic research is a common good for two reasons: it helps to satisfy the human need to understand the universe we inhabit, and it makes new technologies possible. It must be supported from the public purse because it does not yield profits if it is supported privately. Because basic research in science flourishes only when it is fully open to the normal processes of scientific debate and challenge, the results are available to all. That's why it is always more profitable to use someone else's basic research than to support your own. For most people it will also always be easier to let someone else do the research. In other words, not everyone wants to be a scientist. But to fulfill the role of satisfying human curiosity, which means something more than just our own, we scientists must find a way to teach science to nonscientists.



The author as TV star ("The Mechanical Universe") in 1982.

That job may turn out to be impossible. Perhaps professional training is the only possible way to teach science. There was a time long ago when self-taught amateurs could not only make a real contribution to science, but could even become great scientists. Benjamin Franklin and Michael Faraday come to mind immediately. That day is long gone. I get manuscripts in the mail every week (attracted, no doubt, by my fame as a TV star) from amateurs who have made some great discovery that they want me to bring to the attention of the scientific world. But they are always nonsense. The frontiers of science have moved far from the experience of ordinary persons. Unfortunately, we have never developed a way to bring people along as informed tourists of the vast terrain we have conquered, without training them to become professional explorers. If it turns out to be impossible to do that, people may decide that the technological trinkets we send back from the frontier are not enough to justify supporting the cost of the expedition. If that happens, science will not merely stop expanding, it will die.

Tackling in a serious way the as-yet remote task of bringing real education in science to all American students would have at least one enormous advantage: it would give a lot of scientists something worthwhile to do. On the other hand, I'm not so sure that opening our territories to tourism will bring unmixed blessings down upon us. For example, would the scientifically knowledgeable citizens of our Jeffersonian republic think it worth $10 billion of public funds to find out what quarks are made of? I don't know the answer to that question, but I am reasonably sure that a scientifically literate public would not have supported President Reagan's Star Wars program, which in its turn, did help for a while to support at least a small part of my own research. In other words, keeping the tourists away has some advantages that we may have to give up.

Nevertheless, I'm willing to take the gamble if others are. I don't think education is the solution to all our problems, but it does seem like a good place to start. Besides, I really don't know what else we can do. □

David Goodstein, vice provost and professor of physics and applied physics, has been polishing gems at Caltech since 1966. He presented this paper at the 1993 Sigma Xi Forum, Ethics, Values, and the Promise of Science, *in February 1993. It appears in the forum proceedings volume published by Sigma Xi, The Scientific Research Society, P.O. Box 13975, Research Triangle Park, NC 27709, and is reprinted by permission.*

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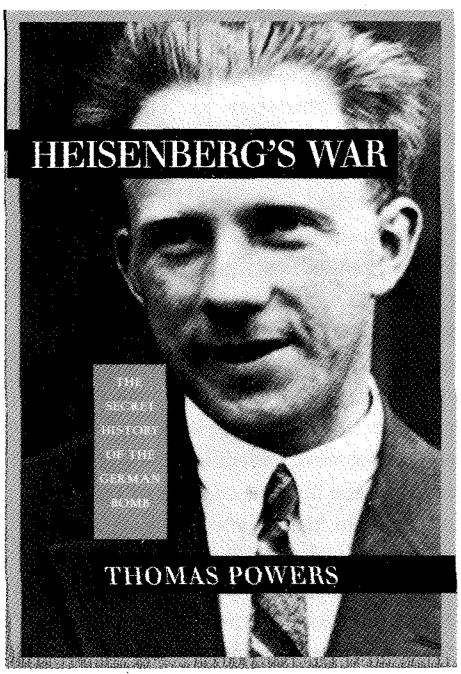

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Heisenberg's Uncertainties

by Daniel J. Kevles

During the Second World War, the nightmare that German physicists would deliver an atomic bomb into Hitler's hands haunted the inner circles of American science. Like most nightmares, this one melded foreboding with facts. Hitler's government controlled rich natural-uranium mines and the world's only plant for manufacturing heavy water, an essential ingredient in nuclear-reactor research. Germany had been a superpower in world physics, a Mecca for American students, its scientists mighty contributors to the recent revolution of quantum mechanics. Despite the loss of many world-class scientists as refugees from Nazism, Germany still appeared formidable. Otto Hahn, who, in 1938, had identified the phenomenon of nuclear fission using tabletop apparatus in his Berlin laboratory, remained in Germany; so did Werner Heisenberg, a theorist of towering talents, who had conceived the famed uncertainty principle and, in 1932, at the age of 31, had won a Nobel Prize for his co-invention of quantum mechanics.

In the mid-1930s, Heisenberg, unimpeachably German but no Nazi himself, had defended Einstein's physics—"Jewish physics," Hitler's minions called it—and had found himself labeled a "white Jew," his career and his life at risk. While he was on a visit to the United States in the summer of 1939, close scientific friends, like the physicist Samuel Goudsmit, had pleaded with him to emigrate, but he had returned home, insisting that he was a German patriot with a duty to help maintain havens of decency in his country and protect German physics for the future. Wartime intelligence reports revealed that his patriotism had deepened to include the hope of a German victory, because it would counter the inroads of Soviets and Slavs threatening from the east. The reports also indicated that the Germans had initiated an atomic project, and that Heisenberg—"the most dangerous possible German in the field because of his brain power," as a distinguished British physicist told American physicists—was involved in it. However, in December of 1944 an American scientific team, sent to Europe to ascertain the state of German nuclear affairs as part of a United States Army intelligence mission code-named Alsos, tentatively concluded that the German atomic-bomb project was paltry and was several years behind the Manhattan Project.

In 1947, Heisenberg explained in the British scientific journal *Nature* that he and his colleagues had known how to make a bomb but had been reluctant to build one for Hitler, and he added that in any case they had not had to face up to the moral decision of whether to

The question of why German physicists did not produce an atomic bomb remains highly charged. It bears upon how we judge any scientist who participated in the Nazi machine or, for that matter, scientists anywhere who for the sake of science or ideology enter into a potentially Faustian bargain with the state.

proceed toward an atomic weapon, because even the military agreed that the task was too large for wartime Germany. They had therefore directed their energies toward making a reactor—which they called an "engine"—to exploit nuclear energy as a source of power to drive ships and electrical generators. Samuel Goudsmit, who headed the Alsos scientific team, promptly responded, in *Life* (and elsewhere), declaring that German physicists *had* tried to build a bomb, that they had failed because of the meddling of Nazi administrators and their own commission of serious technical errors, and that Heisenberg's account disingenuously covered their blunders with a newly minted morality. To Goudsmit, who was Dutch by birth, and whose parents had died in the Holocaust—and to many other Allied physicists—the aim of trying to protect physics did not justify collaborating with the architects of Auschwitz.

The question of why German physicists did not produce an atomic bomb remains highly charged. It bears upon how we judge any scientist who participated in the Nazi machine or, for that matter, scientists anywhere who for the sake of science or ideology enter into a potentially Faustian bargain with the state. Scrutiny of the official German records by several historians has revealed that Heisenberg and his colleagues did in fact understand a good deal about the fundamentals of bomb physics, and that early in the war they raised the prospect not only of a power source but also of "an explosive of unimaginable consequence," as Heisenberg put it in a lecture to a gathering of high German officials in February of 1942.

In *Heisenberg's War: The Secret History of the German Bomb* (Knopf, $27.50), Thomas Powers argues that we can judge Heisenberg only if we can know his intentions, and those the official historical record does not reveal. To ferret them out, Powers has meticulously searched through what he calls "the shadow history of the war," seeking in letters, diaries, recollections, and intelligence files what Heisenberg and his friends "said to each other in the small hours of the night." An accomplished investigative journalist and historian of national security, Powers has exhumed a trove of material and deployed it brilliantly, though somewhat repetitiously, to illuminate the hidden history. His book is provocative and often gripping, and it inventively compels a reconsideration not only of Heisenberg's war but of the relationship to it of several key Manhattan Project scientists, Goudsmit among them.

Powers notes that the German nuclear effort, called the Uranium Club at the time, comprised "an unruly mailing list of competing scientists whose only shared hope was to survive the war." Some in Heisenberg's branch of the club, including Heisenberg himself and his close younger friend Carl Friedrich von Weiszäcker, who was the son of the second-highest official in Hitler's Foreign Office, also wanted to exploit the government's interest in nuclear matters to rescue German physics from Nazi know-nothings. This was a hazardous game, as Heisenberg knew.

Some of Heisenberg's intimates revealed in a trail of leaks intended for the Allies how he was playing the game. Consistent in content, the leaks were exemplifed in a remarkable, unequivocal message that one of Heisenberg's confidants—the theorist Fritz Houtermans, a socialist who had spent time in both Nazi and Soviet prisons and was under suspicion by the Gestapo—had asked a Jewish-refugee physicist named Fritz Reiche to carry by memory to the United States. A contemporary handwritten summary of Reiche's report, which he delivered to a group of physicists in Princeton in March of 1941, reads:

> Reliable colleague who is working at a technical research laboratory asked him to let us know that a large number of German physicists are working intensively on the problem of the uranium bomb under the direction of Heisenberg, that Heisenberg himself tries to delay the work as much as possible, fearing the catastrophic results of a success. But he cannot help fulfilling the orders given to him.

In September of 1941, Heisenberg paid a visit to his mentor and conscience, Niels Bohr, in occupied Denmark, attempting, it seems, to convey that he was at work on a nuclear-reactor project

Theodore von Kármán (soon to arrive at Caltech) took this snapshot of Heisenberg (light suit) in 1927 at the International Physics Conference at Lake Como, Italy. Wolfgang Pauli is the other hatless gentleman at right.

and wanted to keep his research confined to that, but he so fumbled the try that he left Bohr furious, and convinced that he was designing a bomb.

The options for delay were inherent in the details of bomb physics. By late 1941, the Germans, like the Allies, recognized that two types of atomic weapons could be fashioned —one of pure uranium 235 (U-235), the readily fissionable isotope of the element, and the other of plutonium, a newly discovered element that would be produced in the controlled chain reaction of a nuclear reactor from a sister isotope, uranium 238 (U-238). They also knew that U-235 represents less than one percent of natural uranium and cannot be chemically separated from its far more abundant sister. Separation by nonchemical means would be extremely difficult, so obtaining enough pure U-235 to make an explosive—what physicists call a "critical mass"—would require at least several years and untold millions of marks. Powers points out that whenever Heisenberg touted the destructive power of a uranium bomb he also stressed the difficulties, thus discouraging pursuit of a U-235 weapon and encouraging the investment of resources primarily in the creation of a reactor that would produce power and—ultimately, perhaps—element 94, as the Germans called plutonium.

But did he, as his disbelievers insist, emphasize the difficulties because he had committed the key technical blunder of overestimating them? The issue turns on how large the critical mass of pure U-235 would have to be. In several wartime comments, Heisenberg implied that as much as a thousand kilograms would be required—a quantity that would indeed have been impossible to obtain soon enough to affect the outcome of the war. He was heard to make a comparable estimate on August 6, 1945, the day the uranium bomb was dropped on Hiroshima. At the time, Heisenberg and nine other German nuclear physicists, including Hahn and Weiszäcker, were interned at Farm Hall, an estate outside Cambridge, England, where the bedrooms and common rooms were electronically bugged and the conversations routinely recorded and transcribed. The night of August 6, the microphones picked up an unguarded, emotional discussion that started with skepticism that the Americans had succeeded in producing a nuclear bomb, because the Germans did not think that anyone could possibly have obtained enough pure U-235—perhaps two tons, Hahn remarked that Heisenberg had said at one point—to form a critical mass.

Several times during the war, however, Heisenberg had indicated that the critical mass would be only a few tens of kilograms, which was in the right ballpark and was small enough to be considered obtainable; indeed, at a meeting in Berlin in 1942, responding to a question from Field Marshal Erhard Milch, of the Air Force Ministry, Heisenberg had said that London could be leveled with a bomb about as large as a pineapple. The Farm Hall transcripts, which the British kept secret until February of 1992, and which Powers has examined, reveal that on August 6 Hahn recalled Heisenberg's telling him more than once during the war that a uranium bomb could be made with only 50 kilograms of the pure metal. That same night, Heisenberg admitted to Hahn that he had never actually calculated the necessary mass. Eight days later, in a lecture to his colleagues on bomb physics, he led them through the exercise of designing a weapon, showing that it could be done with 16 kilograms of U-235, which was very close to the actual critical mass of the metal. The lecture was stunning in its technical mastery, but also impressive was the fact that Heisenberg had adumbrated part of his analysis in calculations that he had made just two days after Hiroshima. Powers contends that Heisenberg's resolution of the critical-mass problem was so quick that he must have worked out the intricacies of a uranium bomb much earlier, and the Farm Hall

Calculation of the critical mass requires certain essential numbers that characterize the fissioning behavior of U-235. . . . It is clear from the Farm Hall transcripts that Heisenberg had not acquired these numbers experimentally in the course of his wartime research and that no one else had, either.

transcripts, he says, offer strong evidence that Heisenberg "cooked up a plausible method of estimating critical mass which gave an answer in tons, and that he well knew how to make a bomb with far less, but kept the knowledge to himself."

Powers gives too much credit to Heisenberg. Calculation of the critical mass requires certain essential numbers that characterize the fissioning behavior of U-235. These numbers can be determined reliably only by actual measurement. It is clear from the Farm Hall transcripts that Heisenberg had not acquired these numbers experimentally in the course of his wartime research, and that no one else had, either. The news of Hiroshima—that it had been bombed with a uranium device enormous in explosive power yet compact enough to be carried in an airplane—had provided him with a giant hint toward determining the numbers: they had to conform to the reality of the working weapon, and that constraint enabled him to figure out the critical mass in a tour de force of rapid, but advantaged, estimate and deduction. At Farm Hall, Heisenberg explained to Hahn that the reason he had not calculated the critical mass of the isotope precisely was that he had believed that U-235 could not be separated out—which is to say that Heisenberg must have judged obtaining even tens of kilograms of pure U-235 a virtually impossible task. He was right that separation would be costly; the principal American installation for the purpose, at Oak Ridge, Tennessee, was huge. Yet Manhattan Project scientists, a number of whom had in the 1930s built sizable cyclotrons and big laboratories to go with them, had obviously been undaunted by the obstacle, and wartime Germany had been able to provide the immense resources that Wernher von Braun required for his Peenemünde rocket projects. Heisenberg had niether the Big Science temperament nor the experience to envision an industrial-scale separation effort. Physicists in other branches of the Uranium Club did, but he did not throw his prestige behind their ambitions.

Not that he lacked opportunity: he was party to several crucial meetings that high Nazi officials held during the six months starting in December 1941 to evaluate military-research programs for their pertinence to the war effort. Powers, taking an original tack, probes Heisenberg's silences in these colloquies—what he did not say or did not do to advance a bomb project. In all the meetings, Heisenberg accorded no more than brief and casual mention to the alternative route to a bomb—reactor-produced element 94, which did not pose a severe separation problem—nor did he call for a crash program to pursue it. He apparently did not even mention element 94 at a meeting in Berlin on June 4, 1942, where he had the attention of Albert Speer, the boss of the German economy and an enthusiast of big-payoff projects (like von Braun's, for example, for which he would ultimately provide tens of thousands of slave laborers). When Speer asked how much money was needed to press ahead with the nuclear effort, Heisenberg mentioned a figure so ridiculously low that Speer decided—and so informed Hitler—to relegate the project to a low priority. In Powers' view, Heisenberg

In the second row, Moe Berg, the celebrated and cerebral major-league catcher . . . sat with a .32-caliber pistol in his pocket, listening to Heisenberg talk about physics, and resolved to kill him if his remarks indicated that he was seriously at work on an atomic bomb.

managed, without conspiring with friends like Weiszäcker or revealing enough to raise the suspicions of the Gestapo, "to guide the German atomic research effort into a broom closet, where scientists tinkered until the war ended."

Despite the downgrading and the wartime reports of Heisenberg's foot-dragging intentions, the fear that Heisenberg was devoting his mighty brain to the cause of achieving a German nuclear weapon remained undiminished among Manhattan Project personnel, including many of its key scientists and its director, General Leslie R. Groves. The suspicion led the physicists Hans Bethe and Victor Weisskopf, both normally levelheaded, to propose formally, in October of 1942, that Heisenberg be kidnapped in Switzerland, where, as they had learned, he was to lecture later that year. Although the Bethe-Weisskopf initiative was soon rejected—any such move would surely have alienated the neutral Swiss—it eventually helped inspire several operations to deny the German nuclear effort its scientists, and particularly Heisenberg.

Powers devotes substantial space to this campaign against Heisenberg, and his account is chilling. One strategy was to bomb the research institutes that Heisenberg and Hahn directed in Berlin, the objectives to include, as General George C. Marshall learned in an explanatory memorandum from an Army Assistant Chief of Staff for Intelligence, "the killing of scientific personnel employed therein." Another strategy was a revival of the kidnapping idea and then its transmutation into an operation that Bethe and Weisskopf knew nothing about—the assassination of Heisenberg.

Powers tells the story mesmerizingly, having compiled evidence that the scheme was real, that it was fostered by General Groves and the members of the intelligence operations that he established for the Manhattan Project, and that it reached its climax on December 18, 1944, in a lecture hall in Zurich. In the second row, Moe Berg, the celebrated and cerebral major-league catcher, who had finished his career in 1939 with the Boston Red Sox and was now an intelligence operative, sat with a .32-caliber pistol in his pocket, listening to Heisenberg talk about physics, and resolved to kill him if his remarks indicated that he was seriously at work on an atomic bomb. Berg scribbled a note: "As I listen, I am uncertain—see: Heisenberg's uncertainty principle—what to do to H . . . discussing math while Rome burns—if they knew what I'm thinking."

Although Berg obviously did not pull the trigger, Powers holds that the operation that began with Bethe and Weisskopf and eventually put Berg in the Zurich lecture hall contributed to the clouding of Heisenberg's reputation after the war. Goudsmit was complicit in the early kidnapping proposals and, according to Powers, in the assassination scheme itself, having been one of the last of Groves' men to brief Berg, in Paris, before he left for Switzerland. Berg later wrote, in notes about the Paris talks, "Nothing spelled out, but Heisenberg must be rendered *hors de combat.*" Powers implies that not only Goudsmit, but Bethe, Weisskopf, and others were psychologically disposed to reject Heisenberg's account of his passive moral resistance to a German bomb because to

Caltech's Robert A. Millikan (left) and Marie Curie engage in thoughtful conversation, while the dashing Heisenberg (behind them, right) seems to have found something more amusing to discuss. But in Rome in 1931 no one was yet talking about a fission bomb.

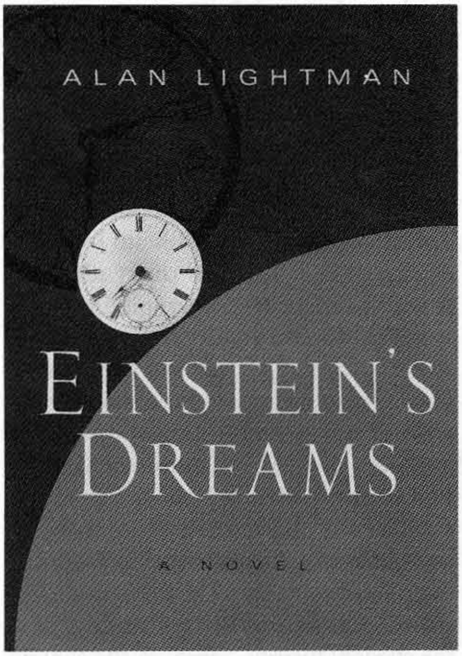
Heisenberg was an unusual man in unusual circumstances, forced to make difficult choices concerning himself, his physics, and his country in a viciously dangerous environment. Even so, it is difficult to accept him as a paragon of moral purpose.

accept it was to indict themselves for their involvements in one or another of the get-Heisenberg projects—and, beyond that, for their own determination to build the world's first atomic bomb.

Here is Powers' evident larger aim: to spotlight Heisenberg as a moral witness against scientists who would forge weapons of mass destruction for their governments. In Powers' judgment, Heisenberg visited Bohr in 1941 primarily with the hope that if he revealed that German scientists were not building an atomic bomb Allied scientists might be persuaded to forgo the construction of one, too. Powers proposes that Heisenberg's postwar claim that moral scruple had figured in his thinking about the bomb could be taken as a rebuke to Manhattan Project scientists for what they had done during the war, noting that if some of them were outraged by the assertion, it was because they were "extremely sensitive to any suggestion that they had done something wrong in building the atomic bomb—especially any suggestion which came from Germans." At the time of Heisenberg's visit, however, Bohr, probably rightly, interpreted his purpose as self-serving patriotism—to stop the Allies from building atomic bombs and dropping them on Germany, a possibility that, in the recollection of Heisenberg's wife, terrified him throughout the war. More important, Heisenberg himself understood that resistance to building bombs for Hitler's totalitarian state could hardly be taken as establishing a moral standard applicable to scientists who devised them for the democratic governments that were Hitler's enemies.

Heisenberg was an unusual man in unusual circumstances, forced to make difficult choices concerning himself, his physics, and his country in a viciously dangerous environment. Even so, it is difficult to accept him as a paragon of moral purpose. Unashamedly eager to use the war to serve German physics, he ingratiated himself with Hitler's henchmen by laying out the requirements for a bomb, thereby obtaining support for nuclear research, his own appointment as director of the Kaiser Wilhelm Institute of Physics in Berlin, and the symbolic reestablishment of modern physics (his as well as Einstein's) in the German scientific hierarchy. He was seemingly tone-deaf to the moral dimensions of politics, uncomprehending of the revulsion that Hitler's domination of Europe stimulated in Bohr and in so many others. Still, while Heisenberg was not a saint, neither was he the devil that Goudsmit saw. The Farm Hall transcripts confirm Powers' reading of the shadow history—that, in the context of Hitler's Germany, Heisenberg and his circle were deeply ambivalent about their nuclear project, that a moral reluctance to see it succeed contributed to its failure, and that Heisenberg himself, as he confessed to his friends on August 6, 1945, was at "the bottom of my heart really glad that it was to be an engine and not a bomb." □

Daniel Kevles, a historian of science and a member of the Caltech faculty since 1964, is the J. O. and Juliette Koepfli Professor of the Humanities. His books include The Physicists: The History of a Scientific Community in Modern America *and* In the Name of Eugenics: Genetics and the Uses of Human Heredity.



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$17.00
179 pages

Einstein's Times

by Jay A. Labinger

First, a little truth-in-advertising. The book jacket calls *Einstein's Dreams* a novel, and notes that author Alan Lightman is a physicist (Caltech MS '73, PhD '74) who teaches physics and writing at MIT. Shouldn't we expect, then, that the book will teach us something about Einstein's contributions to physics? It doesn't. Maybe it's more about Einstein the person? No, not that either. Nor is the book really a novel, in any traditional sense. What is it, then?

The book's basic premise is straightforward: While working on the Special Theory of Relativity, published in 1905, Einstein experiences a series of dreams about time. Each dream portrays an alternate world for which the nature of time is different. The dreams are framed by a prologue and epilogue—in which we see Einstein, early one morning, waiting for the typist to come in and do his completed paper—and are punctuated by several interludes, describing meetings between Einstein and his friend and colleague Besso. According to the prologue, one of the dreams provides the key inspiration: "Out of many possible natures of time, imagined in as many nights, one seems compelling. Not that the others are impossible. The others might exist in other worlds."

Only in some of the 30 worlds does time appear to be *physically* different, and most of those concepts are not unfamiliar—the world where time runs backwards; the world that comes to an end; the world where everything happens over and over again. In other worlds *people* are different—they live forever; they have no memory of the past; they cannot imagine the future. In still others neither the physical world nor its inhabitants seem very different from ours, but people perceive and react to time differently. One world is virtually indistinguishable from the "real" world—time passes more slowly at higher altitudes, but the effect is so tiny that it can only be measured with the most sensitive instruments—nonetheless everyone insists on living in the mountains.

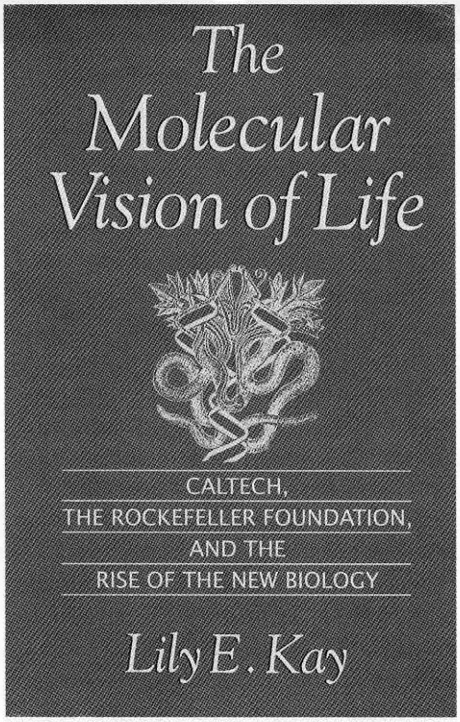
Which is the "compelling" vision that inspires Einstein's theory? None of them—or perhaps all of them. To be sure, some of the dreams tease us with relativistic-sounding concepts. In one, everyone is always moving at high speed, since time thereby passes more slowly (just like our world, with the speed of light reset to somewhere around 55 mph). In another, time depends on relative location, rather than on relative velocity. Gradually, however, as we move from one world to the next, distinctions between the physical, human and perceptual natures of time become less and less important, as do the differences between these alternative worlds and the one we are used to. In the world in which people live for only one day, "either the rate of heartbeats and breathing is speeded up . . . or the rotation of the earth is slowed. . . . Either interpretation is valid." The world of immortals is split into the Laters, who feel no pressure to do anything, since they have infinite time; and the Nows,

who are always busy, since they want to be able to do everything that an infinite life allows. (Does this sound at all like anyone you know?) An understanding of relativity arises not out of any single dream, but from the *global* vision of how time is constituted by interactions between the physical world, its people, and their conception of time.

Even though there may be no overt scientific lesson here, Lightman still provides us much to think about. What is the role of metaphor in scientific discovery? What does the conception of time mean for the novelist? To write a novel, after all, is to construct a world; and consciously or otherwise, the novelist must define the nature of time for that world: Does it proceed linearly or cycle back? Move rapidly or slowly? Smoothly or unevenly? While such issues are not raised explicitly, it is hard to imagine that they did not influence the writing of this book.

One of the dreams can perhaps stand for the entire book: "a world in which there is no time. Only images." Such a world is no world at all—but Lightman makes it a beautiful thing to look at. In like fashion, a book like this can be no novel at all—so don't read it as a novel. Read it as poetry— even though it is not written in any form of verse—for the beautiful writing, the thought-provoking ideas, and above all for the lovely images that arise from the making of the worlds, individually and collectively. □

Jay Labinger is the administrator of Beckman Institute. Also a lecturer in chemistry, he's been a member of the professional staff since 1986 and has written several book reviews for E&S.



Oxford University Press, 1993
$49.95
304 pages

Caltech's Visions?

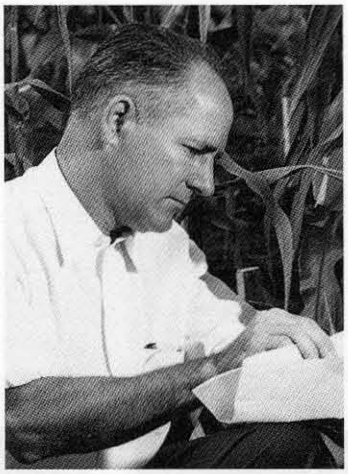
by Robert L. Sinsheimer

This is a curious, disturbing, and ultimately scandalous book—and Caltech and those of us whose research is identified with the Institute are the scandalized. It is startling to be told that, throughout one's entire scientific and academic career, one has been a pawn—worse, an unwitting pawn; and, worse yet, an intellectual progenitor of still more unwitting pawns. But, if Lily Kay is to be believed, that was my life.

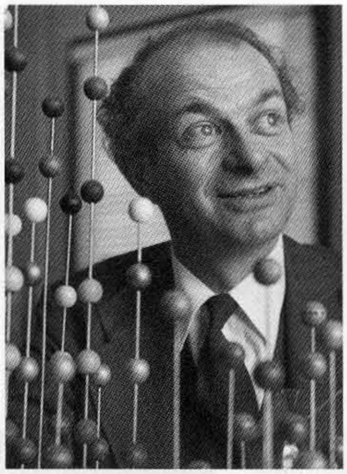
On one level this book presents the history of Caltech's Division of Biology (and therewith the "molecular vision of life") from the mid-1920s to the late 1950s when the division had the generous and sustained support of the Rockefeller Foundation. Arthur Amos Noyes is portrayed as the intellectual father of that vision at Caltech, and it was subsequently implemented by Thomas Hunt Morgan, George Beadle, Linus Pauling (in the Division of Chemistry and Chemical Engineering), and Max Delbrück, to mention only the most famous (all won Nobel Prizes). This history is a work of considerable scholarship and interest. It is densely documented from many sources, including the archives of Caltech and the Philosophical Society, and especially those of the Rockefeller Foundation. Almost every chapter has at least 50 footnotes.

The 1930s through the 1950s (and

Thomas Hunt Morgan



George Beadle



Linus Pauling

Max Delbrück

'60s) were a period of revelation in biology, when the bases for the functional and genetic characteristics of living cells were found to lie in macromolecular structures—in the specific architecture of definable molecules. Many of these advances were made at Caltech. The genetic analyses of Morgan paved the way for Beadle's insightful research, which first linked genes to the performance of specific enzymatic reactions. Pauling's imaginative and painstaking structural studies led finally to the first correct molecular models for proteins and also to the first description of the molecular basis of a genetic disease. And Delbrück's introduction of bacteriophage as a tool for molecular biology research led to a detailed understanding of the genetic role of DNA in replication, mutation, and recombination and in transcription.

Much of this research was indeed made possible by generous support from the Rockefeller Foundation. Kay recounts these advances knowledgeably. Invariably, however, her account of this development of molecular biology is ideologically slanted and hostile, all of it being embedded in, and interwoven with, a subtext—a subtext that purports to reveal a hidden design behind the philanthropy of the Rockefeller Foundation. According to the author, this design was implemented through the skillful guiding hands of its officers Max Mason and Warren Weaver, as they controlled the flow of funds and thereby delicately selected the directions of research. This hidden design was nothing less than the "social control of human behavior," to be achieved through a knowledge of its basic biological origins; the "betterment of man" toward an ideal conceived as the responsible, Protestant-ethic-bound, "Nordic" (Northern European) variety of *homo sapiens*—an ideal reflective of the trustees of the Foundation itself.

Such is Kay's thesis. To be fair, she does not accuse Morgan and the others of being knowing accomplices to the execution of this design. But she does consistently focus her selective vision upon those aspects of their personalities that appear congruent with such a plot—on Morgan's instances of anti-Semitism, on Pauling's small-town-preacher background and his scientific arrogance, on Delbrück's lineage to the German elite and his fostering of a "personality cult," on Beadle's interest in the industrial application of his research. I did not know Morgan, but I knew Beadle, Pauling, and Delbrück well. These men were true scientists, independent thinkers deeply dedicated to the pursuit of knowledge. Each had his personal idiosyncracies, but to suggest that they were somehow manipulated or suborned, their research guilefully co-opted to the hidden designs of the Rockefeller Foundation, I find close to ludicrous.

The author's bias is consistently evident in her choice of language and her persistent (and gratuitous) attribution of motive. For example:

> Graciousness notwithstanding, by retrieving Garrod's "forgotten" work Beadle, of course, was engaging in legitimating his own findings; by setting the record straight he also carved a historical space for his own contributions to biochemical genetics.

> With Pauling's own enthusiatic promotion, in both scientific circles and the popular media, the work [on sickle cell hemoglobin] was regarded as a spectacular achievement . . .

> Robert Sinsheimer at Caltech *rejoiced* in the new powerful technologies . . . (italics mine).

Throughout the book, scientific progress is invariably coupled with a goal of social control. To quote a few examples:

> The program expressed the perception that mechanisms of upward causation were necessary and sufficient explanations of life and the most productive path to biological and social control.

> Equally significant, when the precise

Each had his personal idiosyncrasies, but to suggest that they were somehow manipulated or suborned, their research guilefully co-opted to the hidden designs of the Rockefeller Foundation, I find close to ludicrous.

> mechanisms by which nucleic acids exerted their putative power as the chemical blueprints of life were elucidated, molecular biology would claim greater cognitive authority and technological potential when addressing the unresolved problems of biological deterioration and rational social planning.
>
> Something more profound was at work: a cognitive and social resonance. The Foundation's technocratic vision of social engineering and its representational strategies were articulated on the discursive level of program and policies; the scientist's technocratic vision of life was represented at the bench. The primacy of Caltech on the Rockefeller Foundation's roster reflected these deeply shared interests and convergent social and scientific ideologies.

Kay's implication is clear: Caltech and these particular scientists received the support of the Rockefeller Foundation because its astute officers perceived an underlying "resonance," a shared vision of science and society, that blended the long-range goals of the Foundation, the ethos of the Institute, and the personalities of these faculty. Their science was important but their social perspective was decisive in the Foundation's choices.

That there was a shared vision of science seems likely. That there was a shared vision of social goals is uncertain; if so, knowing these scientists, I cannot believe that it was the program of "social control" or "human betterment" postulated by Kay, although the phrase did apparently find its way into Robert A. Millikan's mouth. But even with the absence of any "written record" linking Pauling to this idea, the author manages to implicate him anyway:

> The synergy between intellectual capital and economic resources buttressed the technocratic vision of progress. With the Foundation's support and the generous help of prominent Pasadena families, Millikan predicted that the Institute could "scarcely fail to win the race for human betterment" through chemical and biochemical advances.
>
> The term "human betterment" must be viewed within a politics of meaning with its own historicity. "The race for human betterment" had a specific linguistic meaning during the 1930s, grounded in eugenic discourse. As the *New York Times* announced, the Rockefeller gift to Caltech was aimed at "the biological improvement of the race." . . . Although there is no written record that during the 1930s Pauling was directly motivated by the social goals of the Rockefeller Foundation's agenda "Science of Man" or by the eugenic campaign of the Human Betterment Foundation, his interests in human applications of biochemical research are documented.

It is not unreasonable for Kay to presume that when its trustees committed the Rockefeller Foundation to "human betterment," they had in mind a world governed by the principles that had led to their personal success—principles of personal responsibility, the work ethic, rationality. And given the evidence that much of human behavior in the world is irrational, it was not without sense at the time to seek biological bases that might explain differences in behavior. To leap from such a relatively benign concept, however, to a

Machiavellian plot, incorporating Caltech and some of the most distinguished scientists of their day and intended to control "human behavior on a global scale," is the stuff of conspiracy buffs.

Accordingly, Kay rejects the thesis that molecular biology was simply the logical outcome of developments in biochemistry, biophysics, and genetics. She writes:

> Current discourse on genetic engineering technologies often characterizes these developments as a natural consequence of the theoretical research that took place during the 1950s, 1960s, and 1970s, a logical evolution from the pure to the applied. The lessons from this book imply the reverse: that from its inception around 1930, the molecular biology program was defined and conceptualized in terms of technological capabilities and social possibilities. Representations of life within the new biology were a priori predicated on interventions that, in turn, aimed from the start at reshaping vital phenomena and social processes.

In one sense, were it not so snide, this view (and indeed the whole book) could be viewed as highly flattering. The very notion that these Caltech scientists could have produced to order such a major scientific breakthrough as molecular biology merely in order to implement the (postulated) social objectives of the Rockefeller Foundation is implicitly a remarkable tribute—although far beyond the possible.

Surprisingly, Kay completely overlooks the historical connection between the conquest of infectious disease by the introduction of antibiotics and vaccines and the increased concern with the residual panoply of genetic diseases. This concern led naturally to a much broader interest in genetics. Instead, she sees only one straight trajectory:

> Molecular biology was mission-oriented basic research. The ends and means of biological engineering were inscribed into the Rockefeller Foundation's molecular biology program, and eugenic goals played a significant role in its design. The program, in turn, formed a key element in the Foundation's new agenda, "Science of Man," a cooperative venture between the natural, medical, and social sciences. This agenda sought to develop a comprehensive science of social control and a rational basis for human engineering.

Thus, she distorts the meaning of statements such as Pauling's in a 1958 broadcast on "The Next Hundred Years":

> Like some of his peers, Pauling saw the deterioration of the human race as the most compelling challenge for the new biology. "It will not be enough just to develop ways of treating the hereditary defects," he said. "We shall have to find some way to purify the pool of human germ plasm so that there will not be so many seriously defective children born. . . .We are going to have to institute birth control, population control."

That "seriously defective" children are born is a human tragedy, and the author's tendency to regard proposals to reduce such tragedy merely as "interventionist concepts of social control," as she does in the next sentence, is simply wrong-headed.

Likewise Kay's perception that emphasis upon the "molecular vision of life" resulted in a diversion of support and interest so that: "important biological problems, such as differentiation, growth, the organization of cells into organs, selection, adaptation, and speciation have remained unsolved for decades." This is also off the mark. On the contrary, these fields are now undergoing dynamic advances thanks specifically to the introduction of the maturing concepts and methods of molecular biology.

It is distressing that such detailed scholarship should have been placed in the service of a distorting, revisionist ideology. Kay clearly belongs to the school of historical determinism that maintains the view that the course of scientific progress cannot be autonomous, but is always a response to cultural, usually political and economic, forces. While this ideology likely has instances of some validity—more so as applied to technology than to science—her attempt to force the development of molecular biology into this mold is misconceived and has led her to an invidious caricature of a great institution and several great scientists. □

Robert Sinsheimer is currently professor emeritus in the Department of Biological Sciences at UC Santa Barbara. He was professor of biophysics at Caltech from 1957 and chairman of the Division of Biology from 1968 until leaving to become chancellor of UC Santa Cruz in 1977. During his 20-year career at Caltech he worked with bacteriophage and was a frequent contributor to E&S *on the ethics of genetic research.*

Random Walk

Benzer Wins Crafoord Prize

Seymour Benzer, the Boswell Professor of Neuroscience, Emeritus, shares this year's Crafoord Prize with William Hamilton of Oxford University. The Crafoord Prize is given annually by the Royal Swedish Academy of Sciences—the same outfit that administers the Nobel Prizes in chemistry and physics—to recognize work done in fields ignored by Alfred Nobel. In Benzer's case, that work was the laying of much of the foundations of modern neurogenetics.

Benzer did so by studying that experimental animal par excellence, *Drosophila melanogaster*, known to the rest of us as the fruit fly. Fruit flies have been beloved of geneticists since Thomas Hunt Morgan for their short life span (less than two weeks), incredible fecundity (hundreds or even thousands of offspring from one pair of fertile flies), low-maintenance lifestyle (they live in glass bottles on a diet of rotting banana mash), and ease of mutability (just zap 'em with a low dose of X rays or a chemical mutagen). To discover the genetic underpinnings of fruit fly behavior, Benzer used the time-honored technique of creating mutant flies, cataloging the mutations, and determining which genes had been mutated.

Benzer's breakthrough was in realizing that an organism as apparently unsophisticated as the fruit fly did, in fact, have a panoply of behavior worth studying. It does, and since the fruit fly is such a simple organism, its behavior is all hard-wired. The fly's brain is much too tiny to actually *think* about such complex things as finding food or a mate, or even about simple things like avoiding a looming shadow that might be a flyswatter, so the fly's responses to its environment have to be built in—genetically determined.

Benzer's group started with fairly simple behavior, even by fly standards. Normal fruit flies fly toward a light source, so the group built an experimental chamber lit on only one side. The group soon found mutants that flew either faster or more slowly than normal flies, and others that flew away from the light instead of toward it. Other experiments explored the genetic drivers of muscular coordination, the 24-hour cycle of sleep and wakefulness, courtship and sex (a remarkably elaborate ritual, even among flies), and even the rudiments of learning (by teaching the flies to avoid a stimulus linked to an electric shock). The trick, of course, was to relate an abnormal behavior pattern to a specific neurological defect, and then to trace that defect to a mutated gene involved in guiding the development of the fly's nervous system.

Benzer's work took on a whole new dimension with the discovery that flies and humans share many genes. Thus the study of these peculiar flies, and the genes that created them, helps illuminate the genetic aspects of neurological development and disease in humans.

Benzer is Caltech's second Crafoord laureate, the first being Jerry Wasserburg, the MacArthur Professor of Geology and Geophysics, who was similarly honored in 1986.

Honors and Awards

Roger Blandford, Tolman Professor of Theoretical Astrophysics, and Ahmed Zewail, Pauling Professor of Chemical Physics, have been elected to the American Academy of Arts and Sciences. Blandford has done research on the properties of black holes, which may power the extraordinarily bright, active nuclei of some galaxies; and on the behavior of pulsars, the small, rapidly spinning remnants of exploded supernovas. Zewail is widely known for developing the new field of femtochemistry—chemistry on the time scale of femtoseconds, or millionths of a billionth of a second.

John Brady, professor of chemical engineering, will receive the Curtis W. McGraw Research Award at the June meeting of the American Society for Engineering Education. The award recognizes outstanding early achievements by a young researcher at an engineering college. Brady specializes in fluid mechanics and transport processes.

Julia Kornfield, assistant professor of chemical engineering, will receive the 1993 Recognition Award for Emerging Scholars from the American Association of University Women. The award is given in recognition of Kornfield's exceptional achievements in chemical engineering to date, and of her promise for future accomplishments. The AAUW is a nationwide grassroots organization of 130,000 college graduates dedicated to promoting equity and education for women and girls.

Professor of Political Science Richard McKelvey was elected in May to the National Academy of Sciences, becoming the first member of Caltech's social science faculty to receive what has long been considered one of the highest honors that can befall a U.S. scientist or engineer.

David Stevenson, professor of planetary science and chair of the Division of Geological and Planetary Sciences, has been elected a fellow of Britain's Royal Society for his studies of the chemistry and physics of planetary interiors, where his studies of brown dwarf stars have developed a link between stellar and planetary physics.

Hugh Taylor, Sharp Professor of Geology, has been named the 1993 recipient of the Geological Society of America's Arthur L. Day Medal, for his "outstanding contribution to geologic knowledge through the application of physics and chemistry to the solution of geologic problems."

Still Boldly Going...

Both Voyager spacecraft appear to have picked up the first sign of the heliopause—the outer limit of our planetary system, where the solar wind no longer blows. The solar wind—actually a stream of ionized particles, or plasma—is continuously boiling off the sun in all directions, forming a bubble of plasma, called the heliosphere, that permeates and surrounds the solar system. The bubble's surface, the heliopause, is that point where the outward pressure of the solar wind is exactly counterbalanced by the flow of the interstellar plasma beyond. Although the heliopause has long been assumed to exist on purely theoretical grounds, there has been much speculation as to its actual location, and thus the dimensions of the solar system.

Not that the Voyagers have actually crossed the heliopause—far from it. Last August, however, they began picking up intense, ultra-low-frequency radio waves. Voyager scientists have now concluded that these waves were generated when strong gusts of solar wind, emitted during particularly violent solar flares last May and June, finally reached the heliopause and slammed into the interstellar plasma. Since the dates of the flares are known, the distance to the heliopause can be calculated by measuring the time it took the solar wind from those flares to reach the heliopause and generate a radio signal that then returned to the spacecraft. The solar wind's speed in the boondocks beyond the Voyagers is unknown, so the scientists, led by Don Gurnett of the University of Iowa, can't place the heliopause more precisely than somewhere between 82 to 130 astronomical units. An astronomical unit (AU) is the average distance between Earth and the sun—93 million miles. By contrast, distant Pluto is roughly 39 AU from the sun. Voyager 1 is 52 AU and Voyager 2 is 40 AU from the sun, so, assuming that the heliopause is actually 100 AU out, Voyager 1 should cross it in about 15 years. Scientists at Caltech's Jet Propulsion Laboratory, which flies both Voyager spacecraft for NASA, have every hope that the Voyagers will still be alive and well and transmitting data when they finally cross that great divide into interstellar space—and even well beyond.

What a Drag!

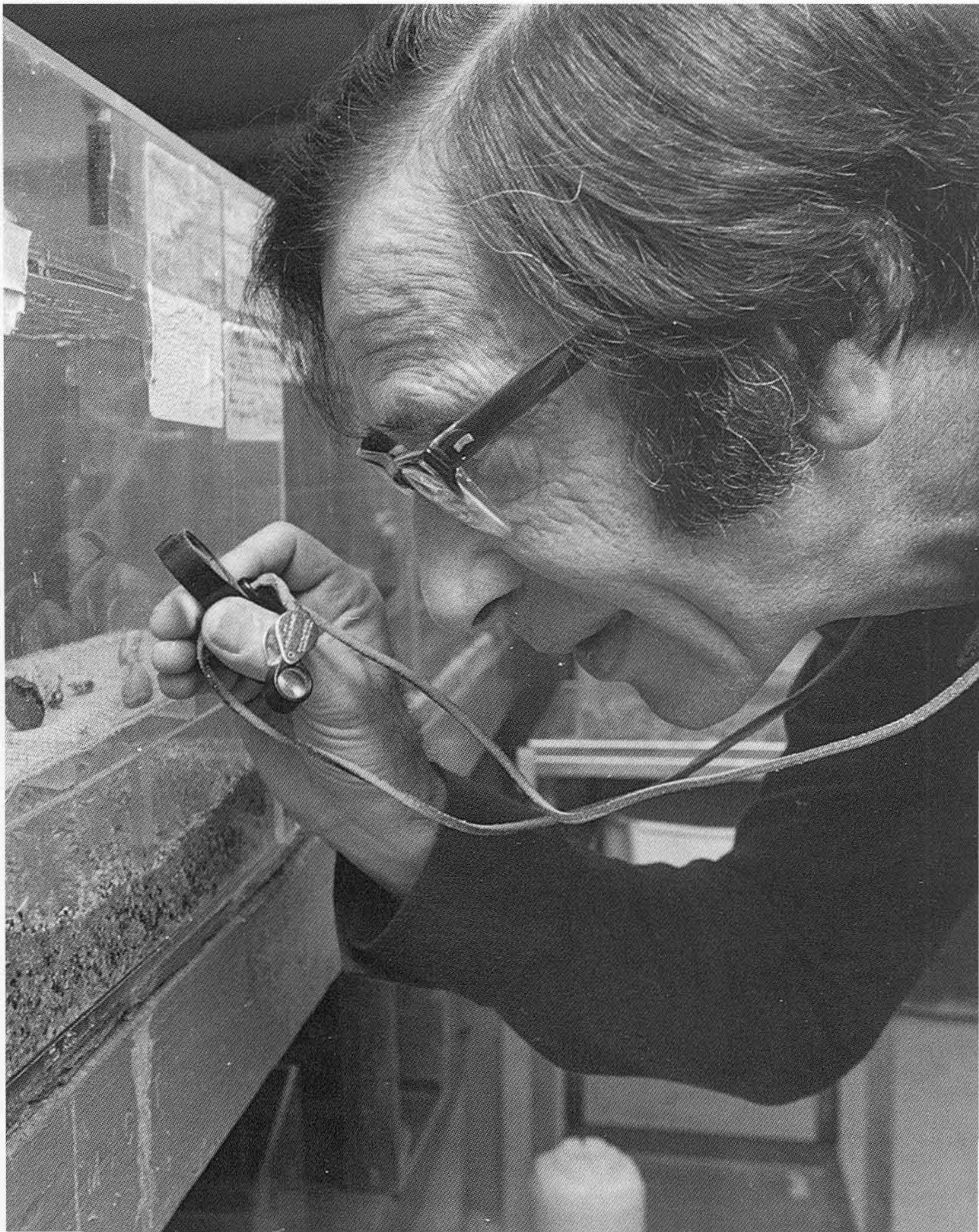
In other news from JPL, the Magellan spacecraft finished its fourth 243-day cycle of mapping cloud-enshrouded Venus on May 25, and fired its maneuvering thrusters to begin dipping the low point of its orbit into Venus's atmosphere. Thus begins the first-ever attempt to "aerobrake" an interplanetary spacecraft—using atmospheric drag to slow the spacecraft down and hence lower its orbit. Magellan's current orbit is egg-shaped, with its high point about 5,300 miles and its low point only 100 miles above the planet's surface. In mid August, when the aerobraking maneuver is complete, the orbit will be a nearly circular 375 by 125 miles. The experience gained may help areobraking become a standard maneuver, enabling future spacecraft to be built without the large thrusters now needed to enter planetary orbits.

Magellan's first three orbital cycles used radar to map 98 percent of Venus's surface, much of it in stereo views. The fourth cycle began the collection of gravitational data. The new orbit will enable Magellan's scientists to make a much more accurate map of Venus's gravitational field, which will in turn tell them what lies beneath the planet's surface. Hotter mantle material is less dense than cooler mantle material, and thus the spacecraft experiences a slightly weaker gravitational tug as it passes overhead. This causes the spacecraft to slow down very slightly, and the resulting Doppler effect creates a measurable difference in the frequency of a continuous radio signal that the spacecraft will be sending to Earth. By correlating Venus's mantle densities to its surface features, project scientists hope to discover what processes are creating the strange landscapes the radar mapper has revealed. Similar measurements of Earth's gravitational field helped uncover the nature of plate tectonics, Earth's fundamental geologic process.

Ricky, Don't Lose That Number

For your comfort and convenience, Caltech's telephone prefix will change from 356 (for the campus) and 397 (for the Beckman Institute) to 395, effective July 1. The four-digit extension numbers will remain the same. Data lines, fax numbers, and private phones not having the 356 or 397 prefix will not be affected. Besides consolidating Caltech and the Beckman Institute into one prefix, this change will provide enough new phone numbers to accommodate future growth.

Heinz Lowenstam, professor of paleoecology, emeritus, died June 7 at the age of 80. Lowenstam's observation that a sea creature, the chiton, could manufacture its own magnetite ("The Case of the Iron Teeth," *E&S*, June 1964) startled both biologists and geologists and wasn't generally accepted until evidence of magnetic bacteria confirmed it in 1975. Since then magnetite has been found in organisms ranging from bees to fish to man, and Lowenstam has become known as the father of biomineralization. Lowenstam came to the Caltech faculty in 1952 from the University of Chicago. Educated in Germany, he wrote his PhD dissertation in the 1930s on the geology of Palestine. An account of his experiences there was published in *E&S* in fall 1990.



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