California Institute
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Engineering & Science

In this issue

We're all in this together: This homeowner wasn't earthquake-prepared, and was out of town when the Northridge earthquake struck. The next-door neighbors, whose house is just out of frame to the left, were more conscientious. They had done all the right things, and they made it through the quake just fine. One can only speculate on their frame of mind during the frequent large aftershocks of the next few days, however, with this unsecured chimney poised to come crashing down on their undamaged house.

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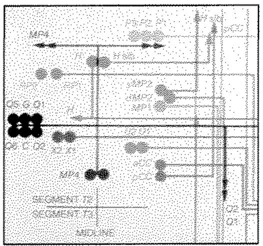

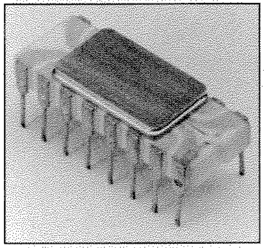

On the cover: In this cross section of a grasshopper embryo, the brown ladderlike structure is the central nervous system. The upper segment has developed normally, while the lower one has been injected with a strand of DNA that knocks out a master control gene. As a result, cells that should become glial cells have turned into surplus nerve cells whose bodies bulge, tumorlike, from the cluster in the segment's center. For more on how to wire up a simple nervous system, see page 2.

Departments

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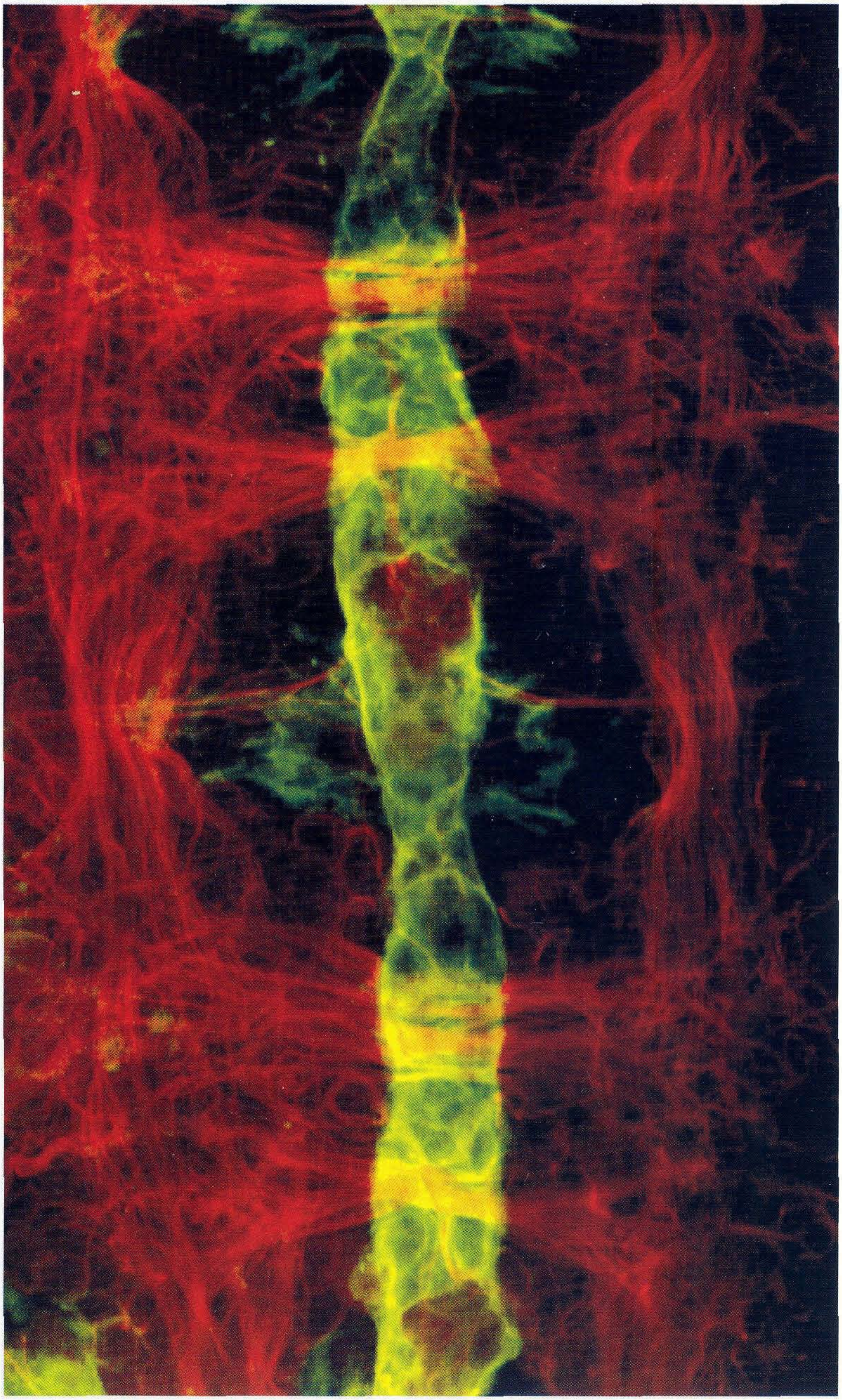


Building an Insect Brain

by Kai Zinn

A single nerve cell can have 10,000 to 100,000 input and output connections.

The fluorescent red wires in this electrician's nightmare is the central nervous system in a single segment of a grasshopper embryo. The two vertical trunks are called the longitudinal connectives, and they run the length of the embryo. The horizontal interconnections are called commissures, and there are two per segment. (This photo actually shows one-and-a-half segments.) Each red line is a bundle of 10 or so axons—the wires of the nervous system——and every connective and commissure has 15 to 20 bundles. The yellow-green band down the center is the midline glial cells that wrap up the neurons' cell bodies and processes.

Neuroscience is the study of the structure and function of the brain, and of the other parts of the nervous system that it controls. There are two basic questions that neurobiologists study: how does the brain work? and, how is it put together during development? These two questions are closely related, since the brain is like a giant electrical circuit and the structure of that circuit partially determines how it works.

The circuit's building blocks are nerve cells, or neurons, and they have certain characteristics in common. Every nerve cell has a nucleus, which contains the genetic material, surrounded by a cell body in which the proteins that make up the cell's machinery are produced. Extending out from the cell body are branching processes—wirelike growths that make connections with other cells. Most neurons have an array of input processes, called dendrites, which receive signals from other cells, and an output process called an axon, which sends signals to other cells. The actual connections between cells are made at junctions called synapses, and each process usually contains many synapses. A single nerve cell can have 10,000 to 100,000 input and output connections.

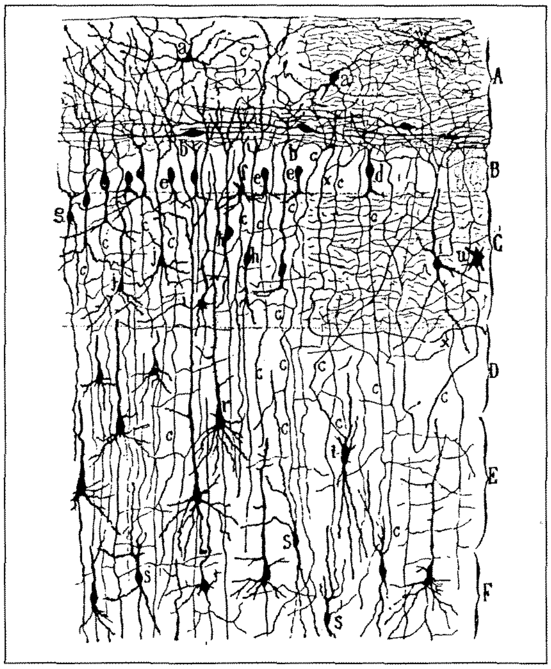
There are thousands of different kinds of nerve cells, each with a distinctive shape. These shapes reflect the shape and organization of the nerve cell's processes, which are tailored to its specific functions. For instance, a retinal bipolar cell's dendrites receive input from the eye's photoreceptors, and its relatively short axon sends output to cells that form the optic nerve. A motor neuron's cell body resides in the spinal cord, and its dendrites receive direct inputs from brain neurons in the form of motor commands. The neuron transmits these commands via an axon that extends all the way from the spinal cord to a muscle in, for example, the forearm.

The structure of a nerve cell's processes is largely determined by the pattern of genes activated, or expressed, within it and within the target cells that it connects to. There are probably about 100,000 genes in the human genetic blueprint, but only a fraction of them are active in any given neuron at a particular time. An initial pattern of gene expression is built into each neuron at its birth, but the chemical and electrical inputs that the cell receives change this pattern. Thus, there is a synergy between gene expression and communication among nerve cells. Gene expression can determine the initial connections between cells. Feedback through these connections then regulates gene expression, which in turn modifies the connections.

A nerve cell does not suddenly spring into being with a full-blown array of processes. Rather, it begins life as a cell body from which the processes must grow out toward their eventual targets. The leading edge of each process is a specialized structure called a growth cone, which navigates through the surrounding tissue to the target. Every process has a mission—a set of cells it is driven to seek out and connect to.

My research group's goal is to identify and understand the functions of the genes that control the shape of a neuron and the connections it makes to other cells. Which genes have to be turned on for a cell to have a certain shape, and

This cross section of a rabbit cortex was drawn by the Spanish neuroanatomist Santiago Ramón y Cajal, who shared the Nobel Prize for physiology or medicine with Camillo Golgi in 1906. *Golgi invented a method of staining individual nerve cells so that they could be easily seen under a microscope.* Ramón y Cajal adapted the method to the brain, and established that the neuron was the fundamental unit of the nervous system.



how do they control the cell's development and growth? We try to answer these questions not by studying the human brain, which is far too complicated, but by looking at the nervous systems of insects, which are much simpler. Some circuits in the insect equivalent of the spinal cord have only a few hundred neurons, and the way in which they connect to one another is completely controlled by genetics. These circuits could be described as hard-wired. The human brain, in contrast, has a vast number of neurons, and most of its connections are not hard-wired.

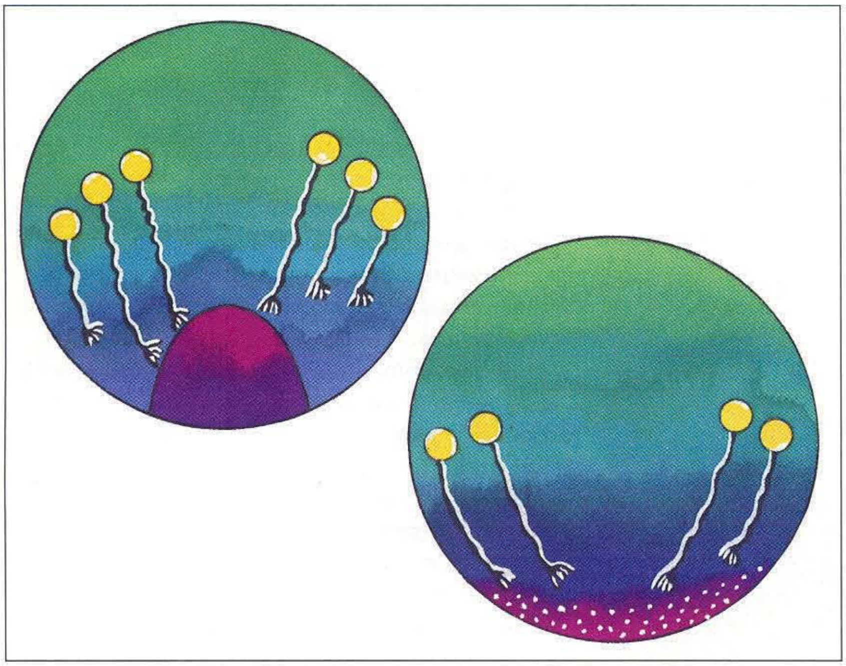
You can actually hand-dissect a grasshopper or fruit-fly embryo, if you don't drink too much coffee that morning.

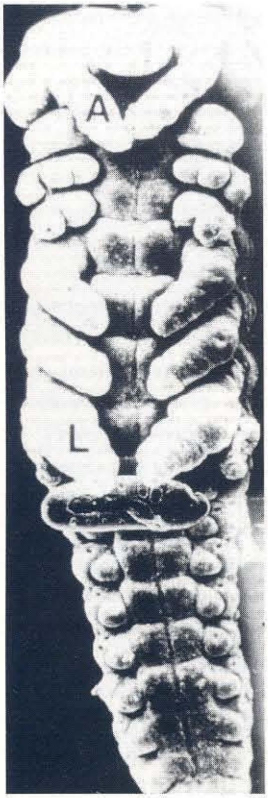
Most of the outer surface of the human brain is composed of a deeply folded, layered sheet called the cortex. Underneath this sheet is a massive amount of wiring—processes like those I mentioned above—that connects the different parts of the brain to each other. Above is a view of a cortex's six layers and a few of the cells within those layers. (The cells are actually much more densely packed.) If you were to unfold it and spread it out, the human cortex would be about the size of a large pizza-box lid. The volume underneath one square millimeter of cortical area contains about 60,000 neurons, about four and a half kilometers of wiring, and some 600 million to 2.5 billion individual synapses. The entire cortex has perhaps 720,000 kilometers of wiring, which is very nearly enough to stretch from the earth to the moon and back again, at least 10 billion neurons, and perhaps 10^{15} synapses. These numbers are so large that it's hard to even begin to think about how to devise a plan to study what directs them to form.

The problem of understanding how this structure gets built, and where its blueprint information comes from, is further complicated by the fact that it's not hard-wired, as I mentioned earlier. The detailed pattern of connections is different in every human—that's part of what makes you a unique individual, with your own memories and skills—in fact, it's different in every individual mammal. Thus, we can't always extrapolate from one brain to another. The patterns vary because the connections in the mammalian brain at birth are very different from what they will be in the mature animal. As the baby mammal interacts with its environment, these acts of exploration essentially rewire its nervous system. A human infant, for example, can't make coordinated motions, can't focus its eyes, and doesn't know what it's seeing. It can't really interpret the world around it. A five-year-old child, however, can do complex physical tasks, such as assembling Lego blocks into an intergalactic battle cruiser, can communicate what it sees and feels, and basically has a mature pattern of connections within its brain.

There's experimental evidence for this rewiring. David Hubel, Torsten Wiesel, and their colleagues at Harvard Medical School have shown that if a monkey or a cat is kept from seeing out of one eye during a critical period following birth, the animal will never be able to see out of that eye again. The researchers placed an opaque patch over the eye—or even a translucent one that admitted light but blurred out forms—and then removed the patch after a couple of weeks. Brain-activity measurements showed that no inputs from the just-uncovered eye were reaching the brain, even though the eye was perfectly functional. This is because the newborn starts out with an unorganized pattern of connections between the eyes and the brain. As the animal looks around, feedback from the brain tells the nerve cells in the eyes which connections are providing useful information and should be maintained, and which aren't and should be changed or eliminated. If one eye is covered, the animal receives useful information solely from the other eye, whose connections proceed to take over the entire part of the brain that should be driven by both eyes. The connections get locked in during the first several months of an animal's life, and the displaced eye can never again provide input.

Given such dependence on the outside environment, why should we think that anything we learn about insect brains would be relevant to understanding the human brain? A variety of experiments over the last 10 years have shown that many of the molecules that are used to wire





Below: A scanning electron micrograph of a grasshopper embryo, magnified some 25 times. Its antennae are labeled "A," and its legs are labeled "L." The handbag it's clutching in its lowest set of legs is actually a fruit-fly embryo.

the nervous system are the same in all organisms, or largely the same. The existence of these so-called "chemoaffinity molecules" was proposed by the late Roger Sperry, Caltech's Board of Trustees Professor of Psychobiology, Emeritus, who won the Nobel Prize together with Hubel and Wiesel. (Sperry studied optic-nerve regeneration in fish. {For more on Sperry's theory of chemoaffinity, see the memorial on page 31 of this issue.]) One example of these omnipresent chemoaffinity molecules is shown above. The upper drawing is a cross section of the spinal cord of a vertebrate embryo. A set of cells known as commissural neurons (yellow) grows processes down toward a structure called the floor plate (red), which lies along the midline of the spinal cord's ventral, or belly, side. The growing processes turn at the floor plate and extend along the length of the spinal cord to form their final connections. A molecule made in the floor plate tells these processes which way to grow. The lower drawing is a cross section through an embryonic soil roundworm called a nematode, which has about 300 nerve cells in its whole body. Neurons in the body wall similarly extend their axons down to the ventral midline (also in red), and they then turn and grow along the axis of the body. The floor-plate molecule was recently discovered by Marc Tessier-Lavigne and his colleagues at UC San Francisco, and it turns out that essentially the same molecule is found in the roundworm. In fact, you can take the molecule from a nematode and put it into a spinal cord culture, and the neurons will do the same things that they would do if the vertebrate molecule were there.

Such discoveries furnished the motivation behind our work. We presume that in a genetically hard-wired circuit, a set of molecules instructs the neurons to form the correct connections. In a system such as the mammalian brain, interactions with the environment may redeploy these same molecules (or others like them) and use them in new ways to rewire the system based on what the animal experiences. If we study a nervous system that is not extensively rewired, we may be able to figure out in detail the genetic rules that control the construction of its circuit. These rules may also prove applicable to the experience-driven rewiring of the mammalian brain.

My research group works with grasshopper and fruit-fly embryos. The two organisms have very similar central nervous systems, and many of the individual connections between nerve cells are the same in both. Back in the early 1980s, Corey Goodman's laboratory (then at Stanford, now at UC Berkeley) performed many of the classic experiments that defined the broad rules for the assembly of the organisms' central nervous systems, and showed that their circuits are highly conserved between insect species. Each kind of embryo has particular advantages. A grasshopper embryo measures several millimeters long compared to a fly's one millimeter, and grasshopper cells are much larger and easier to work with, but fly genetics have been studied in great detail.

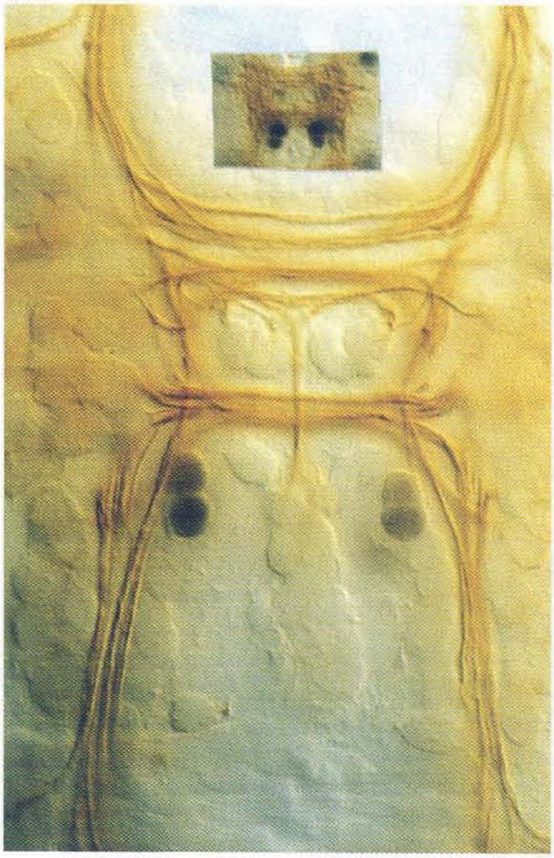
There are several techniques for making the neural hardware and its wiring patterns clearly visible. We can inject individual cells with fluorescent dyes, and track where their processes go. (You can actually hand-dissect a grasshopper or fruit-fly embryo, if you don't drink too much coffee that morning.) We also use antibodies that recognize specific neuronal structures and bind to them, staining them an easily visible brown. We can even tag different antibodies with assorted brightly colored tails, so that we can see several different structures at once. Once we've used one of these methods, we flatten the segment out and photograph it under a microscope at a magnification of about 500. We use special types of microscopes, such as one called a confocal microscope, to focus on a very thin layer of tissue. Ordinary microscopes collect light reflected from the entire thickness of the sample, so the details in any particular layer are blurred by the layers above and below it. A confocal microscope, however, has a pinhole above the sample through which the reflected light must pass. The pinhole's distance from the sample determines the layer whose reflected light is allowed to pass. The pinhole is scanned across the sample, and the transmitted light is recorded by a CCD camera.

In the fly, the wiring is compressed into a much smaller area, so it's more difficult to distinguish individual axon bundles—individual wires, as it were. However, fruit-fly genetics have been intensively studied for 75 years, so we have a tremendous amount of information about the contents of the fly's genetic blueprints. We can create mutants in particular genes and see how those mutations affect the structure of the nervous system. That is, when we find a gene we think does something important, we can "knock out" the gene—mutating it to render it nonfunctional. If something abnormal happens as a consequence, this provides clues to what the gene does in a normal animal. (These kinds of studies on the structure and function of the fly nervous system were pioneered at Caltech in the early 1970s by Seymour Benzer, Boswell Professor of Neuroscience, Emeritus, and his colleagues.)

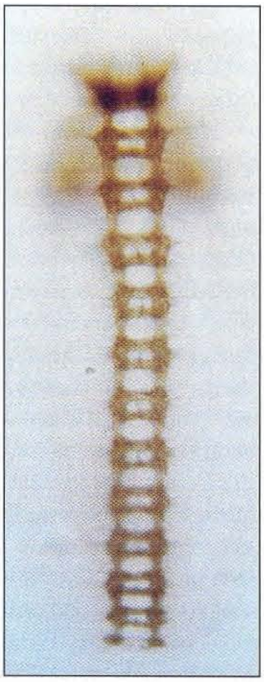
Both species' embryos are divided up into about 15 segments that, at early stages of development, are very similar to one another. There are subtle differences in the details of the neural wiring from segment to segment within an embryo, but the wiring in any segment in one embryo is identical to that in the corresponding segment in any other embryo at the same stage of development. Thus our experiments are reproducible, which is often not possible with higher organisms. Since the basic circuit is almost identical in each segment, if we can understand one set of blueprints, we will understand them all.

Each segment contains a section of the central nervous system and a set of peripheral nerves. The entire central nervous system (left) looks like a ladder. The longitudinal connectives—the sides of the ladder—extend the length of the embryo. The rungs are called commissures, and they are bundles of axons that cross from one connective to the other. (The first few rungs become the brain.) Each segment contains two commissures. Like the mammalian spinal cord, the insect nerve cord contains motor neurons that send axons to specific muscles in the body wall. These motor connections are identical in every embryo, as I mentioned above, and their development has been studied in detail by many people. The embryonic peripheral nervous system consists of sensory neurons, whose shape and arrangement are also invariant. The sensory neurons carry input from the environment. Their cell bodies lie near sensory organs, and they extend processes to the nerve cord and make specific connections. When the embryo hatches, these neurons will receive pressure and chemosensory input that will tell the larva such things as which

The ladder of bundles defines the axons' possible routes in the way that a grid of city streets defines municipal bus routes. And, like a downtown bus route, an axon may follow one street for a while, then turn at an intersection onto a cross street, and so on until it reaches its destination.

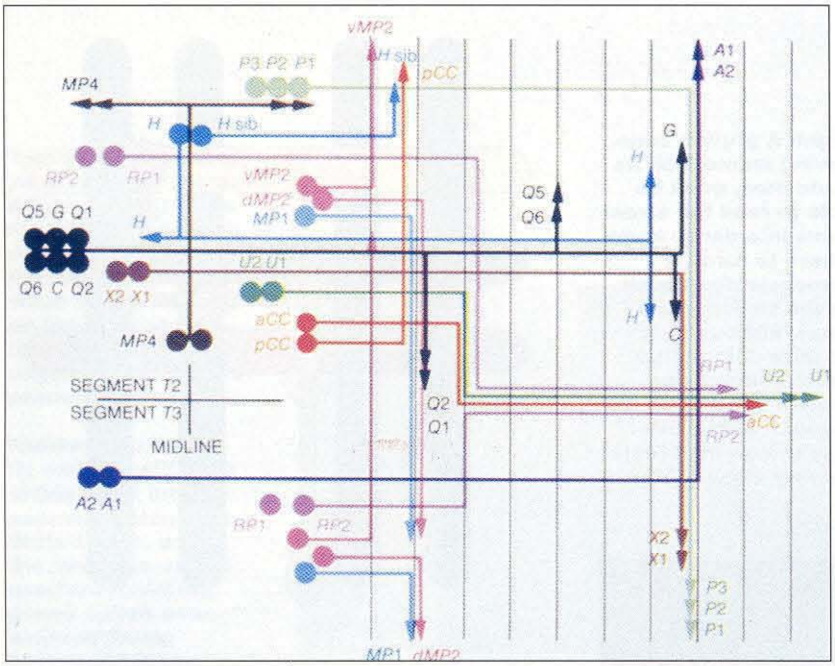


Above: Identical segments of a grasshopper and a fly embryo (inset), identically prepared, identically stained, and shown to the identical scale. The axons, stained brown, clearly reveal the double-runged ladder pattern. Since the embryo is symmetrical around its midline, each cell on the left has a twin on the right. The black-stained cell bodies are the neurons named aCC (top cell in each pair) and pCC (bottom cell in each pair) in the diagram on the opposite page.



Left: A whole fly embryo's central nervous system. The developing brain is the out-of-focus blurs at the top and to either side of the ladder. The double-runged ladder is the insect equivalent of the vertebrate spinal cord. Some of the motor neurons that lead away from it are also visible.

Reprinted with permission from Goodman, et al., Cell Recognition During Neuronal Development, *Science*, Volume 225, pp. 1271–1279, 1984. Copyright 1984 by the American Association for the Advancement of Science.



Left: A partial wiring diagram of a segment. The circles are cell bodies; the arrows are axons. The 10 vertical lines on the diagram's right-hand side are individual axon bundles in the longitudinal connective, and the bundle going off horizontally to the right is the intersegmental nerve. Cells of the same color come from a common ancestor. Neurons aCC and pCC, stained black on the opposite page, are colored red here.

Above: The brown, blotty, spiky thing in the top center of this photo is the growth cone of an axon on the move, sniffing its way through the jungle of tissue cells in search of its target. The neuron's cell body is about 14 inches away from the growth cone at this scale, down somewhere to the lower right.

side is up, and where to find food. Much of the work on the development of the fly peripheral nervous system has been done in the laboratory of Yuh Nung Jan (MS '70, PhD '75) and Lily Jan (MS '70, PhD '74), at UC San Francisco.

Within each commissure and connective there are 15 or 20 different axon bundles, each of which might contain 10 axons. The ladder of bundles defines the axons' possible routes in the way that a grid of city streets defines municipal bus routes. And, like a downtown bus route, an axon may follow one street for a while, then turn at an intersection onto a cross street, and so on until it reaches its destination.

Part of the wiring diagram for a segment at an early stage of development is shown above. At this point, each segment has about 250 nerve cells, each of which can be individually identified. So, for instance, in every grasshopper embryo you examine, you'll find sister cells called aCC and pCC. The aCC cell always sends its axon out along the intersegmental nerve and on to a dorsal muscle, while pCC's axon takes a different route along a longitudinal connective. Cell aCC is a motor neuron, while pCC is an interneuron—a type of cell that makes connections only between other neurons. Thus, even though they came from the same parent cell, the two neurons end up doing very different things.

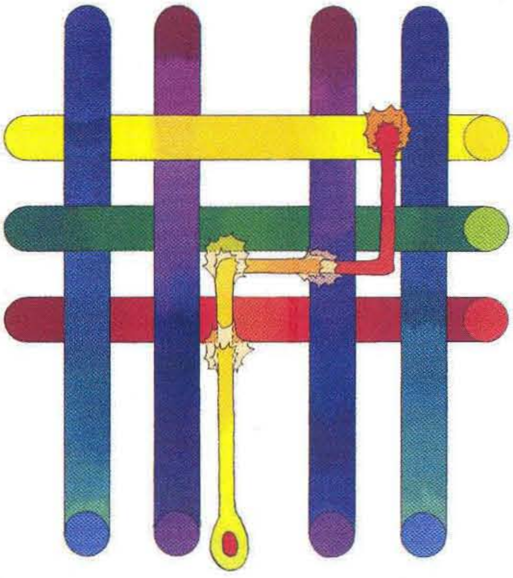
Every cell within the array knows its identity, and which bundle it should extend its axon along. The axon's growth cone (at left) senses the environment and determines where the process should go. The growth cone probably recognizes markers on the surfaces of the bundles it encounters that tell it "this bundle is different from that bundle, and this is where I'm supposed to turn." This is called the labeled-pathways hypothesis. So in the example above, the one cell is expressing the pattern of genes that makes it cell aCC, and its growth cone is looking for a cue that says "aCC turn here." This cue will allow it to follow the correct pathway.

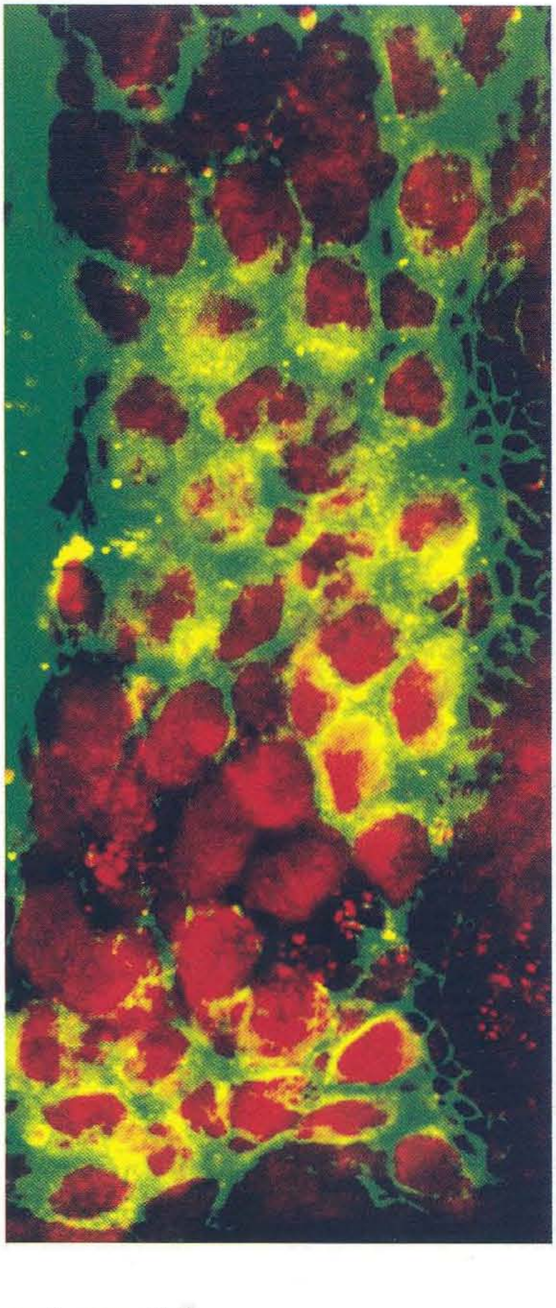
Experiments done in Goodman's laboratory, in which the growth cone's target was removed, have provided evidence for such labeled pathways. Consider a neuron that's supposed to find a pathway defined by the axon of another neuron. If you kill the second neuron with a laser, the first neuron's growth cone will grow to where the pathway should be, and then stop or wander randomly. It can't continue on its prescribed route, because its pathway is missing.

We'd like to understand how the cell makes the sequence of decisions that ultimately sends an axon along a predetermined route to make a particular connection. We could ask two basic questions about this process. First, how does a cell know it's supposed to be a nerve cell at all? Why didn't it become something else? What molecules made it choose a career as a neuron? Second, how does a nerve cell choose the route along which it should extend its growth cone? This question has three parts: the initial decision to choose a particular pathway; subsequent decisions to turn off that pathway onto others; and finally, the decision to stop when the target is reached. For instance, in the watercolor sketch on the next page, we can ask what molecular information tells the axon to go straight across

The growth cone has to communicate back to the cell body and say, "This is the wrong pathway," or "We're up here, and we should make a turn to the left."

Right: A growth cone, having memorized its route map, must be able to read the street signs in order to know where to turn. It somehow distingushes the various pathways, although not by their color. This sketch shows the growth cone interacting with each pathway in turn as it feels its way along.





Right: The right-hand half of the sheet of neuron-progenitor cells in a grasshopper embryo segment. Neuroblast 1-1 is the red cell in the upper left corner. This is a double-stained micrograph—the red-flourescing antibody binds to neurons, while the green one binds to mesectoderm cells, which define the midline. Where both markers bind to the same cell, their superposition comes out yellow, marking the sheath glial cells that surround the neuroblasts. The green strip along the photo's left edge is the segment's midline.

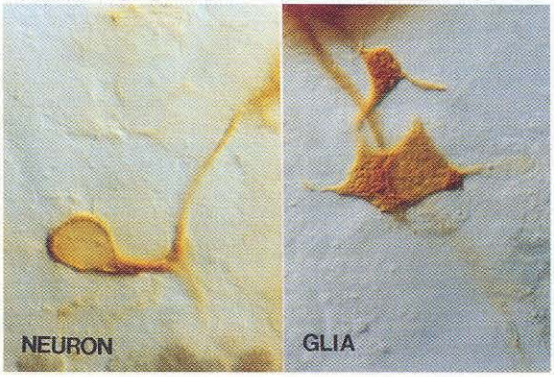
the red pathway, then turn right on the green pathway, and finally turn left on the blue pathway and make a connection near the yellow pathway. As mentioned above, these decision mechanisms involve surface contact between the growth cone and the pathways. They also involve signaling events, because the growth cone has to communicate back to the cell body and say, "This is the wrong pathway," or "We're up here, and we should make a turn to the left." The cell body then has to make new proteins that will cause the growth cone to change its direction of growth.

To consider the first question, of neuronal career choice, we have to understand how the cells that will give rise to neurons are organized. The neural zone of an insect embryo starts out as a gridlike sheet of neuron-progenitor cells called neuroblasts, which we identify by their row and column number. The biography and genealogy of each of these cells is genetically predetermined, and has been exhaustively studied on the cellular level. We know what each cell, and its descendants, is going to do before it does. For example, one red cell in the illustration at left is neuroblast 1-1. This will divide to produce a set of daughter cells, and the first daughter cell will in turn divide to produce the aCC and pCC neurons. The other neuroblasts produce other neurons that also have their own unique identities.

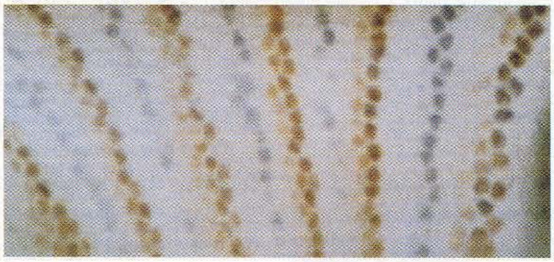
Some neuroblasts also generate glial cells, which are support cells that wrap up an axon and insulate it. Nerve activity is fundamentally an electrical phenomenon, and without the glial cells for insulation, the neurons wouldn't be able to function. The two kinds of cells are easy to distinguish—neurons are round and have processes, and glial cells are irregularly shaped.

We want to discover what information instructs a particular neuroblast to generate the specific set of daughter cells that it produces. But, as I have just shown, that sequence of events actually encompasses several decisions. We have studied the molecular basis for one of them: how does a neuroblast daughter decide whether to become a neuron or a glial cell? Two of the molecules we've looked at that may be involved in cell-fate decisions are called the engrailed and even-skipped proteins. (The term "engrailed" comes from heraldry, and refers to a wavy line that resembles a row of fish scales. It looks a bit like the normal pattern of bristles on a fly's wing—a pattern that changes in the mutant.) The engrailed and even-skipped proteins bind to DNA, the genetic material, and switch genes on or off. Gene regulation is like a pyramid. At the pyramid's apex are the master regulators, which control a battery of secondary regulators, which control the final products at the pyramid's base. That is, some genes cause the synthesis of protein molecules that control the expression of other genes, which then cause the synthesis of the protein molecules that actually make up the cell. So once a developing cell makes the decision to switch on a master regulator, that decision determines everything that happens subsequently. The engrailed and even-skipped proteins are master regulators, and are found in both insects and humans. In the early fruit-fly embryo, these two molecules are expressed in alternating stripes, and they determine segmentation. There is one engrailed stripe per segment. Later on, these stripes control the identities of the neuroblasts that arise from them. So, for instance, neuroblast 1-2, which makes the engrailed protein, will give rise to a different set of cells than will 1-1, which does not. Similarly, the first daughter of 1-1 (but not of 1-2) makes the even-skipped protein, and this may control the identities of the cells that it produces.

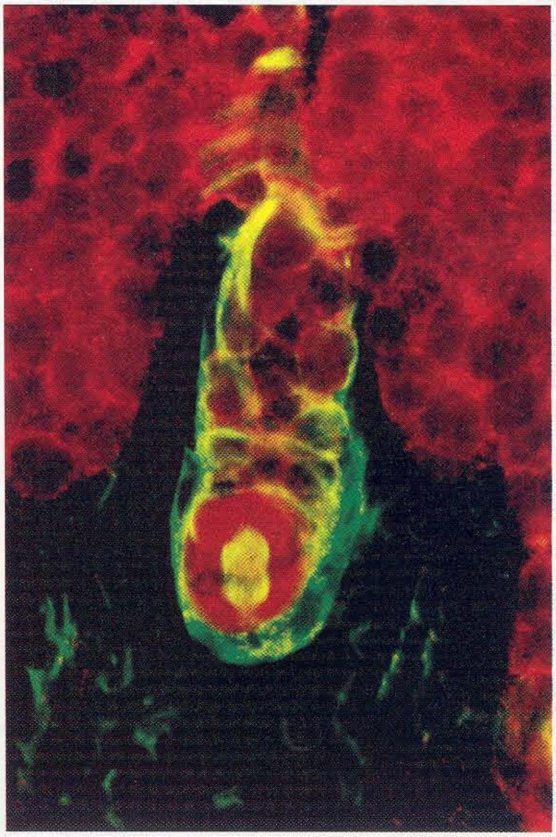
One lineage that Barry Condron, a postdoctoral fellow in my lab, has studied in detail is shown at left. The sequence begins with the MNB, or median neuroblast, an unpaired cell at the segment's midline. It gives rise to a certain set of neurons, which, along with the axons they produce, are labeled red. In addition, it generates all those green cells, which are glial cells that wrap up the bundle of red axons. Thus, the MNB is multipotent—it can generate both neurons and glia. Since the cells in the MNB lineage produce the master-regulator engrailed protein, we can ask if its presence affects whether



Top: By their shapes ye shall know them: a neuron (left) has a round cell body with a long process extending from it, while a glial cell has an irregular shape—this one has scalloped edges and a bulbous process on top.



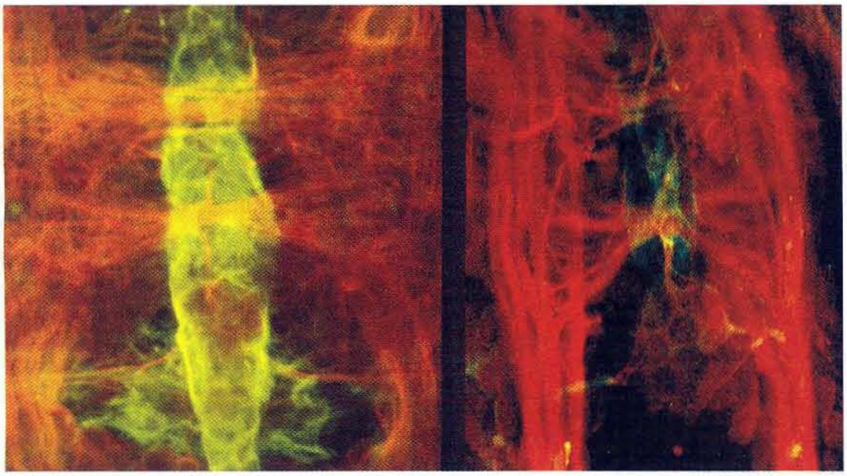
Middle: This fruit-fly embryo will soon divide itself into segments along the dotted lines, under the direction of two master-regulatory genes called *even-skipped* (being expressed in the brown-stained cells) and *engrailed* (in the black-stained cells).



Bottom: The red cell at the tip of this column of cells is called the MNB, or median neuroblast, and it has been caught in the act of dividing. (The MNB's yellow filling is its chromosomes moving apart.) Previous divisions of the MNB gave rise to the pillar of red nerve cells as well as the green glial cells surrounding them. The other yellow regions are a superposition effect.

Below: This posterior midline glial cell has been nicknamed "Batman" for its winglike processes. To see why, turn the magazine upside down.

Opposite, A: The now-familiar central-nervous-system segment, stained black with an antibody that recognizes all neurons. "A com" is the anterior commissure and "P com" the posterior one, "con" is one of the longitudinal connectives, and "ISN" is the intersegmental neuron.
B: An antibody specific for fasciclin I binds only to the ladder's rungs.
C: A fasciclin II antibody binds exclusively to the ladder's sides.



Above, left: The normal product of the MNB's divisions is a grapelike bunch of nerve-cell bodies hanging from a stem of axons (both in red), almost invisible within their sheath of green glial cells that fit tighter than control-top pantyhose. Right: Sabotage the *engrailed* gene, and the glial cells never appear, but are replaced by extra nerve cells.

a given cycle of cell division produces a neuron or a support cell.

If MNB doesn't activate the *engrailed* gene, something quite interesting happens. If we microinject the neuroblast with DNA that prevents the engrailed protein from being expressed by its offspring, the result is shown above right. There are no green cells at all—that is, the glial cells that normally enclose the axons didn't form. Instead, there's a larger than normal number of red cells. (A normal embryo is shown at left for comparison.) The red cells are irregularly shaped, because they're not held in by the sheath of green glial cells. Unconfined, the extra neurons burst out of the side of the bundle and form something like a little tumor, which has a very characteristic shape. So, without the *engrailed* gene, all the progeny become red cells. This molecule apparently determines whether a neuroblast's daughters become neurons or glia.

Which leads to the second question we're exploring: what happens once a cell has opted to become a neuron? What cues does it use to know in which direction to extend its growth cone? You might imagine that if the growth cone wanted to turn at the second cross-path, it would be looking for a signpost molecule that was on the second path but not the first. These signposts could be protein molecules on the surfaces of axons and cell bodies along the pathways. Such proteins would have particular shapes that would be recognized by other proteins on the growth cone's surfaces, and would tell the growth cone to turn left or right. To identify these signposts, one might search for proteins that are expressed only on the surfaces of cells along particular pathways. The whole set of such molecules might determine the set of all possible decisions that any one growth cone could make.

Two such proteins, identified by Michael Bastiani in Goodman's laboratory, are shown on the opposite page. Fasciclin I is expressed only on one bundle in each commissure or rung. Fasciclin II, in contrast, appears only on the longitudinal pathways at this stage of development. So, for instance, a nerve cell might know that it has to recognize fasciclin I, and turn left on that pathway. Once it did so, it would know that it should then search for a pathway that has fasciclin II, and turn right when it got there, and so on.

We can test these rules by making mutants. For example, if a fly embryo does not make fasciclin I (and a certain gene controlling something else has also been mutated), then none of the commissures form. The embryo generates a nervous system that lacks crossbars, but still contains the longitudinal pathways. If we could make mutations of the whole set of such genes, and combine these mutants in different combinations, we might understand the set of rules involved in constructing the array.

We're also searching for the molecules on the growth cone that could read the signposts and tell the cell whether it's going the right way. One such set of molecules are the protein-tyrosine phosphatases (PTPases). Shin-Shay Tian and Chand Desai, postdoctoral fellows in my lab, have shown that four of these PTPases are found on most or all growth cones. The molecules are then left behind on the axons of the central ner-

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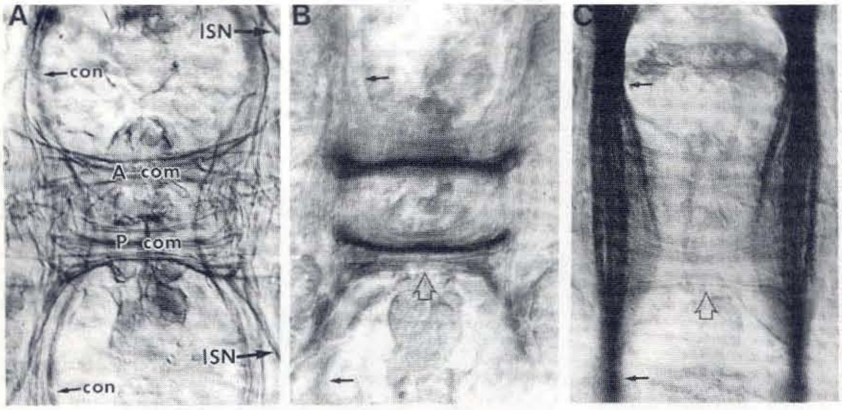

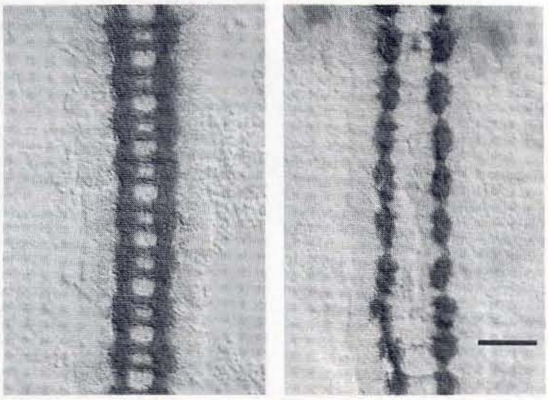

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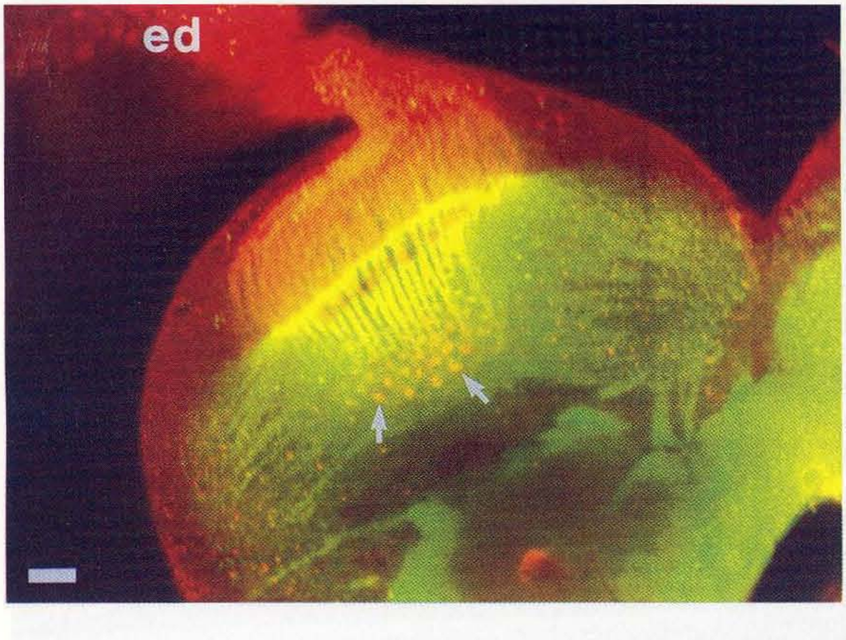
Above, left: A normal fly central nervous system.
Right: A double mutant that doesn't make fasciclin I, and whose central nervous system is a rungless ladder. The scale bar at bottom right is 20 microns.

Below: Wired for sight—the fly's embryonic eye disk (labeled "ed") is in the upper left corner. The red bundle running from it to the brain's optic lobe (at center) is the optic nerve. The cells bearing PTPases on their surfaces fluoresce green, and the orange dots (arrowed) are where connections are being made between the nerve and the brain. The scale bar at lower left is five microns.



vous system ladder after the growth cone moves on, at which point they may serve a second, as yet unknown, function. They are also expressed in the developing larval brain's optic lobes, as shown below. These are regions that receive input from the neurons in the fly's eye. It is possible that the PTPases have roles in determining the organization or function of the axons arriving from the eye.

These PTPases are proteins that span the cell membrane. The part of the molecule outside the cell probably recognizes proteins on another cell surface, and the part inside the cell catalyzes a chemical reaction that removes a phosphate group from another protein, which in turn sends a signal within the cell. So these molecules could couple pathway recognition to a signal that tells the cell to make a decision. They are highly conserved by evolution—for example, the PTPases called DLAR (found in flies) and LAR (found in humans) have very similar structures. Whenever a molecule is this similar in such different species, it probably means that it does something of fundamental importance. Once evolution finds something that works, it sticks with it.

In summary, I've shown a few examples of molecules that are involved in the construction of the insect axonal array. But even insects are very complex. They may have about 25,000 genes, of which at least half are involved in the construction of the nervous system. It's going to take a long time to understand how all those genes interact. The system is still very much a black box. Our experiments are basically fishing expeditions—we're just searching for genes that have something to do with this process, and then categorizing them by what they do. We're not at the stage yet where we can define an overall hypothesis for the mechanism by which the circuit is put together. Our lab hopes to learn about some aspects of the puzzle, and to extrapolate this knowledge into figuring out something about how vertebrate brains, and thus the human brain, are put together. □

Assistant Professor of Biology Kai Zinn earned his BA in chemistry at UC San Diego in 1977, and his PhD in biochemistry and molecular biology at Harvard in 1984. Before coming to Caltech in 1989, he was a postdoctoral fellow in Corey Goodman's lab. Married to Assistant Professor of Biology Pamela Bjorkman, the couple has two children—five-year-old Leif, and four-month-old Katya—giving Zinn ample opportunity to observe nervous system development firsthand. This article is adapted from his Seminar Day talk.

The Northridge Earthquake and the "Earthquake Deficit"

We've had a lot of earthquakes over the last five years, and we're likely to have some more over the next five years.

by Egill Hauksson

This four-story apartment building on Hollywood Boulevard west of Normandie Avenue is actually a seismic success story. The building had been retrofitted with steel tie rods to hold its brick facade to its interior members. As a result, although the building suffered severe damage, it did not collapse and no one was killed in it. The tie rods' ends are the disk- and diamond-shaped plates visible at floor and roof level across the front of the upper stories.

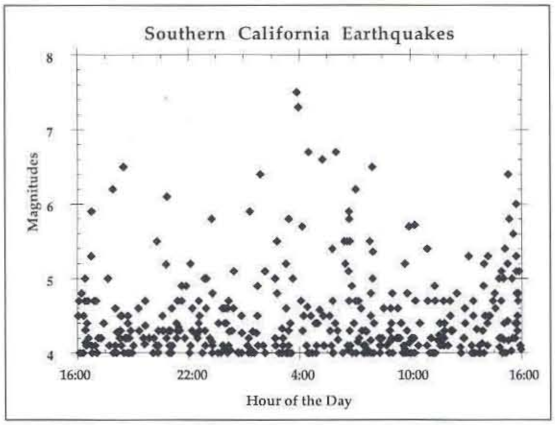
The Northridge earthquake, moment magnitude 6.7, occurred on Monday, January 17, at 4:30 a.m., getting those of us who live in the Los Angeles region out of bed a little earlier than usual. We were lucky in that respect—if the earthquake had occurred in the daytime, many more than 60 people would certainly have died. (Incidentally, several hospitals had to be temporarily closed or evacuated, mostly because of water loss or water damage from broken plumbing.) Seven large parking structures belonging to malls, hospitals, and a university collapsed—some partially and some almost completely—and many public buildings, from schools to shopping malls, suffered heavy damage, as did several freeways. But that's another story, one for the structural engineers to tell. I'm going to describe the earthquake itself, how we measured it, and what we learned from it.

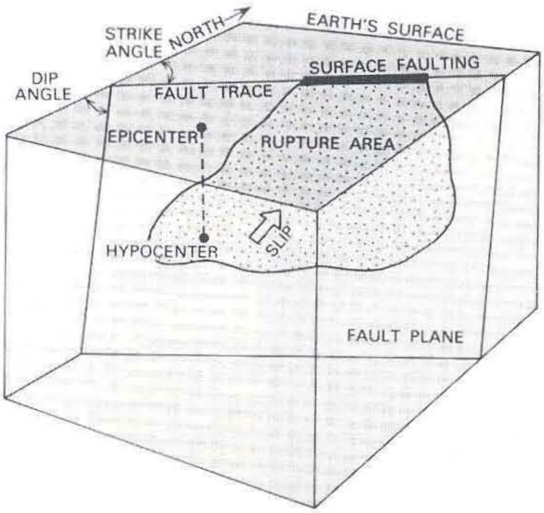
Shortly after the earthquake, the press pointed out that all the recent earthquakes—San Fernando in '71, Whittier Narrows in '87, Sierra Madre in '91, and Landers in '92—had been in the early morning hours. Why was that? So we went back to our earthquake catalog and looked at the last 50 years, and the press had indeed identified a pattern. A plot of Southern California earthquake magnitudes from 1945 to 1994 as a function of time of day shows five earthquakes of magnitude 6.7 or greater between 4:00 and 6:00 a.m. But below magnitude 6.6, the earthquakes are scattered randomly throughout the day and night. This tells us that the sample of large earthquakes is statistically insignificant. Simply put, we haven't had enough big earthquakes to complete the plot. Big earthquakes can happen at any time, so everybody has to be earthquake-prepared day and night.

At Caltech, we're always prepared. We record earthquakes whenever they happen—24 hours a day, 365 days a year—through a network of instruments we run in cooperation with the U.S. Geological Survey (USGS). These instruments are scattered from the Mexican border up into Owens Valley, and from Needles to Coalinga. There are 240 seismographic stations that contain 340 different seismometers, which are the instruments that actually record earthquakes. Most of these stations contain a single instrument that measures vertical motion, but some of the stations contain multiple instruments to measure motion in all three dimensions. All of these instruments are connected to Caltech's earthquake data center by dedicated phone lines, microwave links, or radio links, so that we get the data immediately.

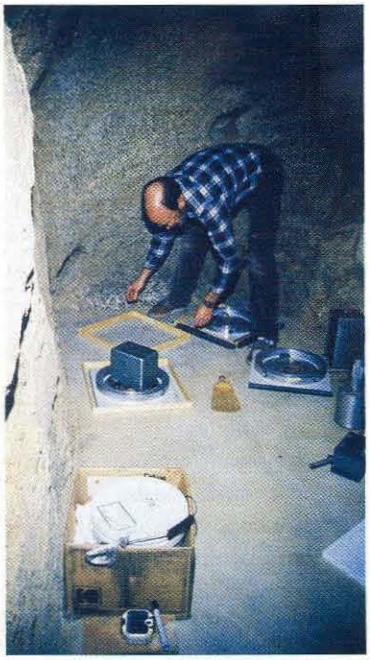
Most of our network consists of standard-issue instruments, but 17 of our sites are TERRAscope stations—state-of-the-art digital seismometers. Unlike conventional seismographs, which are designed to measure only ground motions within a fairly narrow frequency range and which "saturate," or go off-scale, if the earthquake is very large or very close by, digital seismometers can record movements of any size at any frequency. A TERRAscope station has a set of three sensors that measure small-amplitude up-down, north-south, and east-west motions; a second set of sensors for measuring large-amplitude motion (the shaking that actually causes damage) along

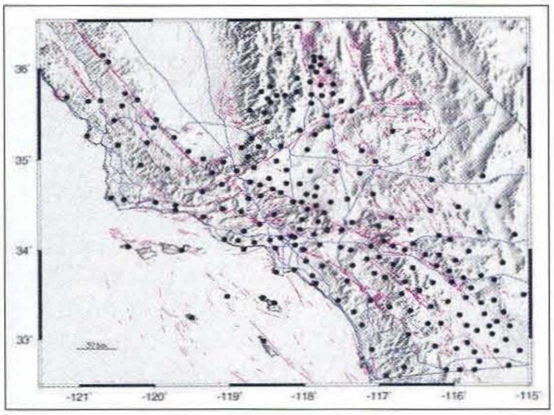
Right: Earthquakes of all sizes happen at all hours. We just haven't had enough large ones to fill in the graph.
Below: An earthquake's moving parts.





Below: The Southern California Seismographic Network, run jointly by Caltech and the USGS, consists of 240 seismographic stations (black dots). The purple lines are faults; blue lines are freeways.
Left: David Johnson, the seismo lab's field technician, installing new TERRAscope instruments in an old gold-mine tunnel near Lake Isabella.



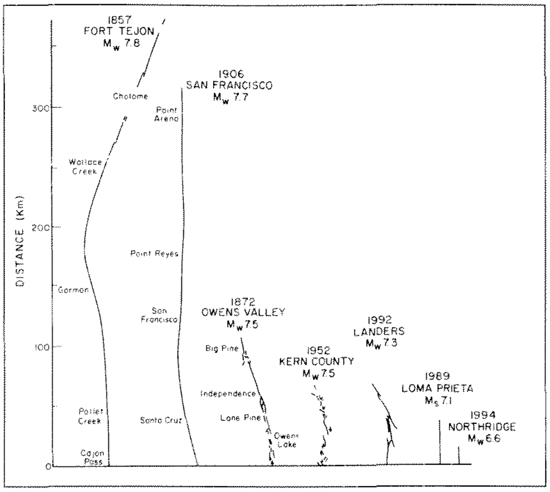


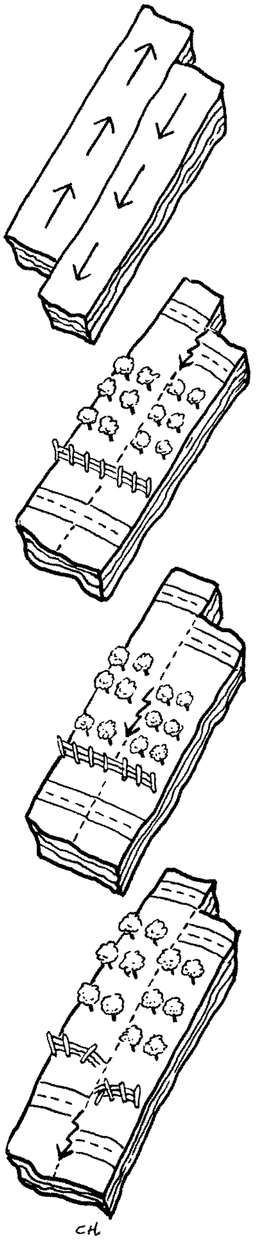
those same three axes; and a GPS (Global Positioning Satellite) receiver that we use to determine the waves' arrival times very precisely. (Most applications of the GPS system use these receivers to determine locations, but the system also generates time signals that are accurate to one-millionth of a second.) A computer at the station records all this data and transmits it back to Caltech via satellite or over a dedicated phone line. Seismologists at other institutions can then retrieve the data from our data center, using high-speed modems, or over the Internet.

When an earthquake happens, we don't know where it was or how big it was, but computers in the Seismo Lab automatically record its waves and their arrival times at our seismometers. An earthquake record consists of a P, or primary, wave—so called because it travels fastest and arrives first—followed by a slower S, or secondary, wave, plus other waves. The P and the S waves travel at well-known speeds, so the delay between the P wave's arrival and the S wave's arrival tells us how far away the earthquake was from the recording instrument. It's exactly analogous to seeing a lightning flash and then counting the seconds until you hear the thunder in order to find out how far away the lightning bolt is. One distance measurement tells us only that the earthquake lies somewhere on a circle of that radius from the seismographic station, but taking the data from many stations gives us the exact location in three dimensions—the point where all the circles intersect. Once we know the location, we can derive the magnitude from the size of the waves we record, because the waves get smaller as they travel farther away from the source. All these measurements and calculations used to be done by hand, with a ruler and a pencil, but now we use high-speed workstations that do much of the work automatically and allow the data analysts to review the results very quickly.

Where the earthquake starts—its focal point at depth—we refer to as the hypocenter. The epicenter is the projection of that point up to the earth's surface. While you can think of a magnitude 2 or 3 earthquake as having a point source, that's not so in larger earthquakes. In the Northridge earthquake, for instance, a section of fault 17 kilometers long and 13 kilometers deep broke. We *do* calculate a latitude and a longitude for the epicenter, so there is an "exact point" of sorts where the earthquake began, but we seismologists don't take that location too seriously because we know that the earthquake's waves are, in fact, radiating off every point on the entire plane of the fault that breaks.

The graphic on the opposite page lists the





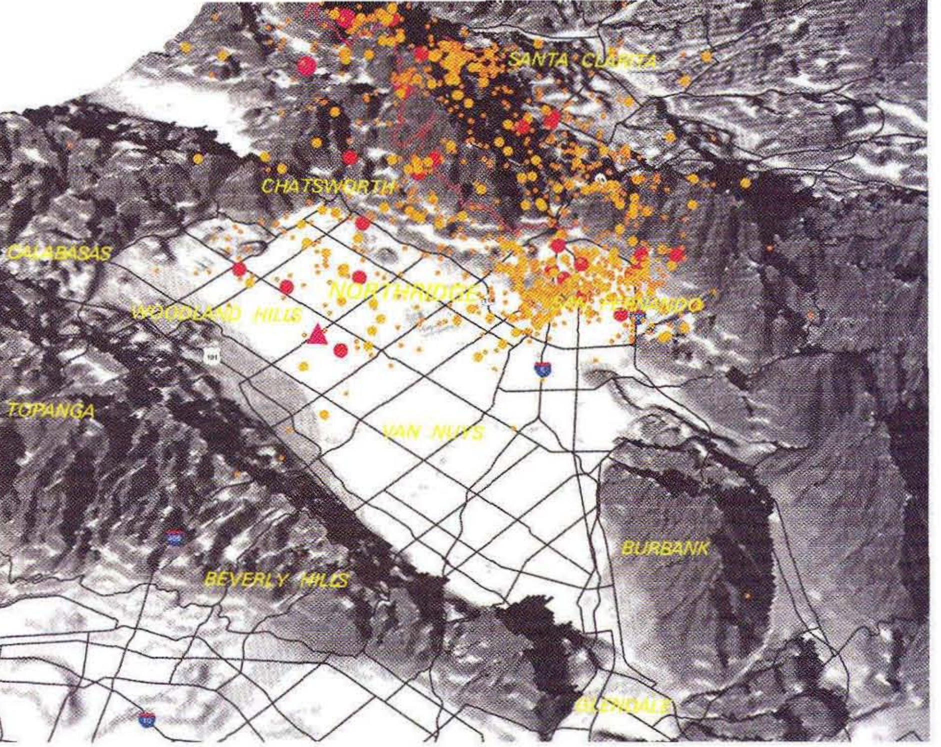
Right: A comparison of how much fault broke in several notorious California earthquakes.
Left: Until recently, it was assumed that an entire piece of fault moved as a unit during an earthquake (top drawing). But seismologists now believe that the slip begins at a single point, and travels along the fault like a zipper (the jagged arrow in bottom three drawings).

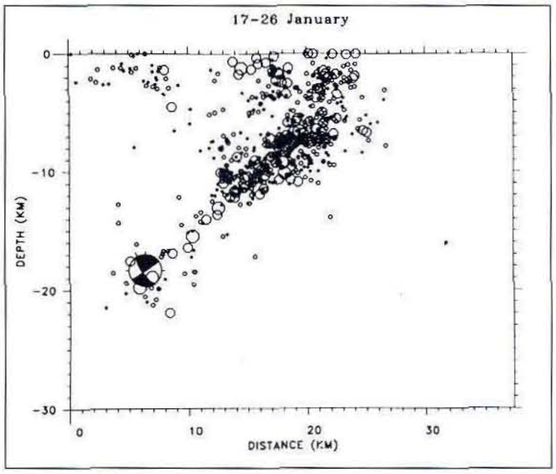
fault length for several California earthquakes. As you can see, Northridge, Loma Prieta (40 kilometers), and Landers (80 kilometers) all broke fault sections of roughly similar length. But compare the Fort Tejon and San Francisco earthquakes, which respectively ruptured for about 370 and 400 kilometers—the latter including some 80 kilometers of seafloor from Bodega Bay north toward Cape Mendocino that's not shown on the graph. These two are what we refer to as major or great earthquakes, the sort that occur on the San Andreas fault, like the coming earthquake that the press refers to as the Big One. So Northridge was, in fact, only a moderate-sized earthquake. It just happened to be underfoot.

There are a few simple rules about earthquakes: the bigger the earthquake, the larger the piece of fault that breaks; the bigger the earthquake, the farther the fault is displaced; and, finally, the bigger the earthquake, the longer it's going to last. In the Northridge earthquake, the fault moved about two meters. In the Big One, the movement could be 10 meters. It took six or seven seconds for the rupture to complete its travel along the fault segment that the Northridge earthquake was on. The Big One may take two or three minutes to break, depending on where the rupture starts. So if by the time you realize you're in an earthquake it's all over, you know it was a small earthquake. But if the shaking continues or even intensifies, you know you're in a big earthquake.

Our ideas about how earthquakes work have changed over the last decade. We used to think that one rigid block of rock would lurch past another rigid block, so that the whole fault moved at once. We now believe that the movement starts in one spot on the fault and propagates along it at about a mile and a half per second. (The speed at which the fault breaks is not the speed at which the P, S, and other waves travel.) A carpet layer putting a big rug on a ballroom floor is a good analogy. Suppose he suddenly realizes he's made a mistake—the rug is too close to the wall, and he has to move it out two feet. He tries pulling on the rug, but it's too heavy to move. But if he walks over to the wall and makes a fold in the rug, he can push the fold across the room with very little effort and move the rug two feet. The same thing happens underground—there's a fold, or pulse, that goes along the fault and allows the rock to move.

Other earthquakes rattled the Los Angeles area in the days before the Northridge earthquake. Starting on Sunday, January 9, there was a swarm of about two dozen small earthquakes under Santa Monica Bay. Many of them were felt, including a magnitude 3.7 just offshore of Venice Beach at three o'clock that afternoon. At Caltech, we refer to these as "media earthquakes"—they don't cause damage or injuries, but all the TV crews go out and ask people what it felt like. Also, 12 hours before the Northridge earthquake, we had a small cluster up near the Holser fault, by Castaic Lake. Both clusters were part of the same overall process that created the Northridge earthquake—the compression of this part of California—but neither was on the same fault as the Northridge earthquake, so we don't





Above: The San Fernando Valley, as seen from the east-southeast. Downtown Los Angeles is off the map to the lower right. The main shock's epicenter is the pink triangle in the lower left, and the first week's worth of aftershocks are plotted as circles. The circle's size is proportional to the aftershock's magnitude; those of 3.9 or less are in yellow, 4.0 or greater are in red. The jagged red line in the mountains is the Santa Susana fault. (3-D Map courtesy of ESRI, Redlands, CA; street map © Thomas Brothers Maps.)

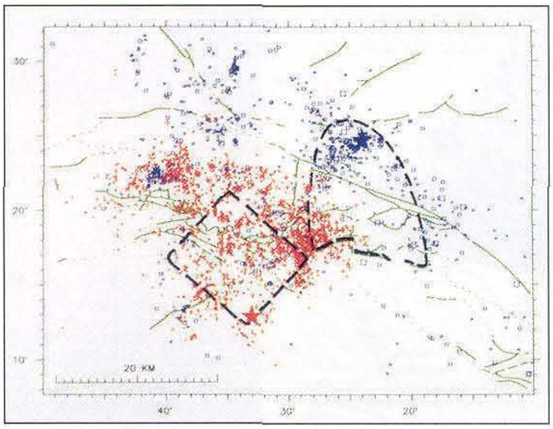
Right: A cross section through the valley and north-northeast into the San Gabriel Mountains, into which the aftershocks have been projected. The main shock is shown as a beach ball; aftershocks are open circles. The beach ball's dark quadrants indicate compression and the light quadrants expansion, showing that the fault moved up toward the surface at an angle of about 40 degrees.

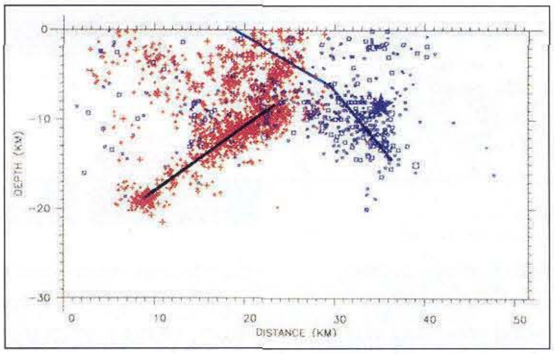
refer to them as foreshocks. We prefer the term "preshock" instead, because we don't really understand how they are related mechanically to the Northridge earthquake.

The Northridge main shock was beneath the floor of the San Fernando Valley, as shown in the map above. The majority of the aftershocks (circles) were also below the valley, and most of the rest were below the Santa Susana Mountains. (The Santa Susana fault, by the way, was not involved in this earthquake sequence.) The main trend in the aftershocks is northward and toward the surface, but there is a second block of very shallow aftershocks directly over the hypocenter, as shown in the cross section, above right. The main shock was about 19 kilometers deep, and the aftershocks that scattered up from there to a depth of eight kilometers or so defined the fault plane that broke during the earthquake. The aftershocks that continue straight up to the surface from there are probably related to the deformation of the near-surface material in response to the main shock—as the rock deep underground is thrust upward, the shallower layers on top of it had to move to accommodate it.

Because the Northridge earthquake's fault lies directly beneath the densely populated San Fernando Valley, there was tremendous damage. Most of the valley lies within the Los Angeles city limits, and by mid-February the city's Department of Building and Safety had inspected some 65,000 residential buildings in the valley and elsewhere and had red-tagged—declared unsafe to enter—1,608 of them, including many large apartment complexes. Another 7,374 bore yellow tags, indicating they were safe for entry for short periods to retrieve personal possessions. (An estimated 20,000 people camped outdoors for the first few days after the earthquake, either because their homes were uninhabitable or for fear of aftershocks; some 9,000 remained in Red Cross shelters and tent cities 10 days later.) Transportation links were severed. Collapsed bridges shut down portions of Interstates 5 and 10 and State Routes 14 and 118, and a freight train derailed in Northridge, blocking the tracks. Several major high-voltage substations within a few miles of the epicenter were knocked out, and some two million customers in the Los Angeles area lost power for the better part of the day; 900,000 of them had their lights back by dusk, but service to some places was not restored for more than a week. In a demonstration of the interconnectedness of our technological society, shattered ceramic insulators in Sylmar led to a three-hour blackout for 150,000 customers in rural Idaho, as well as isolated outages in seven western states and British Columbia. By contrast, the 1971 San Fernando earthquake, which was of equal magnitude and occurred right next door, was beneath the San Gabriel Mountains. Most of the strong ground shaking then was in the sparsely populated mountains, so the damage was much less severe.

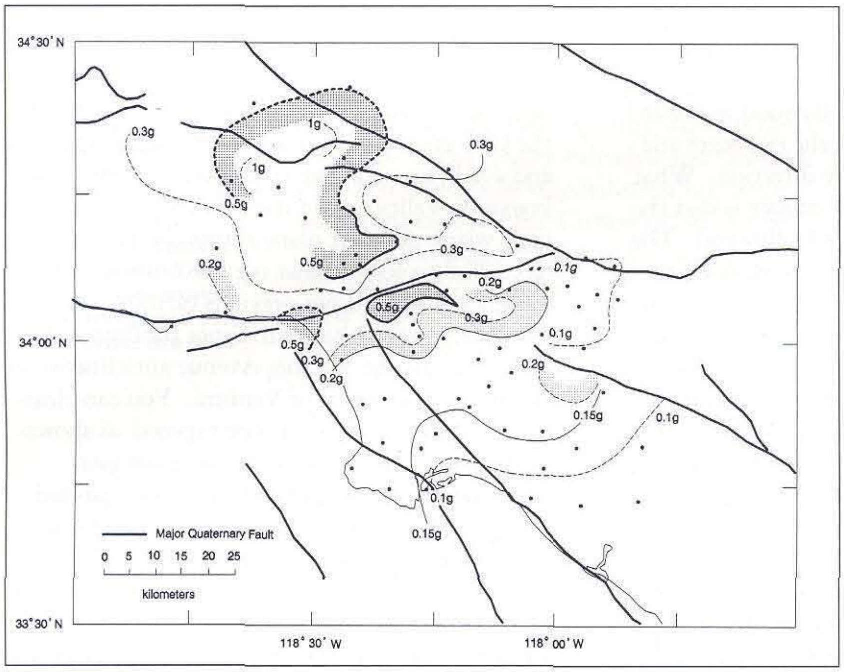
Despite their resemblance in size and place, the San Fernando and Northridge earthquakes were very dissimilar animals. Their rupture planes (dashed areas on the top map on the opposite page) abutted, but didn't cut across each other. A cross section through their faults reveals





The San Fernando earthquake and its aftershocks (in blue) and the Northridge earthquake and its aftershocks (in red) ruptured adjacent fault planes (dashed outlines), as shown in the upper plot. The two main shocks are rendered as stars. The solid black lines are mapped faults. North is at the top. Projecting the epicenters into a cross section from southwest to northeast reveals the faults that broke (lower plot). The San Fernando earthquake ruptured a steeply dipping fault and then a more shallowly dipping fault all the way to the surface. The Northridge earthquake was on a buried fault that didn't reach the surface.

Below: A contour map of horizonal ground shaking as a percentage of gravity. The darker shaded areas experienced in excess of 50 percent of gravity (0.5 g). The dots are instrument locations.

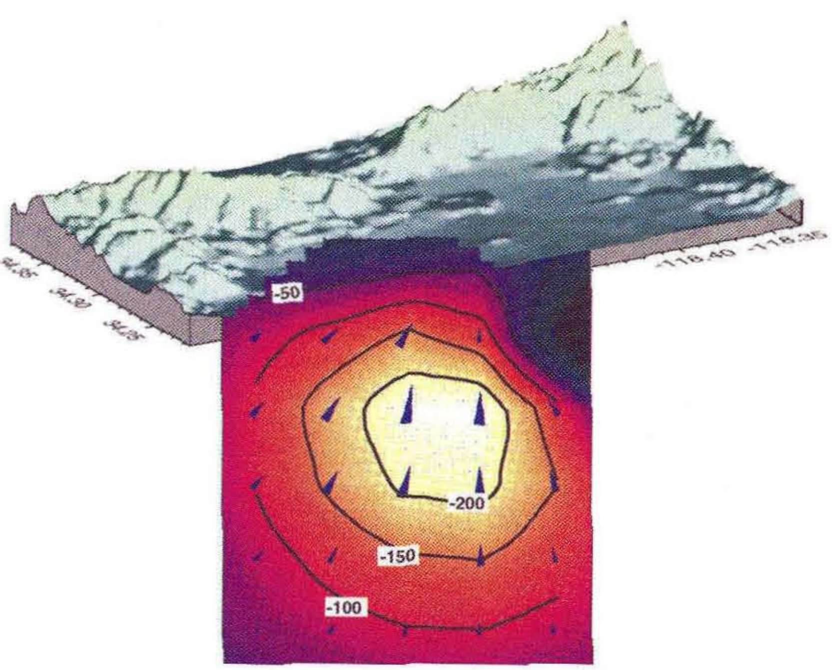


the key difference. The San Fernando earthquake started at a depth of 12–15 kilometers and ruptured all the way up to the surface. In contrast, the Northridge earthquake started at a depth of about 19 kilometers and ruptured up to a depth of 8 kilometers. These are two basically different types of faults: the San Fernando earthquake was caused by movement on a surficial, or surface-breaking, fault, and the Northridge earthquake was caused by movement on a blind, or buried, fault. (Both earthquakes were on reverse faults—a type of fault in which one side of the fault thrusts itself up and over the other. The San Andreas fault is a different type, called strike-slip, in which the two sides of the fault slip sideways with little or no vertical motion.)

And that's the first of three important lessons from the Northridge earthquake: that these blind faults, whose existence was first revealed by the Whittier Narrows earthquake in 1987, lie beneath much of the greater L.A. area. This earthquake confirmed that these faults are widespread and thus extremely dangerous.

The 1971 San Fernando earthquake left a surface rupture—a fault scarp. Nothing like this was found following the Northridge earthquake, simply because the rupture zone did not make it up to the surface. Nonetheless, the Northridge earthquake did cause ground deformation in the epicentral region, in Mission Hills, and in Potrero Canyon. This deformation was subtle in many cases—a slight bump in a sidewalk, an inch or two offset in a curb—but sufficient to crack foundations and break water and natural-gas mains.

About 95 percent of earthquake damage is caused by ground shaking, not deformation. If the ground is shaking with an acceleration that is 10 percent of the force of gravity, you'll feel it but there won't be much damage. Buildings that conform to California's Uniform Building Code are built to withstand horizontal shaking at 40 percent of gravity. (Buildings are routinely designed to carry much greater vertical loads—100 percent of gravity, which is simply the building's own weight, plus another 100 percent or so as a margin of safety to account for the occupants.) During the Northridge earthquake, the San Fernando Valley, Granada Hills, Mission Hills, and Woodland Hills all experienced horizontal ground shaking 50 percent or more of gravity, as did areas in Santa Monica and Hollywood. This is very severe shaking, and explains why there was so much damage. The strongest shaking generally gets focused in the direction along which the fault plane breaks. This fault plane aimed north and to the surface directly at



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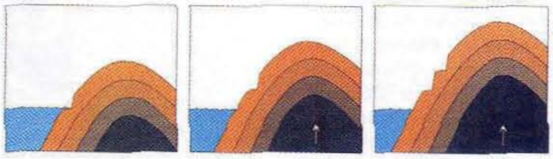


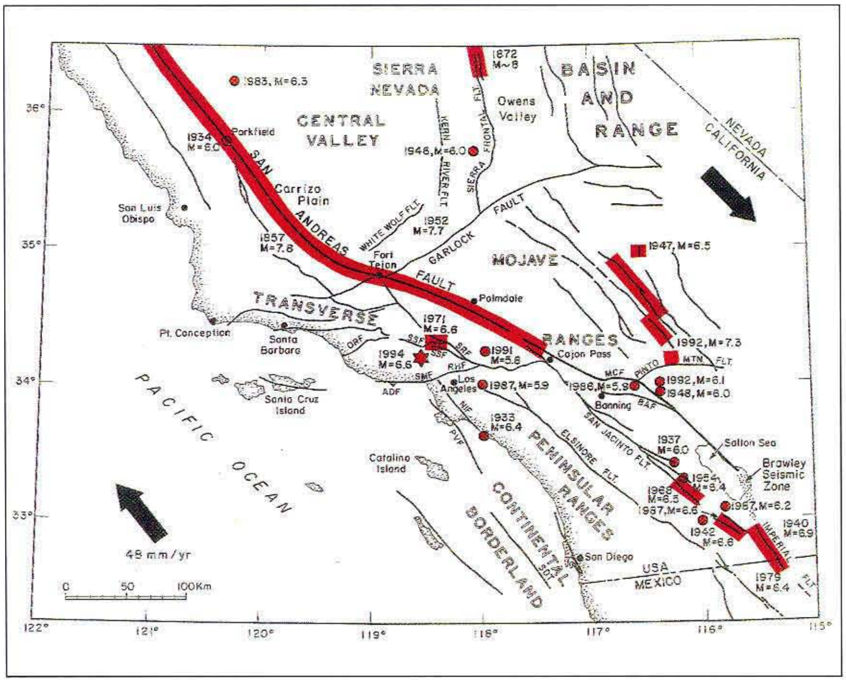
Above: In this view of the San Fernando Valley, we are looking down into the ground at an angle perpendicular to the Northridge earthquake's fault plane, or about 40 degrees from the vertical and 30 degrees east of north. The arrows show the direction of movement along the fault, while the contours show the thrust (vertical) component of that motion in centimeters. The numbers on the contour lines are negative to indicate that the north, or underside, of the fault moved downward with respect to the south side of the fault. The south side of the fault is a thin wedge that carries the San Fernando Valley and the Santa Susana Mountains on it.

the I-5/SR-14 interchange, which is one reason why it was so heavily damaged. The strong ground-shaking waves continued traveling north, ravaging the Simi and Santa Clarita Valleys and the city of Fillmore.

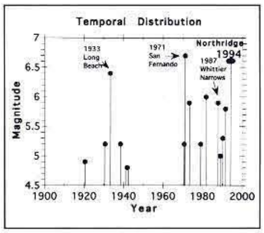
And that's the second lesson from the Northridge earthquake: the ground shaking was severe over a wide area around the epicenter. For the first time, this shaking was recorded on numerous instruments and was thus well documented. In previous earthquakes, such instruments were more thinly distributed, and only one or two of them would register strong shaking. These few records could always be explained away as anomalies of one sort or another—site effects or quirks of the building in which the instrument was located, for example.

But Santa Monica and Hollywood were hard hit, too, and they're far from the epicenter and to the south—in the opposite direction. What happened there? Part of the answer is that the ground is very soft in parts of Hollywood. The I-10 freeway collapsed at La Cienega Boulevard; La Cienega, in Spanish, means The Swamp. In areas where the soil is water-saturated, ground shaking is amplified and structures are more likely to be damaged. (See the article on liquefaction in the Winter 1990 *E&S*.) Santa Monica, on the other hand, may have fallen victim to an edge effect. The city sits on a sediment-filled basin whose edge is the Hollywood Hills. The earthquake's waves traveled through the hard rock of the hills into the sediment, where they got trapped—reverberating off the basin's rock walls and floor like a shout in an empty room. The rebounding waves canceled one another in some locations but reinforced one another in other places, causing extremely strong and long-lasting shaking in the latter spots. Ventura Boulevard, on the San Fernando Valley side of those same hills, suffered heavily for the same reason. (In passing, I'll note that the earthquake caused the usual severe damage to masonry buildings, but we're making progress—the ones that were reinforced may have been badly damaged, but at least they didn't collapse and kill anyone. And that's what reinforcement is all about, really—not making a building earthquake-*proof*, which is prohibitively expensive and perhaps impossible in most cases, but earthquake-*resistant*. This earthquake proved we can do that.)

In addition to ground deformation and shaking, the earthquake caused uplift, as measured by the GPS system. The maximum was about a foot and a half, found at the northern end of the San Fernando Valley and in the Santa Susana Mountains where the fault plane approached the surface. The geology of that region contains ample evidence of uplift from previous earthquakes. For example, as you drive up to Santa Barbara on U.S. 101, you pass the Ventura Avenue anticline about four miles west of Ventura. You can clearly see the folded layers of rock exposed, as shown in the photo above. An anticline is a region where once-flat layers of rock have been pushed up by compression from the sides. This one was created by a cycle of erosion and uplift, as shown in the three drawings below the photo. The process starts with the ocean cutting into a hillside. Erosion creates a cliff with a gently sloping beach



Above: As we all know, Southern California has its faults. This map shows some of the more prominent ones. ORF is the Oak Ridge fault, SSF is the Santa Susana fault, SRF is the San Gabriel fault, SMF is the Santa Monica fault, RHF is the Raymond Hill fault, ADF is the Anacapa–Dume fault, NIF is the Newport–Inglewood fault, PVF is the Palos Verdes fault, BAF is the Banning fault, and MCF is the Mission Creek fault. SDT is the San Diego Trough fault. The red regions show the portions of faults that have broken in historic earthquakes. The earthquakes' magnitudes and years of occurrence are also shown.



Below: Plotting earthquake magnitudes in the L.A. basin versus the year in which they occurred reveals two clusters of activity, one of which continues today.

at its foot. When an earthquake happens, the cliff and its beach get uplifted. The ocean resumes cutting its cliff and beach into the new, freshly exposed hillside, until another earthquake uplifts the hillside yet again. And if the uplift continues over the eons, it carves the hillside into a series of terraces that once were beaches. We can date the earthquakes by dating the terraces, using carbon-14 dating on the seashells we find buried there.

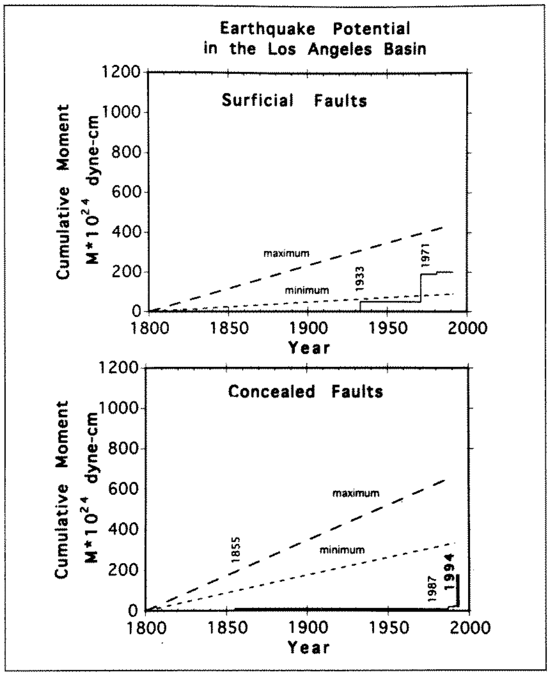
Stepping back a little bit, why do we have all these earthquakes in Los Angeles? Because the Pacific plate and the North American plate meet there, and they're moving past each other in opposite directions. The interface between the two plates is the San Andreas fault, which comes north out of the Gulf of California, extends past the Salton Sea and Palm Springs, jogs left near Morongo Valley, passes Palmdale and Gorman, and then resumes its original more northerly course through Central California and the Bay Area. So the plates move parallel to each other on the southern and northern parts of the fault, but there's this kink in the middle. Material on the Pacific plate gets compressed as it goes into the bend near Morongo Valley, and that causes earthquakes. Then the plate gets shoved out to the west when it wants to go north, and that causes still more earthquakes. This whole process has created about 300 secondary faults, some of which are shown above.

We know from geological and geophysical studies that the net average movement between the Pacific and North American plates is about four and a half centimeters per year. This is about as fast as your fingernails grow. But unlike your manicure going ragged, this movement doesn't happen gradually—it happens in discrete events, i.e., in great earthquakes. We also know that about 80 percent of this movement is accommodated along the San Andreas fault. The remaining 20 percent occurs on the secondary faults. Of that 20 percent, about 5 percent occurs out in the Mojave Desert, and has caused the Landers and other earthquakes. The remaining 15 percent occurs in faults in the L.A. area and in the Transverse Ranges to the northwest.

In the past 60 years, the L.A. area has had three sizable earthquakes and a number of moderate ones, but they haven't been evenly distributed over time or space. If we plot earthquake magnitudes since 1900 as a function of time, we find two clusters of activity. The first one, from 1920 to 1942, included the Long Beach earthquake. The second one runs from 1970 to the present. Most of the earthquakes in the first cluster were south of downtown Los Angeles. Nearly all of the second cluster have been along the northern edge of the Los Angeles Basin, along the front of the San Gabriel Mountains.

A simple forecasting method works quite well, at least for the weather in Los Angeles: the weather today is probably the weather we're going to have tomorrow. So we've had a lot of earthquakes over the last five years, and we're likely to have some more over the next five years. And this is the third lesson from the Northridge earthquake: it drove home the point that we're in a period of increased seismicity.

There's another reason for surmising that we're



The diagonal lines show the calculated minimum and maxium amounts of strain energy stored in L.A.'s faults over the years; the vertical lines show how much of that energy has been released in our various earthquakes.

in for more earthquakes. The red patches in the map on the previous page show which parts of the local faults have broken in recent earthquakes. There are large expanses that aren't red, so we have quite a bit of fault area that still has to move. We can divide these faults into two basic types: surficial faults like the Newport–Inglewood fault, which caused the Long Beach earthquake, and buried faults like the Whittier Narrows thrust ramp (not shown on the map), and the Santa Monica thrust ramp. We can sum up the energy stored on these two groups of faults since 1800, although our knowledge of the accumulation rates isn't perfect. But still, we can get a maximum and minimum value for each fault class. We've had two surficial-fault earthquakes, Long Beach and San Fernando, and it turns out that we're about in balance—the energy released in those two events is about half way between our calculated minimum and maximum. Measuring slip rates on the buried faults is much trickier, obviously, because we can't see them directly, but consulting geologists Tom Davis and Jay Namson have examined geological data from the L.A. basin and found deeply buried sediments known to have been deposited at sea level. Dating these sediments and noting their current elevations gives us a rough idea of how fast they're being moved. (These estimates are really a lower limit, since there are buried faults lurking out there that haven't made themselves known yet.) The buried faults are accumulating energy at about twice the rate of the surficial faults, but we've only had one sizable "blind" earthquake—Northridge. In other words, energy

The Modified Mercalli Scale

I – Not felt.

II – Felt by persons at rest or on upper floors.

III – Felt indoors. Hanging objects swing. Vibration feels like a passing light truck. Duration may be estimated. May not be recognized as an earthquake.

IV – Vibration as of passing heavy trucks, or a heavy object striking the wall. Standing motor cars rock. Windows, dishes, doors rattle. Glasses clink. Crockery clashes. Wooden walls and frames creak, especially in the upper range of IV.

V – Felt outdoors. Direction may be estimated. Sleepers wakened. Small, unstable objects displaced or upset. Doors, shutters swing closed or open. Pictures knock against wall and tilt. Pendulum clocks stop, start, or change rate.

VI – Felt by all. Many frightened and run outdoors. Persons walk unsteadily. Many dishes and glasses and a few windows break; knicknacks, books fall off shelves. Pictures fall off walls. Light furniture often overturns; heavy furniture and appliances move. Weak plaster and masonry cracks. Small church bells ring. Trees, bushes sway visibly and rustle audibly.

VII – Difficult to stand. Noticed by drivers of motor cars. Furniture broken. Unreinforced masonry cracks. Weak chimneys snap off at roof line. Waves in ponds, swimming pools. Plaster, loose bricks, tiles, cornices fall. Large bells ring.

VIII – Steering of motor cars affected. Partial collapse of well-built but unreinforced masonry; masonry properly reinforced against lateral forces endures. Stucco walls fall. Chimneys, monuments, towers, elevated tanks twist and fall. Frame houses move off foundations if not bolted down, panel walls thrown out. Branches broken from trees. Cracks appear in wet ground and on steep slopes.

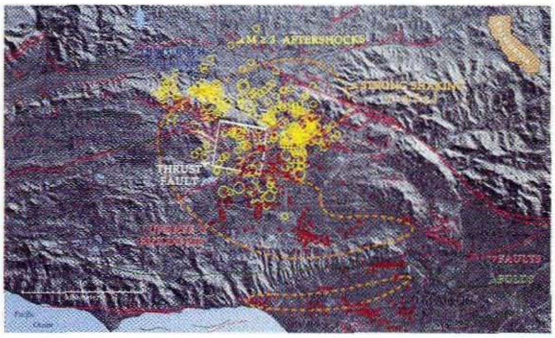
IX – General panic. All masonry, except that especially designed and reinforced to withstand lateral forces, destroyed or seriously damaged. General damage to foundations. Bolted-down frame houses thrown out of plumb. Serious damage to reservoirs. Underground pipes may break. Conspicuous cracks in ground. Sand blows and craters in alluvial areas.

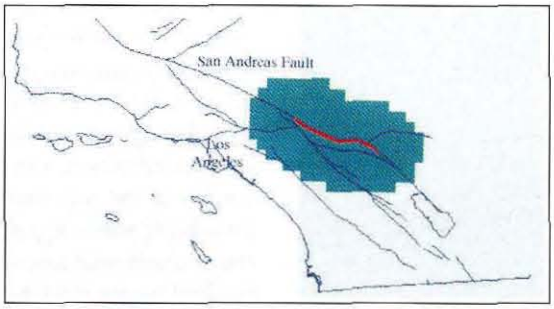
X – Most masonry and frame structures and their foundations destroyed. Large landslides. Serious damage to dams, dikes, embankments. Sand and mud shifted horizontally on beaches and flat land. Railroad rails bent slightly.

XI – Rails bent greatly. All buried pipes broken. Widespread ground disturbances of all sorts, including fissures, slumps, and slides.

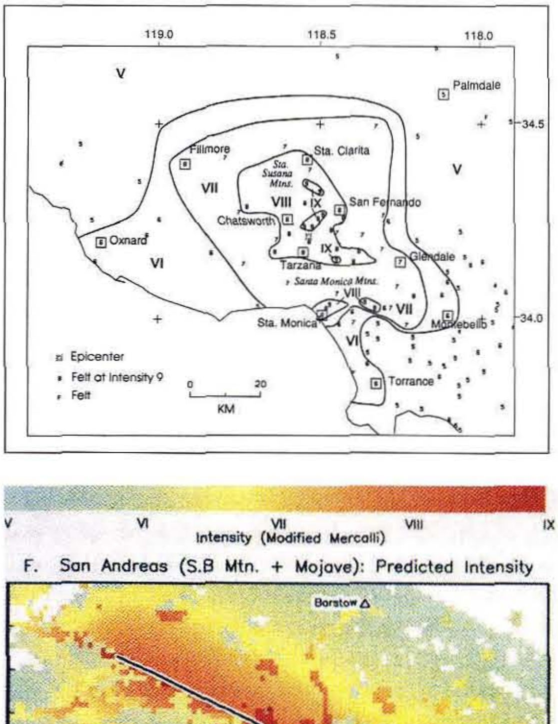
XII – Damage nearly total. Large rock masses displaced. Rivers change course. Lines of sight and level distorted.

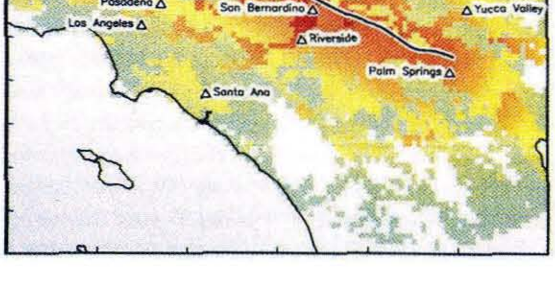
Above right: The areas of strong shaking (in excess of 50 percent of gravity horizontally) felt in the Northridge earthquake are outlined with dashed orange lines. They include most of the San Fernando Valley, and a strip running from Santa Monica to Hollywood.
Below right: For a hypothetical magnitude 7.0 earthquake that breaks the San Andreas fault from Banning to Tejon Pass (red line), the area of horizontal shaking at 10 percent of gravity or greater is shown in green. Here the regions at risk are the San Bernardino, Riverside, and Palm Springs areas.





Above right: A contour map of the modified Mercalli intensities felt in the Northridge earthquake. The small numerals are data points. The squares represent the named cities.
Below right: Calculated intensities for that same hypothetical 7.0 on the San Andreas. The intensities are coded according to the color bar across the top of the map.



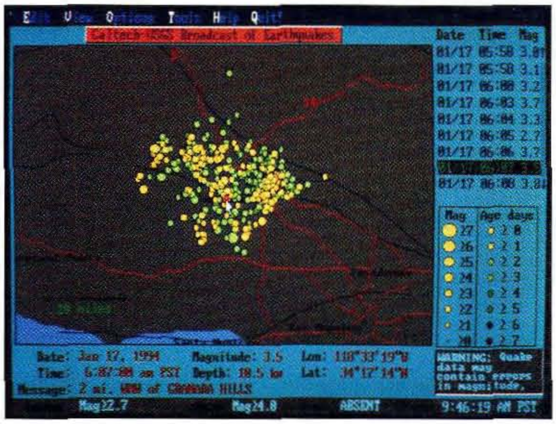


is accumulating on these buried faults faster than it's being released. That's why we seismologists talk about an "earthquake deficit"—we're missing five or six Northridge-sized earthquakes, or one magnitude 7.2 or 7.3 earthquake.

We've designed computer models that estimate the shaking from various plausible earthquakes, so let's compare the Northridge earthquake with what we might expect in a great earthquake on the San Andreas fault. The Northridge earthquake's area of shaking at about 10 percent of gravity or greater is 30 to 40 kilometers across, as shown in the top map at left. If we do a calculation for the San Andreas fault using a magnitude 7 earthquake that starts near Banning and runs up toward Tejon Pass (next map down), we get an area of shaking at 10 percent of gravity or greater that extends for 170 kilometers or so. But the earthquake doesn't cause very strong shaking in the Los Angeles metropolitan area itself. The communities at risk from this earthquake are San Bernardino, Riverside, and their environs.

Another way to look at earthquake damage is with the modified Mercalli intensity scale, which was invented in 1902, back before we had a lot of seismological instruments. It was modified in 1931 by Harry Wood, the first director of what became Caltech's seismo lab, and Frank Neumann, then chief of the seismological division of the U.S. Coast and Geodetic Survey (the forerunner to the USGS), to take into account such innovations as skyscrapers, motor cars, and underground pipelines. This scale describes the strength of the shaking observed at any given location, and goes from I up to XII. (We give Mercalli ratings in Roman numerals to avoid confusion with the Richter and other magnitude scales.) In Northridge, the maximum Mercalli rating was IX, as shown in the third map down. At IX, apartment buildings lose stories, unreinforced masonry buildings are severely damaged, and trains are knocked off their tracks. Where the Mercalli intensity was VIII, everything was thrown off shelves, chimneys toppled, and there was significant damage. At VII, there was strong shaking, but not all that much damage. If we again do a calculation for that 7.0 on the San Andreas (bottom map), we see a much larger area of intensity VIII or greater. Very significant damage would occur in San Bernardino and Riverside Counties, but there would be pockets of damage in the San Gabriel Valley, and somewhat more widespread damage in the San Fernando Valley and communities to the north like Santa Clarita. In an earthquake of this type, you would feel the shaking for a long time—for

Right: After gallivanting across the country from East L.A. to Chicago, Atlanta, and back again, the CUBE data winds up on a computer display. This one shows the first few days' worth of Northridge aftershocks (colored circles). The information in red pertains to the red-highlighted earthquake under the cursor arrow. Black lines are faults; red ones are freeways. Below: The Northridge earthquake bounced some of the 64 cars in this freight train clean off the track. Even though one tanker spilled 8,000 gallons of sulfuric acid, the line was back in service within 48 hours.



Date Time Mag
01/17 05:58 3.0
01/17 05:58 3.1
01/17 06:00 3.2
01/17 06:03 3.7
01/17 06:04 3.3
01/17 06:05 2.7
01/17 06:06 3.7
01/17 06:08 3.8

Date: Jan 17, 1994 Magnitude: 3.5 Lon: 118°33'19"W
Time: 6:07:00 am PST Depth: 18.5 km Lat: 34°17'14"N
Message: 2 mi. WNW of GRANADA HILLS

WARNING: Quake data may contain errors in magnitude.

several minutes—but most of that would be light shaking. The strong shaking would only last for several tens of seconds.

At Caltech, we don't merely study earthquakes. We're also working on various ways to mitigate their effects. I've just described one method, which is to calculate the effects in advance so that structural engineers will know what forces a building in a given area must be designed to withstand. And while we still don't know how to predict earthquakes, we can do the next best thing, which is to provide information about the earthquake very quickly after it happens. This allows dispatchers to send emergency crews (such as fire brigades and ambulances) to the hardest-hit areas, enables gas and water companies to shut off mains that might be broken, permits electric utilities to reroute power around damaged lines, and so forth. We've put together a system called CUBE, which stands for Caltech-U.S. Geological Survey Broadcast of Earthquakes. It's still in the development phase, but we already have 15 utilities and transportation companies as subscribers. Local governments also subscribe to it, and we have two media subscribers—KTTV and KNX radio. CUBE uses the earthquake data—location, magnitude, and time—that's automatically generated by the computers here at Caltech, and sends that information on a radio link to our local cellular-phone company, which pages the 200 or so individual users. In some cases, people carry beepers that read out the information; in others, the pagers are hooked up to PCs that plot earthquakes as dots on a map. It takes about a minute to a minute and a half to determine the earthquake's location and magnitude, and then about 20 seconds to route the data through the paging system. The message has to go from Caltech to the phone company's office in East L.A., which sends it by satellite to Chicago, then Atlanta, and then back to Los Angeles. This is what happens when you really get caught up in high-tech.

But CUBE isn't as high-tech yet as it needs to be, and the system did not respond to the Northridge main shock as a result. (The system has done a good job of reporting the aftershock sequence, however.) CUBE missed the main shock because most of the seismometers in the network are the old-fashioned analog kind that record their data in the form of frequency-modulated waves, a method of data-encoding similar to the way that an FM radio station broadcasts audio signals. The seismic data are transmitted over microwave links that suffer from random bursts of noise caused by various atmospheric phenomena. Our computers think that the noise bursts are actually big earthquakes, and the system gets saturated trying to process them. To prevent this, we've designed "traps" in our software that recognize the noise bursts and discard them. So when the Northridge earthquake hit, the computers assumed that the huge waves in the incoming data were another noise burst, and ignored them. We need to replace our analog instruments, which were state-of-the-art in the early 1970s, with modern digital ones that record and transmit the data as packets of digital information. Then the computers would know that all the data reaching them is real. We're working with our local phone companies to see if we can get dedicated phone lines that wouldn't be subject to the atmosphere's whims, but what we really need to do is to replace those analog instruments with digital ones that have capabilities similar to the TERRAscope. And that's expensive, so we're installing the digital ones as we can afford to. ☐

Egill Hauksson, senior research associate in geophysics, earned his M.S. in geophysics from the University of Trondheim in Norway in 1974, and his M.A., M. Phil. and Ph.D. from Columbia University in 1978, 1980, and 1981, respectively. He joined the Caltech faculty as a research fellow in 1989, becoming a senior research associate in 1992. Hauksson keeps seismology in the family—his wife, Lucile Jones, is a seismologist at the USGS's Pasadena office, conveniently located across Wilson Avenue from the Seismo Lab. (Jones is also a visiting associate in geophysics at Caltech.)

The Accidental Entrepreneur

There is such a thing as a natural-born entrepreneur. . . . But the accidental entrepreneur like me has to fall into the opportunity or be pushed into it.

by Gordon E. Moore

Like many other scientists and engineers who have ended up founding companies, I didn't leave Caltech as an entrepreneur. I had no training in business; after my sophomore year of college I didn't take any courses outside of chemistry, math, and physics. My career as an entrepreneur happened quite by accident.

And it ran counter to early predictions. When I was graduating from Caltech with my PhD in chemistry in 1954, I interviewed for jobs with several companies, one of which was Dow Chemical. Dow was interested in setting up a research laboratory in California, and they thought I might be someone they could send to headquarters in Midland, Michigan, to train to come back here in some kind of managerial role. So they sent me to a psychologist to see how this would fit. The psychologist said I was OK technically but I'd never manage anything. Dow did end up offering me a job in Midland, but the transfer back to California was no longer a part of it.

I didn't go to Midland after all, but went instead to the Applied Physics Laboratory at Johns Hopkins University, which has roughly the same relationship to Johns Hopkins that JPL has to Caltech, and where I could continue to do basic research in areas related to what I had done before. But I found myself calculating the cost per word in the articles we published and wondering if the taxpayers were really getting their money's worth at $5 per word. Just as I was starting to worry about the taxpayers, the group I was working in was, for various reasons, breaking apart. So I decided to look for something that had a bit more of a practical bent, and at the same time see if I could get myself back to California.

Lawrence Livermore Laboratory interviewed me and offered me a job, but I decided I didn't want to take spectra of exploding nuclear bombs, so I turned it down. Then one evening I got a call from Bill Shockley, who had gotten my name from Lawrence Livermore's list of people who had turned them down. Now, Shockley is a name that has a Caltech association. After earning his BS here in 1932 he went on to invent the transistor. He had been working at Bell Laboratories, and now he wanted to set up a semiconductor company out on the West Coast (a lot of Caltech connections here—the operation was financed by Arnold Beckman) with the idea of making a cheap silicon transistor. Shockley knew that a chemist was useful in the semiconductor business; they had chemists at Bell Labs, where they did useful things. And I was a chemist, so Shockley caught up with me. Still not an entrepreneur, I decided to join this operation.

I was employee number 18. This was a start-up operation. All of us except Shockley were young scientists, in our late twenties. I had no management experience or training. Unfortunately, neither did Shockley. He had run a research group at Bell Laboratories, but this was to be an enterprise rather than a research group, and he had no real experience in running a company. I suppose maybe I should have been suspicious when none of the people who had worked with him at Bell Labs joined his new venture, but I didn't even begin to think about that then.

Shockley was phenomenal from the point of view of his physical intuition. One of my

William Shockley's employees drink a toast to him on the day in 1956 when he won the Nobel Prize for inventing the transistor. Eight of the crew shown here, including Bob Noyce, standing at center with raised glass, and Gordon Moore, seated at left and turned toward Shockley, went off on their own the next year and founded Fairchild Semiconductor Corporation.

colleagues claimed Shockley could *see* electrons. He had a tremendous feeling for what was going on, say, in silicon, but he had some peculiar ideas for motivating people. For example, the company had something we dubbed the PhD production line. One day he told a group of us: "I'm not sure you're suited for this kind of a business. We're going to find out. You're going to go out there and set up a production line and run it. You know, *do* the operation, not direct it." This didn't go over especially well, because the group dutifully tried to operate a production line on a product that was still in the early stages of development.

He also set up a secret project. Those of us who weren't involved couldn't know what it was, although Shockley did let us know that it was potentially as important as the invention of the transistor. In such a small entrepreneurial group, having in-people and out-people created some dissension, the sort of thing that makes it hard to keep everybody working together as a team. As another illustration of his motivating skills, one day Shockley asked a group of us what we would like to do to make the job more interesting. Would we like to publish some papers? We said, "OK," so as a way of satisfying this demand he went home that night and worked out the theory of an effect in semiconductors. He came back the next day and said, "Here. Flesh this out and put your name on it and publish it." Finally, the beginning of the end, as far as morale was concerned, occurred when we had a minor problem in the company and Shockley decided that the entire staff was going to have to take lie detector tests to find out who was responsible for it.

Then he switched from his original idea of building a cheap silicon transistor to building a rather obscure device known as a four-layer diode. We viewed this with considerable concern, because some of us didn't understand exactly where the four-layer diode fit in. One day, when Arnold Beckman came around to talk to the group, Shockley made some closing remarks, just out of the blue, indicating that he could take his staff and go someplace else if Beckman wasn't enthusiastic about what was happening there. So, given all these problems, we decided that we had to go around Shockley to solve them. A group of us contacted Beckman and sat down with him through a series of dinners to try to work out a position for Shockley, in which he could give us the benefit of his technical insights but not of his management philosophy. We were thinking in terms of a professorship at Stanford. By that time, he had won a Nobel Prize, and Nobel Prize winners can get a professorship almost anywhere. What we didn't appreciate is that it's awfully hard to push a Nobel Prize winner aside. Beckman decided (as the result of advice he had received elsewhere) that he really couldn't do this to Shockley. We were told essentially that Shockley was in charge, and if we didn't like it we probably ought to look at doing something else. We felt we had burned our bridges so badly by that time that we clearly had to leave, and we started to look at alternatives. (Shockley's company held on for a few years, was acquired by Clevite Corporation, and died eventually.)

And this is where I finally became an entrepreneur. One of our group had a friend at Hayden Stone, a New York investment banking house. He wrote the friend a letter saying that there was a group of eight of us here that really enjoyed working together, but that we were leaving our current employment, and did he think that some company might like to hire all of us. The investment bankers said, "Wait a minute," and sent one of the partners, Bud Coyle, and a young Harvard MBA named Arthur Rock out from New York to visit with us. They talked to us and said: "You don't want to look for a company to hire you; you want to set up your *own* company." That didn't sound bad. By doing that we could stay where we were. We had all bought houses by then (they were affordable in California at that time), and we wouldn't have to move. It seemed a lot easier, so we said, "OK; fine; let's do it," and they said they would find backing for us.

So we sat down with *The Wall Street Journal*, and went through the New York Stock Exchange listings, company by company, to identify which

The Fairchild Eight (a.k.a. the Shockley Eight) at Fairchild Semiconductor in 1959. From left: Gordon Moore, Sheldon Roberts, Eugene Kleiner, Bob Noyce, Victor Grinich, Julius Blank, Jean Hoerni, and Jay Last. (© Photograph by Wayne F. Miller, Magnum Photos)

Each of the eight of us invested $500 in this startup. . . . Fairchild put up some $1.3 million to get us going, and we started Fairchild Semiconductor Corporation.

ones we thought might be interested in supporting a semiconductor venture. We identified 30-some companies, and Arthur and Bud went out and contacted every one of them. They all turned it down without even talking to us. Then, quite by accident, Arthur and Bud ran into Sherman Fairchild, who happened to be a technology buff; he really loved new technology. He introduced them to the chairman of Fairchild Camera and Instrument, who was willing to take a shot at supporting this new company.

Each of the eight of us invested $500 in this start-up. That may not sound like much now, but it was a month's salary in 1957. Fairchild put up some $1.3 million to get us going, and we started Fairchild Semiconductor Corporation. We still weren't really quite entrepreneurs, but we had learned something along the way. We had learned from the Shockley experience that none of us knew how to run a company, so the first thing we had to do was to hire our own boss—essentially hire somebody to run the company. We advertised for a general manager. Now, when you advertise for a general manager for something like this, what you find is that every salesman in the country is convinced that *he* can run a company. But buried among all the responses from salesmen was one from Ed Baldwin, the engineering manager for the Hughes semiconductor operation. In the mid-fifties, Hughes was making diodes and was one of the largest semiconductor companies in the world.

Baldwin came and told us a lot of things we didn't know, so we decided that he was the right guy to bring in to run our company for us. We hired him, and he taught us a variety of things that we hadn't learned before—since most of us had not even worked for a successful manufacturing company. He taught us that the different parts of the organization should be established with different responsibilities; for example, you have to set up the manufacturing operation separate from the development laboratory. You have to engineer and specify manufacturing processes, which is completely different from getting something to work once in the laboratory. He even taught us that we should bring in a marketing manager, which we did. And everything was working fine: the development and preproduction engineering for our processes and first products was complete; we had a thick process-spec book that recorded all the detailed recipes; and we had interested customers. Then one day we came to work and discovered that Baldwin, along with a group of the people that he had suggested we hire, were leaving to set up a competing semiconductor company down the road. This was the first of the Silicon Valley spin-offs that we suffered.

We never really quite understood this. Baldwin had the same potential equity participation that we did; but he never invested his $500 so he never got the stock. He didn't consider Fairchild Semiconductor his company, and since he wanted his own company, he left us. (He and his group also left with the "recipes"; eventually they had to return the copy of the spec book to us.) After our initial feelings of shock and betrayal, we sat down and discussed what we should do. Should we go out and hire another guy to come in and run the company? We decided instead that we would try to go it alone with one of our own. So Bob Noyce, who was the one of us with the most semiconductor industry experience, became general manager. I took a sideways step to his previous position as director of research and development.

Besides what Baldwin had walked off with, we had a few other ideas coming along at that time. One of them was something called a planar transistor, created by Jean Hoerni, a Caltech postdoc whom Shockley had recruited. In fact, I had joined Shockley for the trip down to Pasadena to recruit him. Jean was a theoretician, and so was not very useful at the time we were setting up the original facility at Fairchild, building furnaces and all that kind of stuff. He just sat in his office, scribbling things on a piece of paper, and he came up with this idea for building a transistor with the silicon oxide layer left on top over the junctions. Where the silicon junctions come to the surface of the silicon is a very sensitive area, which we used to expose and had to work awfully hard to keep clean. Hoerni said, "Why not leave the oxide on there?" The conventional wisdom from Bell Laboratories had been that by the time you got done, the oxide was so dirty that you wanted to get rid of it. Nobody had ever tried leaving the oxide on. We couldn't try it either, because it required making four mask steps, each indexed with respect to the next with very high precision—a technology that didn't exist. Our first transistor took two mask steps, and that was a fairly significant development operation.

So we couldn't even try Jean's idea until a year and a half or so after we had gone into business. When we finally got around to trying it, it turned out to be a great idea; it solved all the previous surface problems. Then we wondered what else we might do with this planar technology. Bob Noyce came up with the two key inventions to make a practical integrated circuit: by leaving the oxide on, one could run interconnections as metal films over the top of its devices; and one could also put structures inside the silicon that isolated one transistor from the other.

Noyce and Kilby, who was at Texas Instruments, are often considered co-inventors of the integrated circuit. In fact, they did dramatically different things. Kilby built a laboratory model—a little circuit with transistors and resistors—by etching long, thin semiconductor structures, all connected by tiny wires. It really wasn't a practical production process. What Bob did was to take the idea of the integrated circuit—this planar technology—and come up with a way of building a practical device.

It turned out that the world really wanted some of these new devices, which led to some management challenges. We didn't have any idea of the magnitude of the opportunity we were dealing with. We were still a bunch of guys in a laboratory, somewhat amazed that people actually wanted to buy our products. We hadn't thought about expanding, but here again our theoretician, Jean Hoerni, had early on made a contribution by designing the layout of our facility to allow for what we presumed to be sufficient expansion—an extra furnace here, more nitrogen cylinders there. But we had little notion of the impact of our discovery. Here we had developed and engineered the first integrated circuits, the first family of logic circuits—very simple devices with simple gates and flip flops—and put them into production. I remember calling the senior people in the laboratory together and saying, "OK, we've done integrated circuits. What'll we do next?" And we started looking for all the peculiar physical effects we could find to see what *new* devices we could invent. We had *no* idea at all that we had turned the first stone on something that was going to be an $80 billion business.

As a result of our ignorance, we sent our profits back to the parent company on the East Coast rather than asking to reinvest them in expanding Fairchild Semiconductor more rapidly. Now, it's not clear that we could have expanded a lot more rapidly even if we had tried, because there were significant limitations on the management crew we had. We were all still going through on-the-job training. We were mining an extremely rich vein of technology, but the mining company was too small to handle what was going on. The net result was what I call the "Silicon-Valley effect": every new idea that came along created at least one new company. Literally dozens of companies came out of the Fairchild experience. Not only did the technology come out of it, but Fairchild also served as a successful and encouraging example of entrepreneurship—the if-that-jerk-can-do-it-so-can-I syndrome.

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While we were learning on the job, Fairchild grew to be about a $150 million business and some 30,000 employees by the late sixties. It was a fairly significant corporation by the time we were done. But things began to deteriorate—partly, I think, because it was controlled by an East Coast company. The West Coast tail was not very effective at wagging the East Coast dog. Fairchild developed some management problems. In fact, the board fired two chief executive officers within a six-month period, and was running the company with a three-man committee as the board of directors. Clearly the direction of the company was going to change. When Bob Noyce (who was the logical internal candidate to become chief executive of the parent company, Fairchild Camera) saw that he would be bypassed for the job, he decided to leave. I felt that new management would probably change the nature of the company significantly. I decided I'd rather leave before any changes, than after. So the two of us set off to do something else. We had really come a long way at Fairchild. We had also made a tremendous number of mistakes, and we had squandered opportunities along the way. It was excellent on-the-job training, but there probably is a more efficient way of training entrepreneurs than by letting them make all the mistakes. Fortunately, good products make up for a lot of problems in an organization, and I think that was what happened in our case.

When Bob and I started looking around for business opportunities, we identified one that we thought would minimize the advantages of the established companies.

When Bob and I started looking around for business opportunities, we identified one that we thought would minimize the advantages of the established companies, such as Texas Instruments, Fairchild, and the others. This opportunity was one that would change the leverage. The semiconductor industry had gotten to the point by then where having large, low-cost assembly plants in Southeast Asia was very important competitively. But technology was capable of making a more complex chip than we were capable of defining. If you identified a complex circuit function, it tended to be unique; it might be used only once in every computer, so the unit volume did not allow you to amortize the design cost. The net result was that relatively simple circuits were still being made, even as the technology continued to evolve. We thought we saw in semiconductor memory an opportunity to make a product of almost arbitrary complexity that could be used in all digital systems, and that would change the leverage from low-cost assembly back to cleverness in processing silicon. We started our business on this idea.

Moore (left) and Noyce at Intel in the early seventies. Starting a company wasn't always easy.

Now, this was at a time when venture capital was at a peak. Bob Noyce called Arthur Rock and said, "Hey, we want to set up a new company. Would you help us raise the money?" Arthur said sure, and that was the commitment of our first round of financing. We wrote a single-page business plan. It was *very* general. It said we were going to work with silicon; we were going to do diffusion and other similar processes and make interesting products.

Then we started looking at technologies that would be appropriate for the business we were undertaking. In retrospect, I call it our "Goldilocks" technology strategy. We pursued three

In 1969 Intel's 106 employees pose in front of the original plant in Mountain View, California. Noyce stands in front at left and Moore at right. Work was already under way on the microprocessor, which was invented by Intel engineer Ted Hoff (standing at right behind Moore). The world's first commercial microprocessor, Intel's 4004, shown on the opposite page, was introduced in 1971.

different directions. One was a particular kind of bipolar transistor, called a Schottky bipolar, that was different from what was being used then. It turned out that the technology worked just beautifully; better than anyone could have expected. In fact, it worked so well, that competitors were able to copy it rapidly. That was technology that was too easy. We chose another technology for assembling a lot of memory chips in one package—flipping them over and doing an advanced type of assembly. We're still working on that one, 25 years later. That one was too hard. Fortunately, we also chose a third one—a new version of the MOS (metal oxide semiconductor) technology called silicon-gate MOS. Here the transistor's "gate" electrode that previously was made of a metal (usually aluminum)—the M in MOS—was replaced by a film of silicon that had several important advantages for device switching speed and packing density on the silicon wafer surface. And that one was just right. By concentrating on this one technology and focusing our attention on a couple of difficult problems with it, we were able to solve the problems and get on with it. But the established companies that were tending their main business and doing new process development on the side, didn't have time to focus on solving the problems and took several years to get going on it. Our initial estimate was that we had five years to grow big enough to prevent the existing companies from putting us out of business. In fact, we had seven years before the big companies got into our technology. Fortunately, very much by luck, we had hit on a technology that had just the right degree of difficulty for a successful start-up. This was how Intel began.

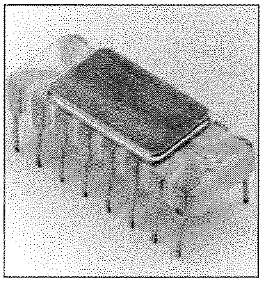
At Intel, we decided not to make the same kind of mistakes that we had made at Fairchild. At Fairchild, for example, we used industrial distributors to sell a good portion of our products—we sold them to a distributor; the distributor sold them to the final customer. We recorded the sale when the product went to the distributor. But in our business, prices only go down. The only question is whether they go down at 20 percent per year or at 80 percent per year. Once at Fairchild, for example, when we had our distributors well stocked with our products, Motorola introduced a competitive device at a significantly lower price. To match their prices, we would have had to reverse sales revenue we had already counted and to take a terrible hit on our profit-and-loss statement, which we didn't think we could afford. So we sat there and watched our market share deteriorate while that inventory sold out. We decided we wouldn't let this happen at Intel. We don't take credit for a sale when we sell a product to our distributor, but only when it has moved off his shelves to the final customer. This was a bit of "technology" that we had to sell to our accounting firm, because it hadn't been done previously. But it turned out to solve that particular problem very well. It is now standard industry practice.

From my own point of view, I had grown very frustrated running a laboratory at Fairchild. As the manufacturing group grew more competent technically, they were less willing to listen to the people in the laboratory as the experts. So when we came up with some new idea in the laboratory—for example, stable MOS devices—we had great difficulty transferring the detailed instructions to manufacturing. We were much more effective in transferring new technology to the spin-off companies than we were internally. To avoid that problem at Intel, and to promote maximum efficiency of transfer from development to manufacturing, we decided not to set up a separate laboratory. We've set up a variety of different kinds of mechanisms and organizations along the way to make the development-to-manufacturing transfer as efficient as possible, even at a sacrifice in efficiency of either the manufacturing or the development process individually. This has minimized spin-offs, because we design our development specifically to transfer into the factory; so we don't have the problem of developing technology and ideas that we have no place for. Technology transfer is always difficult. We have tried to minimize the need to transfer it.

From the beginning at Intel, we planned on being big. Since we had already been fairly successful at Fairchild, anything less successful in our new venture would have been a disappointment. So, at the very beginning we recruited a staff that had high potential and that we thought would be around to run the company for some time. This is an opportunity that many start-ups miss. There is no better chance to train managers than in a start-up, where they have the opportunity to see the entire company as it grows. It starts small and simple; one can see all the operations as they get bigger. I think that people looking at start-ups, venture capitalists in particular, ought to push very strongly not to squander the opportunity to develop management during that time period.

We also tried to minimize bureaucracy. When we started Intel, for example, each one of us took an area of technology as his own. And instead of purchase requisitions, we gave our engineers purchase *order* forms, so they could work with the equipment supplier directly and just hand the salesman a purchase order. This shocked some of the vendors, but it was a very effective, no-bureaucracy way of getting going. Unfortunately, we can't do that today any more, but at the time, when we were a fairly small company, it worked very well.

Another thing we had learned along the way was to raise money before we needed it. One thing you find out after a little bit of experience as an entrepreneur is that the bank will lend you money as long as you don't need it. You can sell stock as long as you really don't have to. With good advice from directors such as Arthur Rock, we have always had plenty of capital on hand, so that we haven't been hindered in our ability to raise more.



Intel's 4004 chip, which contained 2,200 transistors, was a 4-bit microprocessor and addressed 9.2 K of memory (on another chip). Although this computer-on-a-chip began the revolution in personal computers, the company missed the boat on getting into the PC business.

At Fairchild we had no idea that we needed an organization—that we had to set up a manufacturing department and an engineering department and a sales force. All these things sound logical, but they take a while to figure out. And one of the most important elements in an entrepreneurial organization is people management. There are a lot of things that I've learned very late in life about managing people, and if I could go back again to the beginning of Intel, I would do many things differently. For example, I have come to appreciate the value of regular one-on-one meetings with subordinates, where the subordinate controls the meeting agenda. Such sessions are very efficient for transferring information in both directions.

I suppose I can't end without bragging a bit about Intel. We just completed our 25th year; our revenue was $8.8 billion, and our earnings were over $2 billion. That puts us at least in the top 20 and maybe in the top 10 of the world's most profitable companies. We have steadily increased our lead as the largest semiconductor manufacturer and have more than 30,000 employees worldwide.

"What the heck would anyone want a computer for in his home?"

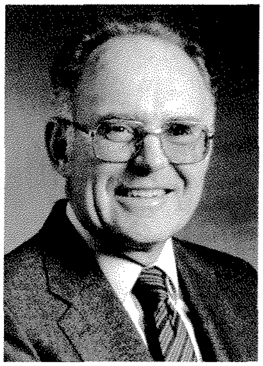
But there are some things I'm not quite so proud of that have come along with it. In 1984, for example, we hit a peak of 26,000 employees; in 1986 we were down to under 18,000. Laying off 8,000 employees is not a very pleasant task, and it's something I think could have been avoided had Intel management been a bit more careful and perceptive.

And I can look back at a few missed opportunities. Some we missed by default. I remember talking with venture capitalist Bill Davidow, when he worked for Intel, about an engineering workstation. Intel sold something we called a "development system," which was a special-purpose computer for the engineer. We imagined that the engineer in the future would have a single computer on his desk, and we talked about what it ought to be. But even though we talked about it, we were too darned busy doing other things, and we never got around to moving in that direction. So we missed that chance completely. I suppose I could also look at the PC as an opportunity we missed. Long before Apple, one of our engineers came to me with the suggestion that Intel ought to build a computer for the home. And I asked him, "What the heck would anyone want a computer for in his home?" (I still sometimes wonder, in spite of having a few of them.) The only example he could come up with was something for the housewife to put her recipes on. I could imagine Betty at the stove cooking, poking at her computer to read the recipe. It seemed ridiculous! Well, perhaps we didn't miss that opportunity after all, because we do make a profit out of the PC business—not by being in it, but by serving it. And that may be the best way.

We missed other opportunities by poor execution. One that really bothers me is that in 1985 we were driven out of the dynamic random access memory (DRAM) business, the business in which we had made our first significant profits and which had gotten us started as a successful company. But we were driven out partly because we didn't execute a couple of generations of products very well, and partly because Japanese dumping drove us out. Different economics for the Japanese companies allowed them to run their factories and sell their products far below cost. But it still bothers me that we couldn't compete successfully in a business we had created.

Most of what I learned as an entrepreneur was by trial and error, but I think a lot of this really could have been learned more efficiently.

Now, I don't mind missing an opportunity because we tried and failed. We took some fairly aggressive and not always successful steps toward producing computer products early in the company's life—for example, the 432. The 432 was probably the first 32-bit microprocessor; it was hardware designed to execute object-oriented software; the hardware and software were designed together and had many advanced features. At the time that we designed the system, the technology wasn't quite ready for such complexity, and in order to get all the functionality on the chip, we had to sacrifice performance. It ended up being so slow it could do hardly anything, and we had to abandon it. But at least it was an aggressive shot—one that we just didn't target correctly.



Another shot that misfired was digital watches. We were the first company in the liquid-crystal, digital-watch business. We hoped the watch was a path to a portable digital product that could be expanded to do much more than tell time. Other companies entering the eletronic-watch market drove prices through the floor. The business opportunity we saw was completely destroyed, if it ever really existed. I still have my $15-million watch, along with memories of Microma Watch, a division of Intel. It wakes me up in the morning; it has a good alarm system on it, and liquid-crystal displays last for at least 20 years.

In retrospect, there are a lot of things we could have done better along the way, but we did enough right to grow a fairly large company. The world has really been changing, too, during this time period. Industry here and abroad has enjoyed huge improvements in efficiency. For example, I mentioned earlier that we had 26,000 employees in 1984. We just passed 26,000 employees again during this last year, and the company is five times as big in revenue as it was then. The competition today is a lot stronger than it was in the past. A start-up company today probably can't afford the sort of on-the-job training that we could.

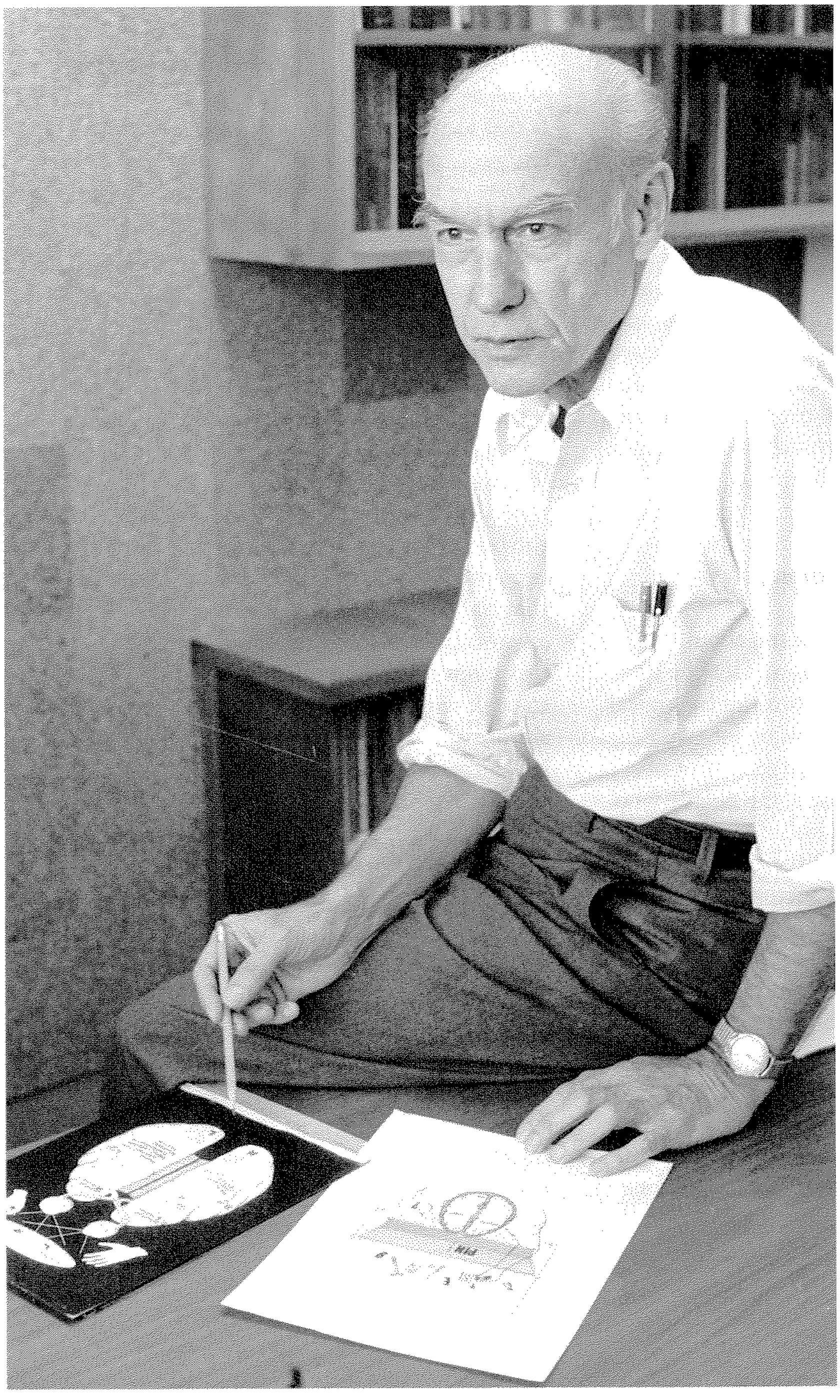
There is such a thing as a natural-born entrepreneur, for whom the entrepreneurial urge drives everything and who can make a business out of almost anything. But the accidental entrepreneur like me has to fall into the opportunity or be pushed into it. Then the entrepreneurial spirit eventually catches on. To me the opportunities to start a company are few and far between. Things have to line up right. I'm not the sort of entrepreneur who can just say, "I'm going to start a company. Let's look for an opportunity." In my entire career I think I've seen only about three ideas come by that I would consider a basis on which to try to start an enterprise. But starting a company is certainly exciting, and building a successful enterprise is satisfying and rewarding.

Most of what I learned as an entrepreneur was by trial and error, but I think a lot of this really could have been learned more efficiently. I think a place like Caltech could offer an opportunity to avoid the need for trial and error in a lot of this. Broadening the education to include some instruction in business—a little bit about finance and organizations—would certainly be useful, and I think a course in this direction would probably be a significant addition to the curriculum. But a technical education is probably the best start for an entrepreneur in a high-tech business.

And it's important to remember one other thing that is essential for any entrepreneurial organization: do what you do well. Look at other things as incremental opportunities, but don't change the basis of what you do well. For Caltech, what it does well is train the best scientists and engineers in the world. My advice to Caltech is this: help students a bit if they want to move in entrepreneurial directions, but don't change the basic nature of a Caltech education. ☐

Gordon Moore is chairman of the board of Intel Corporation. He is also chair of Caltech's board of trustees, elected last fall to succeed Ruben Mettler. This article is adapted from a talk he gave at Caltech last March at the groundbreaking ceremonies of the Gordon and Betty Moore Laboratory of Engineering, being built with a $16.8 million gift from the Moores.

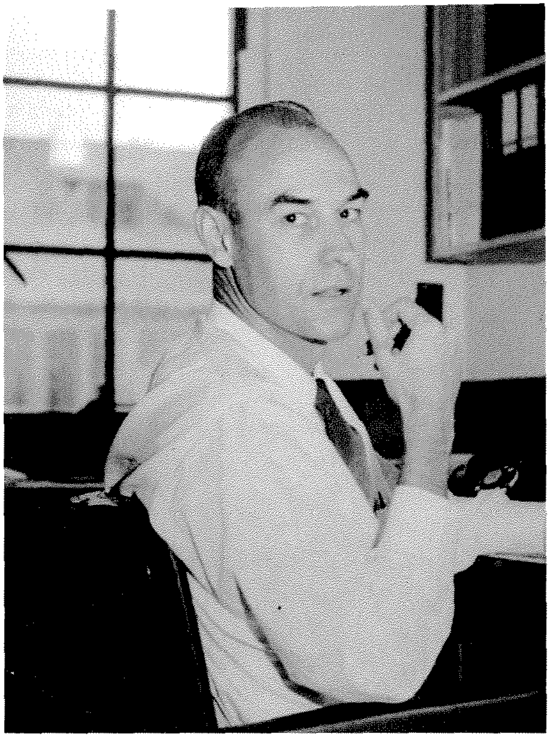
Roger W. Sperry
1914–1994



Roger W. Sperry, the Board of Trustees Professor of Psychobiology, Emeritus, died April 17, 1994, of complications associated with lateral sclerosis. He had been a member of the Caltech faculty since 1954, for most of that time the Hixon Professor of Psychobiology, and in 1981 won the Nobel Prize for his discoveries concerning the functional specialization of the two hemispheres of the brain. John M. Allman, the current Hixon Professor, chaired a memorial observance June 3 in the Beckman Institute Auditorium, during which several people who had known Sperry well spoke of the importance of his scientific work and his impact on his students, on his colleagues, and on society.

Norman Horowitz
Professor of Biology, Emeritus

I was Roger Sperry's oldest friend on the Caltech faculty. I first met him in 1951, when we were both on the program of a symposium that was held at Smith College in Northampton, Massachusetts. Roger's talk, which I can still remember, was truly brilliant, dazzling. I'm a geneticist; I'm not a neuroscientist, or a behavioral biologist, or a psychobiologist, but I could recognize a master at work. I would not have been surprised if someone had told me then that Roger would be one of the principal shapers of the modern view of how the brain works. In his talk, Roger demonstrated the capacity to design experiments that gave clean answers to interesting questions in one of the most difficult areas of



Roger Sperry at Caltech in 1954.

biology. His surgical skill and his imagination in designing tests of brain function were enormously impressive to me. He proved beyond a reasonable doubt in that paper that the many individual nerve fibers that make up the regenerating optic nerve in amphibians have separate chemical identities that determine where they make their connections as they grow back into the brain. He showed this by logical inference from his biological results, without performing any actual chemistry.

The second thing that struck me about Roger's lecture was its conclusion. Here he displayed a comprehension of the broader biological issues that made him almost unique for that time. I want to read the last two sentences of that paper. They may surprise some people who came to know Roger only in his later years. "Finally, to return to our original theme, it would seem that with the foregoing picture of the developmental processes, almost no behavior pattern need be considered too refined or too complicated for its detailed organization to be significantly influenced by genetic factors. The extent to which our individual motor skills, sensory capacities, talents, temperaments, mannerisms, intelligence, and other behavioral traits may be products of inheritance would seem to be much greater on these terms than many of us had formerly believed possible."

When I got back to Caltech I knew what I had to do. At that time we were searching for the first Hixon Professor of Psychobiology. I spoke to George Beadle, who was then division chairman, and to Anthonie Van Harreveld, who was chairman of the Hixon search committee, and suggested that Roger be invited for some lectures. Roger was invited; he came, he conquered, and the rest is history.

Ronald Meyer
Professor of Developmental and Cell Biology
University of California, Irvine

(Meyer came to Caltech as a graduate student in psychobiology, earning his PhD in 1974. In his memorial talk he described some of his experiences in Sperry's diverse lab and traced the development of Sperry's theory of chemoaffinity—work that began in the 1930s and 1940s. Although it had been postulated earlier that growing nerve fibers used chemical clues to find their way, no one had been able to discover any evidence to support that idea, and current studies were leading in the opposite direction. At the University of Chicago, Paul Weiss concluded that all nerve fibers were created equal, that the pattern of nerve connections was unimportant, and that the important thing in innervation was learning. Sperry was Weiss's graduate student at Chicago and set out to test this idea.)

Initially he was interested in its functional aspects, so one of the first experiments he did was to cross some of a rat's nerves so that they innervated the wrong muscles. Then he looked at the behavior of those rats very carefully; it hadn't been done all that carefully before. He observed that the basic reflex behavior of these animals was always abnormal; the rat could learn to adapt to this screwed-up leg, much as you might be able to walk with a cast, but the function was really abnormal. What he concluded from this was that specific nerves did mediate certain responses, and that connection was important.

Around this time a number of reports appeared showing that in lower vertebrates, such as frogs and salamanders, you could cut the optic nerve and it would grow back. The folks who did this interpreted the results along the lines Paul Weiss had championed at the time—that the animals were simply learning to adapt to abnormal connections. Then Roger did what was perhaps his most famous experiment: he rotated a frog's eye 180°, then cut the optic nerve and let it grow back. (This was not particularly easy. I tried it a few times as a graduate student and gave up.) Now, normally you can tell what the frog sees by using a little wire with a fly on the end. If you put it in front of the frog, the frog will try to eat it; if you put it in back of the frog, the frog turns around and then tries to eat it. The frog has very good visual localization. What

Roger found when he turned the eye upside down was that, when you put the lure behind the frog, the frog would try to eat it. And if you put the lure in front of it, the frog would turn around. It saw the world completely turned around by 180°. Fortunately frogs are very stupid. They never learned how to adapt to this; if you didn't feed them forcibly, he found, they would simply starve to death.

The conclusion Roger drew from this experiment was that nerve fibers from the eye must have grown back to specific locations within the brain, in an area called the tectum. These regenerating fibers were reestablishing the map from the retina onto the tectum. In spite of the fact that they had started out upside down, they managed to straighten themselves up and go back to their original targets. He theorized that during development a position-dependent differentiation occurred in the retina so that different nerve fibers from different regions acquired specific properties. In the tectum, nerve fibers in different regions also acquired specific properties, and there was some way in which the nerve fibers growing from the retina could then locate those particular regions in the tectum and find the right place. This is the heart of chemoaffinity.

He wrote 16 or so papers extending this finding to a number of different nerve systems. His work evidently made quite an impression at the University of Chicago, and they duly acknowledged it by denying him tenure. If there is a top-ten list of the worst tenure decisions in the world, Roger must be on it.

If there is a top-ten list of the worst tenure decisions in the world, Roger must be on it. But Chicago's loss was Caltech's gain.

But Chicago's loss was Caltech's gain. When he came here he performed what was probably one of his next most important experiments. Together with Domenica Attardi, he developed a method for visualizing optic fibers while they were growing into the brain of a goldfish. He found that if he removed part of the eye, fibers from the remaining part of the retina would grow into the tectum; they would even grow past the wrong regions and selectively terminate in the correct regions. Furthermore, en route to the tectum they would take specific paths to lead up to the correct angle to approach the tectum. On the basis of this work, in the early 1960s Roger published an elaborated version of his chemoaffinity hypothesis, spelling it out with particular reference to the visual system: that during development, retinal cells acquire a position-dependent differentiation, probably in a gradient fashion; that the tectum acquires a similar gradient distribution of molecules; and that fibers from the retina or elsewhere could selectively navigate through this myriad of cues in a selective fashion and innervate the particular targets.

Some of us kept on with this work, but Roger's interest shifted toward the higher-order functions in the brain, such as consciousness and perception. Why, you might wonder, didn't he go after the molecules to prove that his chemoaffinity hypothesis was correct? He felt that this was a waste of time, that he had already solved the problem. And, really, from his perspective he had. He had asked the question: does the specificity of nerve connections determine function? And he had shown that it does. How do they do it? By a developmentally regulated process that gives them labels. He wasn't interested in going after the molecules, and, since no one has yet definitely proven what they are in the visual system, I'd say that was a wise career decision on his part. He was interested in how nerve structure determines behavior and function, and he went on to examine issues at the higher end. How does nerve structure determine perception in the cortex? How much is that wired in? What was the basis of consciousness? It didn't really represent that much of a change of interest.

(Meyer went on to describe some of the arguments and skepticism about Sperry's chemoaffinity hypothesis that arose during the 1970s and 1980s, and speculated that they might have been responsible for his not receiving the Nobel Prize for this work. Meyer made a distinction between what he called Sperry's special theory of chemoaffinity, based on the expanded research into the visual system, which Meyer called 90 percent correct, and the general theory of chemoaffinity—the big picture of how the formation of nerve connections is regulated. There is no serious questioning of the general theory today, and it's accepted as a theory nearly in the same way that evolution is just a "theory." Meyer concluded with the observation that, since the theory is now so generally accepted, it's easy to forget that there was ever even a question of whether nerves had specific identities and that we have Roger Sperry to thank for telling us that they do.)

Brenda Milner
Dorothy J. Killam Professor of Psychology
Montreal Neurological Institute, McGill University

(Milner, according to Allman, is one of the great figures in the field of neuropsychology, known for her classical studies of the role of the medial temporal lobe structures, including the hippocampus, in memory. These studies have had enormous influence on modern neurobiology. She spoke of Sperry's contributions to the study of human cognition, beginning with some personal reminiscences, recounting a time in 1972 when they

both happened to be in Cambridge, England, where Sperry was receiving an honorary degree; Milner described the ceremony.)

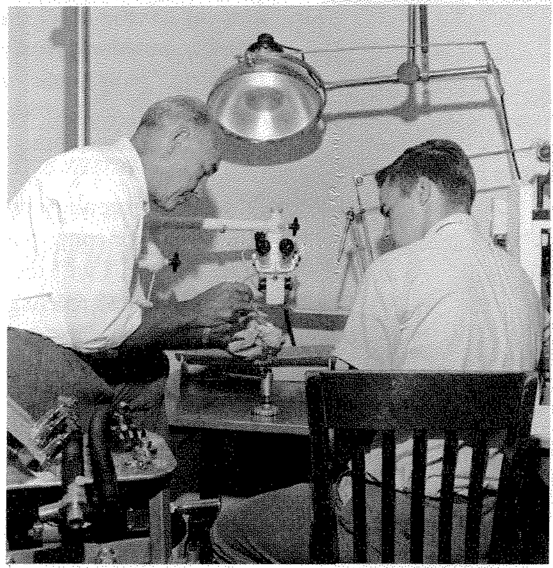
The Cambridge orator, who had to give an account of everything in Latin, had a lot of trouble with the frogs and the motor connections because apparently the Romans had the same word for nerve and muscle. So he had a little difficulty making this distinction in elegant Latin; fortunately we also had a translation. He began by pointing out that this is a person with many careers; that it is given to very few scientists to make major contributions in more than one field—contributions that will have enormous impact on the work of future scientists in those fields. If you want to explore complex issues and problems, you have to ask yourself the right questions. And Roger was so good at asking the right questions, the important ones, and he had little use for what he thought were trivial questions. He could be very impatient about things he thought were really no longer issues and always wanted to look ahead at some big question that was waiting to be tackled. How did he do it? He did it, first of all, not by expensive equipment, but by very simple means with very elegant methods. And, the orator added, he did it with dexterous hands, with skilled hands—this is the meticulous surgeon, the meticulous scientist. And above all, he did it with a mind that was dedicated to looking for natural causes, with the inquiring mind and the investigative look. The orator concluded by saying that if you have this approach and these qualities, then you can open up a "broad path into a closed field." And I think the broad path into the closed field is what we saw in the results and consequences of Roger's work on the split brain.

How did the field look before this work, and what difference did Roger make? For years there had been a number of neurologists and psychologists in different countries who had been gathering compelling evidence of the important contributions of the right, nondominant hemisphere to intellectual, cognitive tasks. These contributions particularly involved visuo-spatial skills, the representation of visual patterns, and so on. Here there was real evidence that the right hemisphere was not merely competent, but that it was more competent than the left, language-dominant, side. Most of this evidence came from the study of patients with circumscribed brain lesions. This is evidence by subtraction—the richness and the capacity of the person's intellect was in some way diminished by reduction of language, memory loss, loss in visuo-spatial perceptual abilities, or by some change in personality. There's a diminution of an entity.

Commissurotomies (cutting the corpus callosum, which connects the two hemispheres) had been done before, but no one had discovered that anything was wrong. What was new with Roger's approach was that he told us how to examine these patients, how to address questions to each of the separated hemispheres. We wonder now how others could have missed something so glaring. Roger believed very strongly that you learn good lessons from behavioral studies in animals. He had learned from all his work on commissurotomies in monkeys and cats that the two sides of the brain can function amazingly independently in carrying out various tasks. He took the next logical step and asked whether this occurs in humans. And of course, the logical, commonsense answer was, why not? Illogically, however, the feeling was that, no, consciousness is one, a human brain can be diminished by a lesion but it can't be split. But Roger followed the path of science and applied to humans the methods he had applied to other species. (It's much more difficult when you can't just cut the optic tract; he had to develop methods for channeling information into one hemisphere and out from the same hemisphere.) Then he presented the world with this notion of two minds in one head, two organizations of neurons capable of thinking. Of course, they have the brain stem in common, exerting some general unifying influence on the two halves, but cognitively they remain distinct. The right hemisphere doesn't talk, or talks very little, but it thinks for itself. Roger demonstrated that by addressing the question directly to one side of the brain, he could elicit one kind of behavior, and a contrasting behavior when the same question or task was addressed to the other side. Encountering patients who exhibited these two consciousnesses coexisting gave us a fresh understanding of the logical consequences of separating two equally complex organizations of nerve cells and pathways.

Then he presented the world with this notion of two minds in one head, two organizations of neurons capable of thinking.

Roger characterized the right side as being more holistic, the left as more analytic. Perhaps the popular press took this over too wholeheartedly and talked about "educating the right hemisphere," and so forth. In the person with intact hemispheres, I think, you can only educate an organism, the whole person, but the demonstration of the coexistence of two entities, two thinking minds, in the patients with divided commissures was incredibly compelling. It gave a great boost to our field. This field, now fashionably called cognitive neuroscience, owes an enormous debt to these insights of Roger Sperry.



Sperry in his lab in 1974.

I hung on his every word, of which there were not very many.

Dr. Joseph Bogen
Clinical Professor of Neurological Surgery, University of Southern California
Adjunct Professor of Behavioral Neuroscience, University of California, Los Angeles

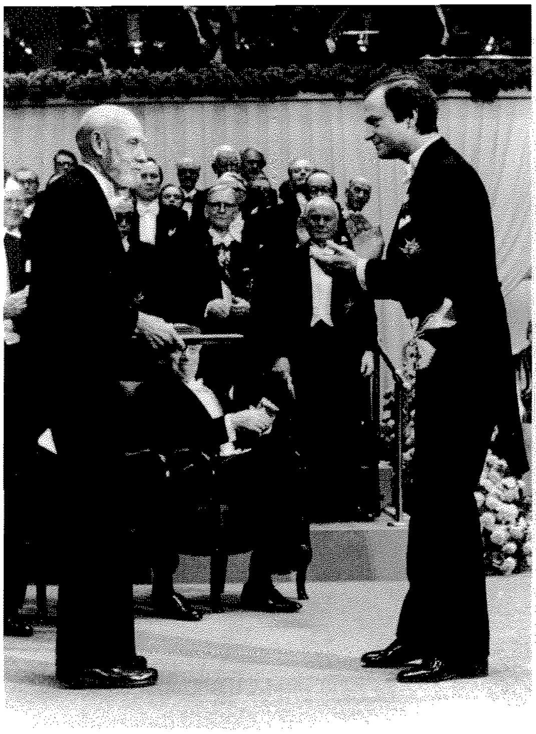
(Bogen, who was stimulated by Sperry's experiments to propose severing the corpus callosum to limit epileptic seizures in humans, worked with Sperry over several decades in studying these split-brain patients. He contributed a few anecdotes that he hoped would "reflect my impression of Roger's genius, his style, and his demeanor." He also referred again to the elegance and simplicity of Sperry's early experiments with frogs' leg muscles and rotated eyes—in the latter the demonstration of the frog's vision using a fly on the end of a stick or wire. "That took real genius—to think of this really simple way.")

The first time that I saw Roger was over 40 years ago, when he gave his famous Sigma Xi lecture in the Athenaeum. He lucidly explained and astoundingly illustrated the discriminative ability of cats that had various alterations of their visual cortices. I was not in a position at that time to arrive at an informed evaluation of that talk, but perhaps I can convey to you how I felt by referring to a time when I was trying to educate my elder daughter about wine—how to tell good wine from not-so-good. I gave her some wine to try; she swirled it and sniffed it and rolled some around in her mouth and swallowed it. Then she said, "Nobody has to tell me *that's* good." That's how I felt about Roger's talk that day, and that's the way I've felt about Roger Sperry for the ensuing 40 years.

In 1955 I came to Caltech as a graduate research assistant to Anthonie Van Harreveld. Van Harreveld's lab and office were on the third floor of Kerckhoff, just down the hall from Roger's. I spent quite a bit of time down at Roger's end of the hall, because those split-brain cats were mind-boggling. They made a profound impression on everybody who saw them. It was the most influential experiment that I ever saw or ever knew about or heard about before or since. It set the course of my life.

Three years later I came back to Van Harreveld as a postdoc, and during that time Roger and I became better acquainted. It was necessary for me to go up and down the hall several times a day, and usually when I would go by Roger's office the door would be open and he would be sitting there reading or maybe doodling on a pad. Sometimes he'd just be leaning back in his chair with his feet on the desk, staring into space. Then one day he disappeared, into the lab. Not long after that we had a biology seminar, at which Sperry and Attardi presented their work on optic nerve regeneration. The slides were sections of goldfish brain that were stained a deep bluish-black, except for the regenerating fibers. These fibers, sneaking their way through the jungle of the optic chiasm, up around the optic lobe and then abruptly diving into their intended destinations, were stained a brilliant pink. It was spectacular. It has always seemed regrettable to me that when this work was finally published in the *Journal of Experimental Neurology*, the pictures were reproduced in black and white. That was in 1963, five years after the abstract first appeared in the *Anatomical Record* in 1958. Such a long delay was not unusual for Roger. He could keep a paper on his desk for a long time for a variety of reasons. One of them was that he liked to see how the follow-up experiments were going to turn out. The idea was that when you went back and wrote the final form of the first paper, the discussion would have some sensible things to say. It seemed to me he thought of everything. I hung on his every word, of which there were not very many. . . .

Around 1960, when I was working at County Hospital, I wrote an essay about epilepsy, entitled "A Rationale for Splitting the Human Brain." I brought it up to Roger, and he had a number of recommendations, the first of which was, "Maybe you should change the title." Also, he told me to look up some papers by Akelaitis, which I did, and it turned out that the callosal surgery performed by Van Wagenen 20 years before had actually turned out better than was then, in



Roger Sperry receives his Nobel Prize from Sweden's King Carl Gustav in Stockholm on December 10, 1981.

1960, the prevailing medical opinion. This suggestion led to a joint effort of nearly 30 years.

At about the same time, one of my projects involved some behavioral experiments with rats, the results of which I really didn't understand. I thought that if anybody could explain it, it would be Roger. So I brought my data up to Caltech. He had some helpful comments to make, and then he said, "If you keep working with that, you might very well come up with something dramatic." Roger Sperry's facility for coming up with something dramatic time after time in a variety of contexts was not simply because he kept in mind the value of a decisive counterintuitive result. And it was not just because he was a highly creative and self-disciplined presenter, and not just because he was an expert experimentalist. Essential, I believe, was the fact that Roger was among the deepest, the most profound neurothinkers of our time.

Theodore Voneida
Department of Neurobiology
Northeastern Ohio Universities College of Medicine

(Voneida met Sperry as a visiting graduate student in 1958, working with split brain cats in Sperry's lab, and returned to Caltech as a postdoc in the early sixties. "This was a rich and exciting time for me, and it really established the pattern of my research, which has continued in the same vein for over 30 years. . . . I never failed to be impressed with his tremendous insight into questions of research. One could spend an hour discussing an idea with Roger and leave the discussion knowing whether or not it was worth pursuing." Voneida remembered fondly Sperry's sense of humor and the Sperrys' great parties, where Roger served his famous punch, "one glass of which would strip away at least 200,000 dendritic spines.")

I do not intend to review all or even a few of Roger's major contributions. Others have done that. Rather, I will restrict my comments to the area of his most recent interest, namely the concept of mind, or consciousness, as an emergent property of brain function. Like most emergent properties the mind is a unique entity, and as such in no way resembles the structures from which it arises, namely the hundreds of millions of neurons constituting the central nervous system. An important point, however, is that while the mind is not the same as the central nervous system, it is dependent for its existence on the central nervous system. This may appear obvious, but Roger always emphasized that this concept of the mind should not be construed in any way as supportive of dualism. A second point, and one that may not at first be obvious, is that the mind continuously feeds back onto the central nervous system and this feedback results in a constantly changing nervous system. The feedback aspect is very important and often is not recognized. The cognitive revolution, according to Roger, from an ethical standpoint might equally well have been called a values revolution, through which "the old value-free, strictly objective, mindless, quantitative, atomistic descriptions of materialist science are being replaced by accounts that recognize the rich, irreducible, varied, and valued emergent macro and holistic properties and qualities in both human and nonhuman nature."

He goes on to tell us that "subjective human values become the most critically powerful force shaping today's civilized world, the underlying answer to current global ills and the key to world change." In short, the cognitive revolution, in Sperry's view, represents a possible last hope for survival, through which two powerful groups—science and religion—might find a common ground for cooperation in dealing with problems such as increasing population pressures, ecological destruction, and global warming. This concept of the mind as a single unifying force was generated in great part during his retirement years, and may prove to be one of his most significant contributions, though it is difficult to say that about a person who has made so many significant contributions.

In an article entitled "Science and the Problem of Values," written in 1972, he wrote, "The

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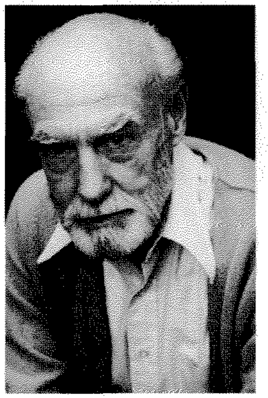


1981

prime hope for tomorrow's world lies not in outer space or improved technology, but rather in a change in the kind of value belief systems we live and govern by. The more strategic way to remedy global conditions such as poverty, population explosion, energy and pollution, is to go after the social-value priorities directly in advance, rather than waiting for the value changes to be forced by worsening conditions. Trends toward disaster in today's world stem mainly from the fact that, while man has been acquiring new, almost godlike, powers of control over nature, he has continued to wield these same powers with a relatively shortsighted, most ungodlike, set of values, rooted on the one hand in outdated biological hangovers from evolution in the Stone Age, and on the other in various mythologies and technologies based on little more than faith, fantasy, and intuition." He concludes that the human brain is today the dominant control force on our planet: what moves and directs the brain of man will, in turn, largely determine the future from here on.

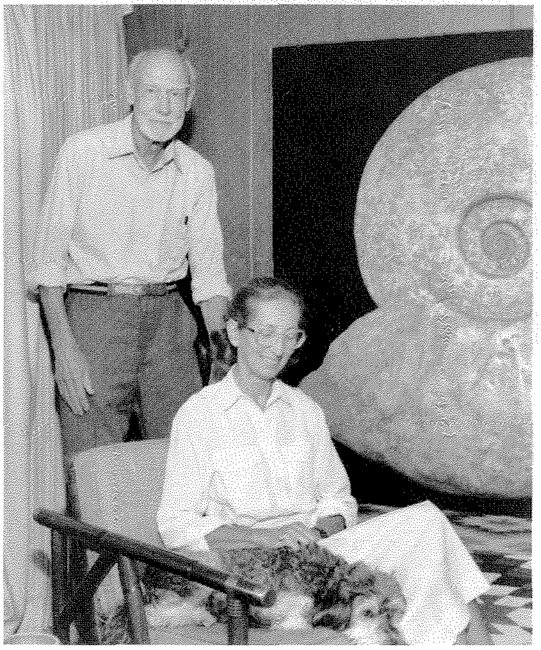
(Voneida noted Sperry's discussions of his ideas with Nobel (physiology or medicine) Laureate Rita Levi-Montalcini, who proposed an international meeting at Trieste. This grew into two international meetings, one in December 1992 and the other in November 1993, at which a 12-point Declaration of Human Duties—with the subtitle "A Code of Ethics of Shared Responsibility"—was drafted. When approved and signed, it will be delivered to the United Nations, where it will serve as a corollary document to the Declaration of Human Rights. Voneida presented Sperry's work at these conferences and is also active in an ongoing series of conferences in Japan that are working toward establishing the Network University of the Green World, in which students worldwide will communicate on topics related to human values.)

This has been a necessarily brief overview of some of the impact and the promise that Roger's recent ideas have had. They continue to be heard, and there is no question that they will continue to have an enormous impact on our thinking about mind, consciousness, and values well into the 21st century and beyond.

From my own point of view I'd like to say that my own life was very greatly enriched by having known and having worked with this quiet, reserved man with a grand, wry sense of humor. To me he was a superb teacher, a wonderful and generous colleague, and a dear friend. I owe him more than I'm able to say. And the best I can do is to continue studying and disseminating what I have learned from him to the widest possible audience, with the hope that humankind will open its collective mind to a new way of thinking and a new set of values before it's too late.

Seymour Benzer
James G. Boswell Professor of Neuroscience, Emeritus
Crafoord Laureate

Roger Sperry, as well as having influence on the world as a whole, also had a great influence on me and my career of the last almost 30 years. It's a well-kept secret that I spent two years in Roger's laboratory here at Caltech from 1965 to 1967. We never worked together and we never published together, so there's no fossil record of those events. I came to Caltech for a change of career, switching from molecular biology to an interest in neurobiology. I had done that once before, many years earlier, when I switched from physics to molecular biology by coming and working in Max Delbrück's laboratory. Delbrück's laboratory and Sperry's laboratory had a great deal in common; not only were they headed by towering intellectual figures, but each lab was what is now referred to in industry as an "incubator"—an institution that forms a sort of protective cover over young entrepreneurs who are trying to establish their own businesses. A dozen of them are put together in the same building so they can share facilities and learn from each other the ropes of making a career in the business world. So in both Delbrück's and Sperry's labs there was a motley crew of characters working on many different things and, in both cases, many of



Roger and Norma Sperry collected fossil ammonites, some of which were exhibited, along with some of Sperry's artwork, at the memorial service. This 70-million-year-old specimen, however, displayed in their living room in 1987, was too large to move. The Sperrys found it in the Mexican Rio Grande.

the people who emerged have had very distinguished careers.

OK, fine. We'll count on solving the biochemical basis of memory in the fall and there'll be plenty of time meanwhile to decide what to look at next.

I had been at Purdue working on the structure of the gene and how genetic information gets transcribed and translated in bacteria. It struck me that if people have different genes, then their nervous systems might not develop in exactly the same way. That might account for their different behavior, which was beginning to puzzle me, especially with respect to my two children, who, from day one, behaved very differently. On looking into the literature, I encountered Roger Sperry's ideas and experiments on chemospecificity of neurons and its role in wiring up the nervous system, and his idea that genetics was behind the mechanism. Since I still had a fond attachment to Caltech from the Delbrück experience, I asked Max whether Sperry's lab would be a good place to go. His response was, "You could do worse." When I approached Sperry with the idea, he apparently also asked Max about *me*, and I suspect he got the same answer.

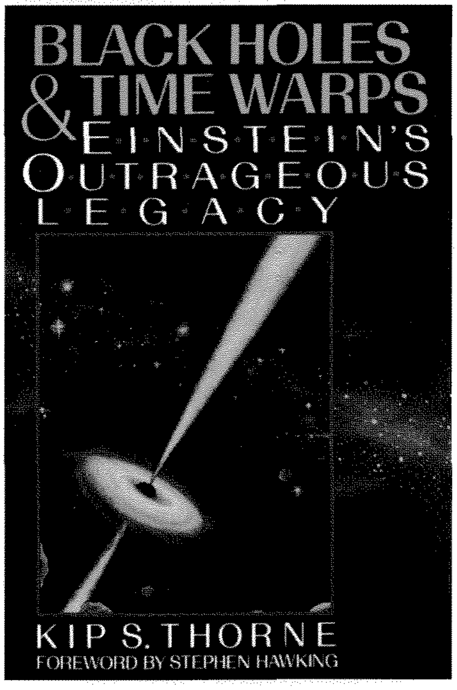
Last night, I dug out some of the original correspondence with Sperry in 1965. It goes as follows:

> Dear Dr. Sperry: Thanks for making my visit to your group such a pleasant and informative one. It seems silly for me to look any further for the best place to learn the brain business. Would you permit me to spend my sabbatical in your laboratory starting in September? Best regards.
>
> Dear Dr. Benzer: Yes, we'd all be happy indeed to have you spend your sabbatical here. I should probably warn you that you may find our Caltech group rather small and lacking in much of the exciting diversity you might see in a larger setting. This might be compensated in part by visiting connections with UCLA, which has become much closer to Caltech in recent years through new freeway developments. I'm not sure what you'd like to do, if anything, in the way of actual experimentation that might involve research space and facilities. I assume you mainly want a kind of home base from which to read, think, watch, talk, and learn, rather than a place to engage in specific projects. But mainly we should probably be sure you don't wish to use a special brand of computerized bevatron that we don't have. Others here, including Max Delbrück and Jean Weigle, are equally enthusiastic about the idea.
>
> Dear Dr. Sperry: Thank you for your yes. Please rest assured that I've little interest in either computers or bevatrons, let alone their combination. Nor do I intend to spend a year on the freeway, which to me is a foretaste of hell. My ambition is to work seriously with you on a specific problem. While a solution to the memory riddle in one year may be too much to ask [I was pretty naive], I do hope for an opportunity to obtain a working knowledge of brain splitting, psychological testing, and the associated arts.
>
> Dear Dr. Benzer: OK, fine. We'll count on solving the biochemical basis of memory in the fall and there'll be plenty of time meanwhile to decide what to look at next. All best wishes and we'll be looking forward to your arrival.

(Benzer described some of the personalities and the work going on in the incubator of Sperry's lab, which he called "a real zoo, both in animal terms and intellectual terms.")

All the activities in the lab had one thing in common: none of them had anything to do with genes. To me this was the wide open field through which one could build a big road. So I went around the corner to Ed Lewis [who is now the Thomas Hunt Morgan Professor of Biology, Emeritus] and got some fruit flies and some test tubes, which were not to be found in Sperry's lab, and went to work. And I've been doing that ever since. So Roger's contribution was not unlike Delbrück's: a role model for creative thinking; an attitude of skepticism, which served as a goad to do something that would make an impression; but at the same time the generosity to provide a supportive environment in which each of us could learn from the other weirdoes in the lab, try out crazy ideas, and develop our own thing. For that I will always be indebted to, and will always remember, Roger Sperry. □

Books



W. W. Norton & Company
$30.00
619 pages

by John Preskill

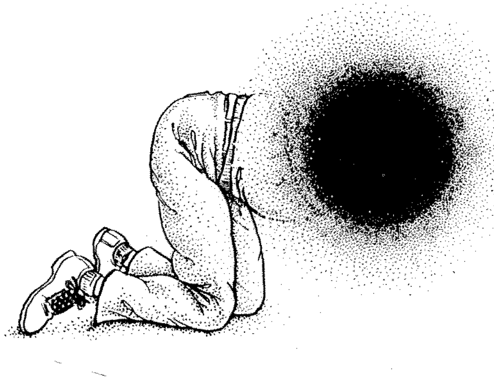
It is dangerous to ask a scientist to review a book on science that is intended for a lay audience, particularly if the subject of the book is close to the reviewer's own specialty, as in this case. So I may not be the best qualified to judge how effectively this book reaches its intended readers. Nevertheless, I can say with confidence that Kip Thorne's account of the "outrageous" consequences of the general theory of relativity is one of the best popularizations of science that I have read. It is surely the best by far of the many popular books on relativity theory.

An essential part of the appeal of the book is its subject, for the general theory of relativity is arguably the very greatest triumph of the human intellect, and nothing better illustrates the profound beauty of the natural laws that govern the universe. Thorne brings a unique set of qualifications to the demanding task of explaining relativity to the layperson. First, few active researchers can match his deep grasp of the relevant science. Second, he is a gifted teacher whose pedagogical skills have been well honed by guiding a generation of Caltech students through the subtleties of relativity. Third, he writes prose that is lucid and absorbing. Finally, he has an insider's view of the exciting developments, stretching back to the early sixties, that are the focus of most of the book.

Rarely has a world-class scientist shown such devotion in the preparation of a nontechnical book; Thorne worked on the manuscript, on and off, for some 15 years. It traces the history of relativity theory from its origins in the early 20th century and documents the subsequent struggle to understand the theory and its implications. Though Thorne is not a historian, he recounts this history with meticulous attention to detail. In particular, he conducted taped interviews with 47 scientists who were directly involved in the developments that he describes. For the earlier history, he relies more heavily on secondary sources, but he has also studied many of the original research articles. (In the case of Einstein's papers, it was necessary for Thorne to read many of them in Russian, because he does not read German, and they have never been translated into English.) The sources are well documented in the notes at the back of the book.

I consider the book nontechnical in the sense that it contains no equations (aside from a few in the notes). This is not to say that it is easy reading. A reader unfamiliar with the material will need to work hard to fully absorb the nearly 600 pages. But that dedicated reader will be amply rewarded. This book contains the real stuff; Thorne has resisted to a remarkable extent the temp-

A wormhole is a "short circuit" in space that connects distantly separated points, and enables someone who travels through it to reach a remote location virtually instantaneously.



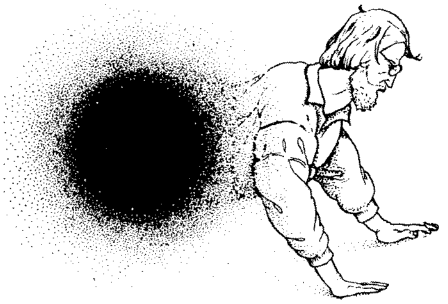
tation to water down the scientific content for the sake of ease of presentation. The reader who takes the trouble to master this book will have achieved a grasp of many subtle and elusive concepts. Sadly, the same cannot be said of most science writing, and certainly cannot be said of most popular accounts of relativity theory. Considering Thorne's high standard of scientific accuracy, the book is amazingly readable.

There is much more here than a remarkably lucid description of the science. A very important part of what makes the book enjoyable are the portrayals of many fascinating personalities. Perhaps the three most interesting are John Wheeler, the American theoretical physicist who was Thorne's mentor and who coined the term "black hole" in 1967; Stephen Hawking, the British theorist whose brilliant contributions to the theory of black holes in the early seventies are vividly related here; and Yakov Borisovich Zel'dovich, the Soviet astrophysicist. I especially enjoyed the account of the career of Zel'dovich, who was a key figure in the design of the Soviet hydrogen bomb, and who then funneled his enormous energy and intellect into astrophysics beginning in the late fifties. By 1964, he had built the strongest theoretical astrophysics team in the world. Thorne's many contacts with Zel'dovich and other Soviet physicists have enabled him to offer intriguing insights into the contrast between the Soviet and American styles of doing science.

While captivating figures such as Zel'dovich add spice to the book, the real main character is the truly outrageous black hole, the central topic of 8 of the 14 chapters (and also of a long prologue). One of the most outrageous features of a black hole is that it is the only macroscopic object with so simple a structure; a black hole is composed of nothing but pure warped spacetime. And a black hole surrounded by empty space is an essentially unique object; once its mass and rate of rotation are known, its structure is completely determined. (In John Wheeler's apt phrase, "A black hole has no hair.") Equally outrageous is a black hole's appetite for destruction: astronauts who foolishly enter a black hole can never escape; rather they will be inextricably drawn to a "singularity," where their bodies will be torn apart by enormous gravitational forces.

Thorne chronicles the evolution of the concept of the black hole, from abstract mathematical idealization to concrete physical object. The astronomer Karl Schwarzschild first discovered what we now call a black hole as a mathematical solution to Einstein's gravitational field equation (while he was serving in the German army on the Russian front during World War I). But for decades most physicists stubbornly resisted the preposterous implications of Schwarzschild's solution. This included Einstein himself, who wrote a regrettable (and quite incorrect) paper in 1939 arguing that black holes cannot exist. Not until

Like his colleague Kip Thorne, John Preskill is professor of theoretical physics at Caltech and equally familiar with the "outrageous"—his current work concerns the quantum mechanical properties of black holes. Preskill holds an AB (1975) from Princeton and PhD (1980) from Harvard, and has been a member of the Caltech faculty since 1983.



Kip Thorne crawls through a hypothetical, very short wormhole. (Illustration by Matthew Zimet from *Black Holes and Time Warps.*)

the 1960s did the black hole concept firmly take hold in the community of physicists and astronomers. Thorne nostalgically recounts how the "golden age" of black hole research opened up around 1964 as the first hints emerged that black holes have no hair. The golden age lasted some 10 years. During this period came, among other things, the discoveries that black holes can spin and vibrate, and that they can exchange energy with the matter that surrounds them. (Many of Thorne's own students made fundamental contributions during this period.) These insights ushered in a new discipline, relativistic astrophysics, and led to the (presumed) detection of black holes by astronomers and experimental physicists as X-ray emitting binary star systems, and as quasars emitting extraordinarily powerful radio signals.

Of particular interest to the Caltech community is the chapter of the book concerning gravitational-wave detection and the LIGO project. Gravitational waves are ripples in the geometry of spacetime that are expected to be copiously created in rare cataclysmic astronomical events, such as a collision of two black holes. These waves are exceedingly difficult to detect because the events that produce strong signals typically occur only at great distances from us. LIGO (for Laser Interferometer Gravitational-Wave Observatory) is an ambitious effort by a joint Caltech/MIT team to construct a facility that, it is hoped, will directly detect gravitational waves for the very first time. The apparatus must be extraordinarily sensitive, and although construction of LIGO has now begun, the success of the enterprise is still far from assured.

It was Thorne himself who proposed in 1976 that Caltech initiate a program aimed at detection of gravitational waves, and he recalls here his own struggle at that time to evaluate the risk and potential payoff of such a program. He also recounts the sometimes painful evolution of the project from the free-wheeling style of its early days to the much more regimented style that became necessary as it neared the construction stage. Thorne is at his best contemplating the scientific potential of LIGO; his passion for the prospect of viewing the universe in a whole new way shines through in this chapter.

The most outrageous implications of general relativity are the subject of the final chapter of the book, which is called "Wormholes and Time Machines." The topic here is more speculative than in the earlier chapters, and is described more from Thorne's own personal perspective. I suspect that some readers will also find it to be the most interesting chapter, as it offers a glimpse of the cutting edge of current research on an intrinsically fascinating topic.

Thorne recalls how Carl Sagan prevailed upon him to invent a system of interstellar transport for Sagan's novel, *Contact*. Thorne suggested wormholes. A wormhole is a "short circuit" in space that connects distantly separated points, and enables someone who travels through it to reach a remote location virtually instantaneously. Sagan's request inspired Thorne and his students to investigate whether an "arbitrarily advanced civilization" could in principle create such wormholes. (This remains an open question.) Thinking about wormholes eventually led Thorne to the startling insight that a wormhole can be turned into a time machine by moving one end of the wormhole in an appropriate way. This subtle trick is explained here in detail and with exceptional clarity. There follows a sober and careful discussion of the implications. Thorne concludes that whether time machines can exist is really a question about the (still poorly understood) laws that govern quantum gravity. He reports that his own gut feeling is that the laws of physics forbid time machines—but we still don't know for sure.

During the 30 or so years of Kip Thorne's scientific career, the study of gravitational physics has been radically transformed. In the early sixties, general relativity was widely perceived (with some justification) as a beautiful but highly abstract and complicated theory that made very little contact with the real world. Since then, advances in technology and in theoretical understanding have changed that perception forever. Today, observational astronomers and experimental physicists routinely seek and find evidence for black holes in binary star systems and at the centers of galaxies. To a great extent, this book is the story of how this transformation took place, as seen by a central participant. Above all, it is a story of human reason at its best, following the tortuous path toward an undertanding of the deepest truths.

I believe that many Caltech students, faculty, and alumni will enjoy this book. A dedicated reader will learn a great deal of physics. But even if some readers don't have the patience to absorb the details of all of the arguments, they will still delight in the insights into the scientific process, the vivid anecdotes, and the sense of adventure inherent in the difficult struggle to grasp the fundamental laws of Nature. ☐

Books
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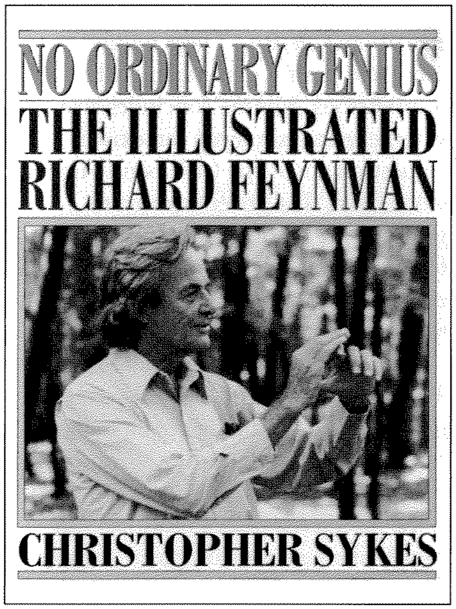


W. W. Norton & Company
$29.95
272 pages

by Shirley Marneus

One sunny noontime a few years back, I had a lunch I will never forget with Dick Feynman. I think we both had the *soup du yesterday.* We sat outdoors on the west terrace of Chandler Dining Hall and admired the spotted bark peeling off the sycamore trees. The Theater Arts Program was rehearsing a trio of one-act plays written by a physics graduate student, Greg Tomko-Pavia, and Dick was playing The Professor (who might also be The Devil) in one of the scripts, entitled *The Subduction.* Astrid Howard, a graduate student in geology, was directing it. Dick really enjoyed working with students in this slightly unorthodox way. He knew that his participation encouraged their creative efforts, and he also was aware that his on-stage appearance would attract a larger audience to support their work.

We had continued talking into the midafternoon, roaming from the plays to just about everything else, when Dick said (and this is what makes the lunch memorable), "Shirley, you're just like me; you wanna know how everything works!" Well, I was flattered (!!!), to put it mildly. But, then, who am I to argue with a bona fide genius, especially one who was *No Ordinary Genius?*

Which just happens to be the title of the book I have before me. As the subtitle indicates, there are pictures—more than a hundred. I would have liked more—I would have liked one of every person who contributed—and I would have liked more candid snapshots of Feynman himself. *No Ordinary Genius* is a recent addition to the spate of publications about the Caltech legend, tireless raconteur, enthusiastic teacher, spirited drummer, amateur artist, and the only Nobel Prize winner to appear regularly in Caltech theater productions. He was an adventurer, a supportive and loving parent, an unconventional humorist, a good friend, and a "curious character" (as he liked to style himself); he was a serious man who loved to laugh, who delighted in play but who worked with uncompromising discipline, and who had so much natural dignity that he did not need to be, and indeed could not be, further dignified by any title or honor.

Meanwhile, back on that autumn afternoon at Chandler, he was, at least in part, correct: I *do* like to know how things work. So the way this book works is this: On the cover is a color photograph of Richard Feynman. He is obviously engaged by what he is describing. Intelligence and humor glow around his face; his hands, fine-boned and elegant, are poised, apparently arrested by the camera in the process of creating a new sign language right before your eyes. The appeal is instant and irresistible.

You pick the book up, flip it open, and start reading—there's some pretty interesting stuff here (stuff was one of Feynman's favorite words). Before you get very far, you discover that you aren't really reading so much as you are *listening.* What's more, you are listening to the distinct voices of individual people: Feynman's sister, Joan, and his children, Carl and Michelle; Al Hibbs chimes in, as do Zorthian the artist, an Air Force general, a musician, Feynman's favorite model, Hans Bethe, David Goodstein, students, fellow adventurers, loyal partners, and even some colleagues who express reservations and who qualify their admiration. And all of these people are *talking*. The aural illusion is so strong that you almost turn your head to look from speaker to speaker, to see who has just walked into the room to join the conversation.

The way this book works is very much the way a conversation does—give, take, toss in two cents' worth—thrust, parry, add on, toss out—contradict, reinforce, reiterate—all talk pivoting around one particular person. Then, into the arena walks the subject himself and joins in. He tells about what he feels, comments on the others' comments, gives you his point of view, answers, jokes, ignores. These observations accumulate, assume shape, acquire depth; wonderful irregularities emerge from the matrix, little quirks and contradictions spurt out, and yet, all of it is of a piece, of a person.

Aristotle told us centuries ago how, on a stage, in a drama, we learn about a character. We know a man, Aristotle claimed, first by what he says about himself, then by what others say about him, and finally by what he does. All three expository techniques are required for a complete representation. This book works, too, by Aristotelian devices of character exposition. As a script, it's short on plot developments, but it's very long on compelling character.

On the title page Christopher Sykes is not billed as "author," but, rather, as "editor." Sykes is a documentary filmmaker, and his editorial technique is cinematic. He has recorded interviews and then arranged and rearranged excerpts into meaningful sections, establishing rhythms, accentuating affinities, discerning order. He builds to revelations, pulls back, then goes for a tight close-up, and he has executed these maneuvers with careful attention to original meaning. Even removed from context and intercut with other voices, each speech retains its integrity.

Sykes has drawn his text from three programs featuring Feynman that he produced for the BBC: "The Pleasure of Finding Things Out" (1981), "The Quest for Tannu Tuva" (1988), and "No Ordinary Genius" (1993). He also produced a series of six short programs called "Fun to Imagine" (1983), which have not been incorporated into the book; nor, I believe, have they been shown on television in the United States. The three longer programs have all been shown here on public broadcasting channels. It's important to note that Sykes has not merely transcribed the television program "No Ordinary Genius" to create this book. He has combined all three into a new, much more complex (and satisfying) souvenir of a life.

This is, I think, a real service to the reading public. These words can now be read and reread, looked up, quoted. They need not be caught on the wing between commercials. For the many who have not seen, and probably will not be able to see, these shows, the book provides a wonderful opportunity to encounter the remarkable Richard Feynman. The conversational format invites participatory reading (which is the best sort, I think), as if the reader's ideas are somehow incorporated into it. Sykes writes in his introduction: "I remember Feynman as always smiling, and he made me wish I had been a scientist. I think he should be a household name, and that is why I have compiled the book."

It seems like a good reason to me. ☐

Shirley Marneus has been director of Caltech's Theater Arts Program (TACIT) since 1970. She had left behind years of work in theater and television in search of a new cast of characters, which, clearly, she has found.

Random Walk

LIGO Groundbreaking

Construction of LIGO, the Laser Interferometer Gravitational-Wave Observatory, began with groundbreaking ceremonies in Hanford, Washington, on July 6. This is one of two sites (the other is in Livingston, Louisiana) for the joint Caltech/MIT project, funded by the National Science Foundation. The two L-shaped facilities, with arms four kilometers long, will operate in tandem to try to detect gravitational waves. For more on LIGO's mission, see the review of Professor Kip Thorne's book, *Black Holes and Time Warps: Einstein's Outrageous Legacy,* beginning on page 39. For still more, read the book.

Honors and Awards

Yaser Abu-Mostafa, associate professor of electrical engineering, is one of 20 to be honored with a $10,000 W. M. Keck Foundation Award for Engineering Teaching Excellence.

Richard Andersen, the Boswell Professor of Neuroscience, will receive the W. Alden Spencer Award from Columbia University's College of Physicians and Surgeons. The $1,000 prize honors his "highly original contributions to research in neurobiology."

Seymour Benzer, the Boswell Professor of Neuroscience, Emeritus, has

Random Walk
continued

been granted a McKnight Senior Investigator Award by the McKnight Endowment Fund to support research by two postdoctoral fellows on a fruit-fly gene that may provide insights into such human disorders as Alzheimer's.

Roy Gould, the Ramo Professor of Engineering, will receive the James Clerk Maxwell Prize in Plasma Physics. The $5,000 award, sponsored by Maxwell Laboratories Incorporated and presented by the American Physical Society, honors "contributions to the advancement and dissemination of the knowledge of properties of highly ionized gases."

Steve Mayo, assistant professor of biology, has been named a 1994 Searle Scholar and given a three-year grant of $180,000 to continue his research in automated protein design.

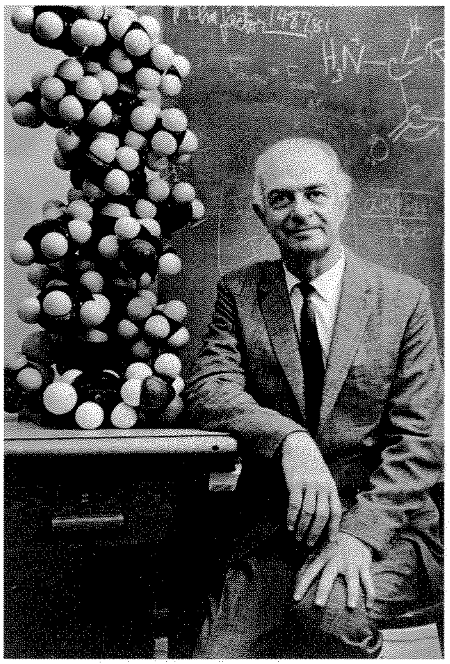
Carver Mead, the Moore Professor of Engineering and Applied Science, has received the Robert Dexter Conrad Award—the Navy's highest honor for scientific achievement—for his "enormous impact on very large scale integration and neural network technology."

Wallace Sargent, the Bowen Professor of Astronomy, has been selected to receive the 1994 Catherine Wolfe Bruce Gold Medal from the Astronomical Society of the Pacific for his achievements in the field of astronomy.

Erin Schuman, assistant professor of biology, has won a $240,000 John Merck Scholarship in the Biology of Developmental Disabilities in Children for her studies of how memory is stored.

Ahmed Zewail, the Pauling Professor of Chemical Physics, will this month receive the *Bonner Chemiepreis*, from the Chemical Institutes in Germany for his work in femtochemistry.

Linus Pauling 1901–1994



Linus Pauling, Nobel Laureate and professor of chemistry, emeritus, died August 19 at the age of 93 at his Big Sur home.

Pauling had been a faculty member at Caltech for 37 years. After receiving his BS in chemical engineering in 1922 from Oregon State College (now Oregon State University), Pauling entered Caltech as a graduate student. He earned his PhD in chemistry in 1925 and joined the Caltech faculty the next year. As professor of chemistry (from 1931), he served as chair of the Division of Chemistry and Chemical Engineering from 1936 to 1958 as well as director of the Gates and Crellin Laboratories of Chemistry. He was named professor of chemistry, emeritus, in 1971.

Pauling had, however, already left Caltech in 1964. He went on to positions at the Center for the Study of Democratic Institutions in Santa Barbara, at UC San Diego, and at Stanford. In 1973 he established the Linus Pauling Institute of Science and Medicine in Palo Alto to concentrate on the chemistry of life and on challenges in medicine, an interest that had begun with his work on sickle cell anemia in the 1950s. His theories on the beneficial health effects of Vitamin C made his name familiar to a wide public.

But it was his earlier work in structural chemistry—the determination of the structures of molecules through X-ray diffraction and electron diffraction—that brought him legendary status at Caltech and in the scientific community. In 1939, many of his discoveries and insights led to *The Nature of the Chemical Bond*, one of the most influential scientific books of the 20th century. In the mid-1930s, Pauling became interested in biological molecules, and in the late 1940s discovered the alpha helix as the basic structure of proteins. He won the Nobel Prize in chemistry in 1954 for his work on the nature of the chemical bond and its use in understanding the structure of such complex substances as proteins and antibodies.

Pauling campaigned passionately against the atmospheric testing of nuclear weapons during the 1950s; his efforts were credited as significant in bringing about the nuclear test ban treaty of 1963. They also won him his second Nobel—the Nobel Peace Prize—in 1962. Pauling is the only person to have won two unshared Nobel Prizes.

Pauling returned to campus in recent years for celebrations of his 85th and 90th birthdays. The latter was the occasion for a scientific symposium in February 1991, the first in a series celebrating Caltech's centennial. A memorial service for Pauling will be held on the Caltech campus in early October and will be covered in a subsequent issue of *E&S*.

Were Comet Shoemaker-Levy 9 still around to stand trial, it would need a very slick attorney indeed to avoid doing time for assault with a deadly weapon. This true-color Hubble Space Telescope image of a shotgunned Jupiter's southern hemisphere shows seven impact sites. They are, from left to right: the E/F complex (barely visible on the planet's edge), asterisk-shaped H, tiny N, Q1, small Q2, R, and the D/G complex on the right limb. Planet Earth would fit within the enormous bruise surrounding the D/G complex. The livid blotches are impact ejecta, probably in aerosol form. The smallest features visible are less than 200 kilometers across. The Great Red Spot can also be seen. North is to the upper right.