Richard Feynman and the Connection Machine

W. Daniel Hillis

Citation: Phys. Today **42**(2), 78 (1989); doi: 10.1063/1.881196
View online: http://dx.doi.org/10.1063/1.881196
View Table of Contents: http://www.physicstoday.org/resource/1/PHTOAD/v42/i2
Published by the American Institute of Physics.

RICHARD FEYNMAN AND THE CONNECTION MACHINE

In his last years, Feynman helped build an innovative computer. He had great fun with computers. Half the fun was explaining things to anyone who would listen.

W. Daniel Hillis

One day in the spring of 1983, when I was having lunch with Richard Feynman, I mentioned to him that I was planning to start a company to build a parallel computer with a million processors. (I was at the time a graduate student at the MIT Artificial Intelligence Lab). His reaction was unequivocal: "That is positively the dopiest idea I ever heard." For Richard a crazy idea was an opportunity to prove it wrong—or prove it right. Either way, he was interested. By the end of lunch he had agreed to spend the summer working at the company.

Richard had as much fun with computers as anyone I ever knew. His interest in computing went back to his days at Los Alamos, where he supervised the "computers," that is, the people who operated the mechanical calculators. There he was instrumental in setting up some of the first plug-programmable tabulating machines for physical simulation. His interest in the field was heightened in the late 1970s when his son Carl began studying computers at MIT.

I got to know Richard through his son. Carl was one of the undergraduates helping me with my thesis project. I was trying to design a computer fast enough to solve commonsense reasoning problems. The machine, as we envisioned it, would include a million tiny computers, all connected by a communications network. We called it the Connection Machine. Richard, always interested in his son's activities, followed the project closely. He was skeptical about the idea, but whenever we met at a conference or during my visits to Caltech, we would stay up until the early hours of the morning discussing details of the planned machine. Our lunchtime meeting on that spring day in 1983 was the first time he ever seemed to believe we were really going to try to build it.

Richard arrived in Boston the day after the company was incorporated. We had been busy raising the money, finding a place to rent, issuing stock and so on. We had found an old mansion just outside the city, and when Richard showed up we were still recovering from the shock of having the first few million dollars in the bank. No one had thought about anything technical for months. We were arguing about what the name of the company should be when Richard walked in, saluted and said, "Richard Feynman reporting for duty. OK, boss, what's my assignment?"

The assembled group of not-quite-graduated MIT students was astounded. After a hurried private discussion ("I don't know, you hired him . . ."), we informed Richard that his assignment would be to advise on the application of parallel processing to scientific problems. "That sounds like a bunch of baloney," he said. "Give me something real to do."

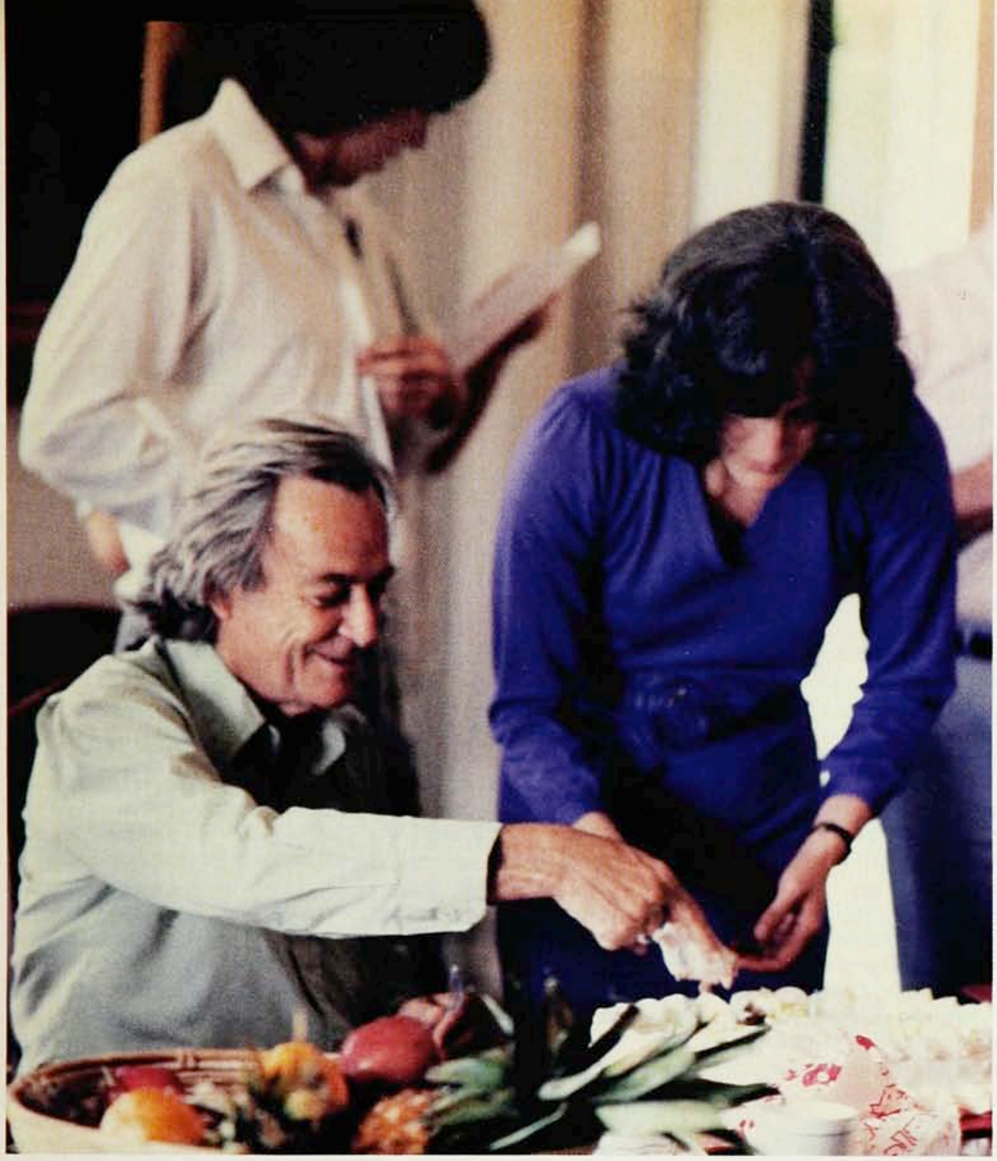
So we sent him out to buy some office supplies. While he was gone, we decided that the part of the machine we were most worried about was the router that delivered messages from one processor to another. We were not entirely sure that our planned design would work. When Richard returned from buying pencils, we gave him the assignment of analyzing the router.

The machine

The router of the Connection Machine was the part of the hardware that allowed the processors to communicate. It was a complicated object; by comparison, the processors themselves were straightforward. Connecting a separate wire between every pair of processors was totally impractical; a million processors would require 10^{12} wires. Instead, we planned to connect the processors in the pattern of a 20-dimensional hypercube, so that each processor would only need to talk directly to 20 others. Because many processors had to communicate simultaneously, many messages would contend for the same wire. The router's job was to find a free path through this 20-dimensional traffic jam or, if it couldn't, to hold the message in a buffer until a path became free. Our question to Feynman was: Had we allowed enough buffers for the router to operate efficiently?

In those first few months Richard began studying the router circuit diagrams as if they were objects of nature. He was willing to listen to explanations of how and why things worked a certain way, but fundamentally he

W. Daniel Hillis is the founder of Thinking Machines Corporation in Cambridge, Massachusetts, and the inventor of the firm's Connection Machine.



At the lunch buffet at Thinking Machines Corporation in Cambridge, Massachusetts, Richard Feynman happily avails himself of the assistance of company president Sheryl Handler.

preferred to figure everything out himself. He would sit in the woods behind the mansion and simulate the action of each circuit with pencil and paper.

Meanwhile, the rest of us, happy to have found something to keep Richard occupied, went about the business of ordering the furniture and computers, hiring the first engineers and arranging for the Defense Advanced Research Projects Agency to pay for the development of the first prototype. Richard did a remarkable job of focusing on his "assignment," stopping only occasionally to help wire the computer room, set up the machine shop, shake hands with the investors, install the telephones and cheerfully remind us of how crazy we all were. When we finally picked the name of the company, Thinking Machines Corporation, Richard was delighted. "That's good. Now I don't have to explain to people that I work with a bunch of loonies. I can just tell them the name of the company."

The technical side of the project was definitely stretching our capacities. We had decided to simplify things by starting with only 64 000 processors, but even then the amount of work to be done was overwhelming. We had to design our own silicon integrated circuits, with processors and a router. We also had to invent packaging and cooling mechanisms, write compilers and assemblers, devise ways of testing processors simultaneously and so on. Even simple problems like wiring the boards together took on a whole new meaning when you were working with tens of thousands of processors. In retrospect, if we had had any understanding of how complicated the project was going to be, we would never have started.

'Get these guys organized'

I had never managed a large group before, and I was clearly in over my head. Richard volunteered to help out. "We've got to get these guys organized," he told me. "Let me tell you how we did it at Los Alamos."

It seems that every great man has a certain time and place in his life that he takes as a reference point ever after: a time when things worked as they were supposed to and great deeds were accomplished. For Richard, that time was at Los Alamos during the Manhattan Project. Whenever things got "cockeyed," Richard would look back and try to understand how now was different from then. Using this formula, Richard decided we should pick an expert in each area of importance to the machine—software, packaging, electronics and so on—to become the "group leader" of that area, just as it had been at Los Alamos.

Part two of Feynman's "Let's Get Organized" campaign was a regular seminar series of invited speakers who might suggest interesting uses for our machine. Richard's idea was that we should concentrate on people with new applications, because they would be less conservative about what kind of computer they would use. For our first seminar he invited John Hopfield, a friend of his from Caltech, to give us a talk on his scheme for building neural networks. In 1983, studying neural networks was about as fashionable as studying ESP, so some people considered Hopfield a little crazy. Richard was certain he would fit right in at Thinking Machines.

What Hopfield had invented was a way of constructing an "associative memory," a device for remembering patterns (see the article by Haim Sompolinsky, PHYSICS TODAY, December, page 70). To use an associative memory, one trains it on a series of patterns—for example, pictures of letters of the alphabet. Later, when the memory is shown a new patterns, it is able to recall a similar pattern it has seen in the past. A new picture of the letter *A* will "remind" the memory of another *A* it has seen before. Hopfield figured out how such a memory could be built from devices functionally similar to biological neurons.

Not only did Hopfield's method seem to work; it seemed to work particularly well on the Connection Machine. Feynman figured out the details of how to use one processor to simulate each of Hopfield's neurons, with the strength of each connection represented as a number in the processor's memory. Because of the parallel nature of Hopfield's algorithm, all the processors could be used concurrently with 100 percent efficiency; the Connection Machine would thus be hundreds of times faster than any conventional computer.

An algorithm for logarithms

Feynman worked out in some detail the program for computing Hopfield's network on the Connection Machine. The part that he was proudest of was the subroutine for computing a logarithm. I mention it here not only because it is a clever algorithm, but also because it is a specific contribution Richard made to the mainstream of computer science. He had invented it at Los Alamos.

Consider the problem of finding the logarithm of a fractional number between 1 and 2. (The algorithm can be generalized without too much difficulty.) Feynman observed that any such number can be uniquely represented as a product of numbers of the form $1 + 2^{-k}$, where k is an integer. Testing for the presence of each of these factors in a binary representation is simply a matter of a shift and a subtraction. Once the factors are determined, the logarithm can be computed by adding together the precomputed logarithms of the factors. The algorithm fit the Connection Machine especially well because the small table of the logarithms of $1 + 2^{-k}$ could be shared by all the processors. The entire computation took less time than doing a division.

Concentrating on the algorithm for a basic arithmetic operation was typical of Richard's approach. He loved the details. In studying the router he paid attention to the action of each individual gate, and in writing the program he insisted on understanding the implementation of every instruction. He distrusted abstractions that could not be directly related to the facts. When, several years later, I wrote a general-interest article on the Connection Machine for *Scientific American*, he was disappointed that it left out too many details. He asked, "How is anyone supposed to know that this isn't just a bunch of crap?"

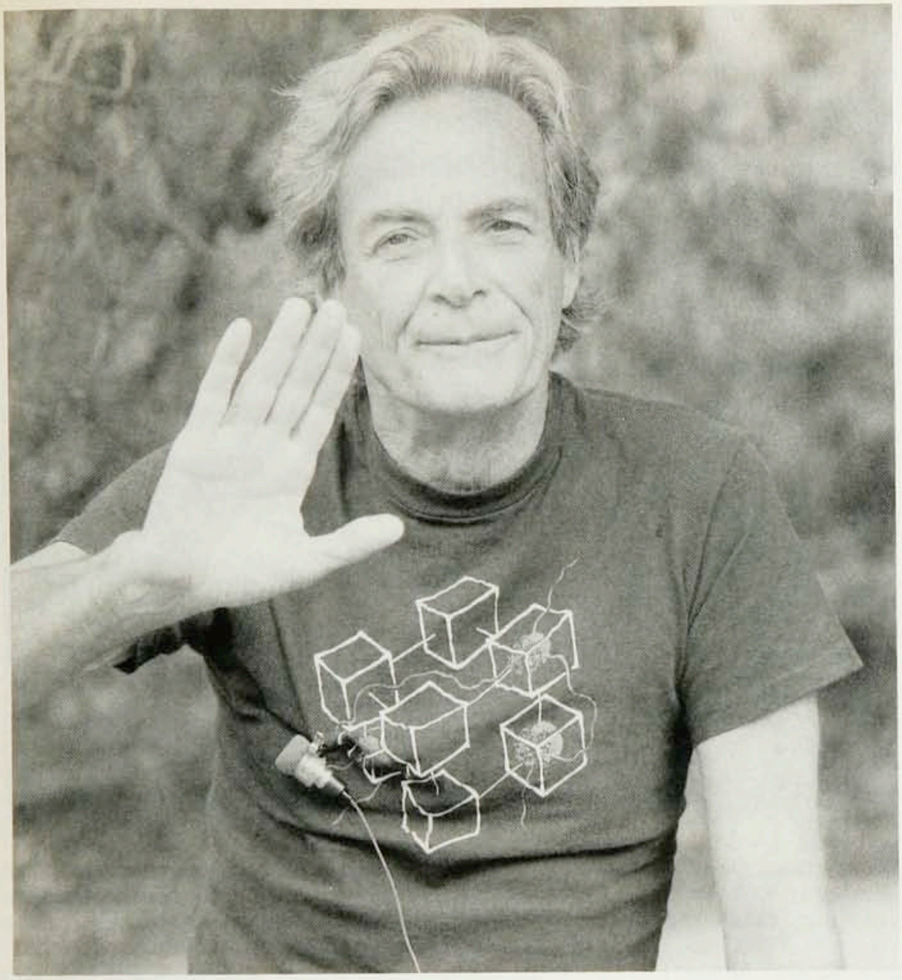
Feynman's insistence on looking at the details helped us discover the potential of the machine for numerical computing and physical simulation. We had thought that the Connection Machine would not be efficient at "number crunching," because the first prototype had no special hardware for vectors or floating-point arithmetic. Both of these were "known" to be requirements for number crunching. Feynman decided to test this assumption on a problem he was familiar with in detail: quantum chromodynamics.

Quantum chromodynamics is the presently accepted field theory of the strongly interacting elementary particles in terms of their constitutent quarks and gluons. It can, in principle, be used to compute the mass of the proton (in units of the pion mass). In practice, such a computation might require so much arithmetic that it would keep the fastest computers in the world busy for years. One way to do the calculation is to use a discrete four-dimensional lattice to model a section of space–time. Finding the solution involves adding up the contributions of all the possible configurations of certain matrices at the links of the lattice, or at least some large representative sample. (This is essentially a Feynman path integral.) What makes this so difficult is that calculating the contribution of even a single configuration involves multiplying the matrices around every loop in the lattice, and the number of loops grows as the fourth power of the lattice size. Because all these multiplications can take place concurrently, there is plenty of opportunity to keep all 64 000 processors busy.

To find out how well this would work in practice, Feynman had to write a computer program for quantum chromodynamics. Because BASIC was only computer language Richard was really familiar with, he made up a parallel-processing version of BASIC in which he wrote the program. He then simulated the operation of the program by hand to estimate how fast it would run on the Connection Machine.

He was excited by the results. "Hey Danny, you're not gonna believe this, but that machine of yours can actually do something *useful*!" According to Feynman's calculations, the Connection Machine, even without any special hardware for floating-point arithmetic, would outperform a machine that Caltech was building explicitly for quantum chromodynamics calculations. From that point on, Richard pushed us more and more toward looking at numerical applications of the machine.

By the end of that summer of 1983, Richard had completed his analysis of the behavior of the router, and much to our surprise and amusement, he presented his answer in the form of a set of partial differential equations. To a physicist this may seem natural, but to a computer designer it seems a bit strange to treat a set of Boolean circuits as a continuous, differentiable system. Feynman's router equations were written in terms of



Feynman at Thinking Machines wears the corporate T-shirt that bears a schematic representation of the firm's Connection Machine, which he helped design.

variables representing continuous quantities such as "the average number of 1 bits in a message address." I was much more accustomed to inductive proof and case analysis than to taking the time derivative of "the number of 1's." Our discrete analysis said we needed seven buffers per chip; Feynman's differential equations suggested we only needed five. We decided to play it safe and ignore Feynman.

The decision to ignore Feynman's analysis was made in September, but by the following spring we were up against a wall. The chips we had designed were slightly too big to manufacture, and the only way to solve the problem was to cut the number of buffers per chip back to five. Because Feynman's equations claimed we could do this safely, his unconventional methods of analysis started looking better and better to us. We decided to go ahead and make the chips with the smaller number of buffers.

Fortunately, Feynman was right. When we put together the chips, the machine worked. The first program run on the machine was John Horton Conway's Game of Life, in April 1985.

Cellular automata

The Game of Life is an example of a class of computations that interested Feynman: cellular automata. Like many physicists who had spent their lives going to successively lower levels of subatomic detail, Feynman often wondered what was at the bottom. One possible answer was a cellular automaton. The notion is that the space–time continuum might ultimately be discrete, and that the observed laws of physics might simply be large-scale consequences of the average behavior of tiny cells. Each cell could be a simple automaton that obeys a small set of rules and communicates only with its nearest neighbors—like the points in the lattice calculation for quantum chromodynamics. If the universe in fact works this way, there should be testable consequences, such as an upper limit on the density of information per cubic meter of space.

The notion of cellular automata goes back to John von Neumann and Stanislaw Ulam, whom Feynman had known at Los Alamos. Richard's recent interest in the subject was aroused by his friends Ed Fredkin and Stephen Wolfram, both of whom were fascinated by cellular automata as models of physics. Feynman was always quick to point out to them that he considered their specific models "kooky," but like the Connection Machine, he considered the subject crazy enough to put some energy into.

There are many potential problems with cellular automata as a model of physical space and time—for example, finding a set of rules that gives relativitistic invariance at the observable scale. One of the first problems is just making the physics rotationally invariant. The most obvious patterns of cellular automata, such as a fixed three-dimensional grid, have preferred directions along the grid axes. Is it possible to implement even Newtonian physics on a fixed lattice of automata?

Feynman had a proposed solution to the anisotropy problem that he attempted (without success) to work out in detail. His notion was that the underlying automata,

Consultants Feynman and Stephen Wolfram at Thinking Machines in 1986. Both were particularly interested in using the Connection Machine to do cellular-automata calculations.

rather than being connected in a regular lattice like a grid or a pattern of hexagons, might be randomly connected. Waves propagating through this medium would, on average, propagate at the same rate in every direction.

Cellular automata started getting attention at Thinking Machines in 1984 when Wolfram suggested that we should use such automata not as a model of nature, but as a practical approximation method for simulating physical systems. Specifically, we could use one processor to simulate each cell with neighbor-interaction rules chosen to model something useful, like fluid dynamics. Wolfram was at the Institute for Advanced Study in Princeton, but he was also spending time at Thinking Machines.

For two-dimensional problems there was a neat solution to the anisotropy problem. It had recently been shown that a hexagonal lattice with a simple set of rules gives rise to isotropic behavior on the macroscopic scale. Wolfram did a simulation of this kind with hexagonal cells on the Connection Machine. It produced a beautiful movie of turbulent fluid flow in two dimensions. Watching the movie got all of us, especially Feynman, excited about physical simulation. We all started planning additions to the hardware, such as support for floating-point arithmetic, which would make it possible to perform and display a variety of simulations in real time.

Feynman the explainer

In the meantime, we were having a lot of trouble explaining to people what we were doing with cellular automata. Eyes tended to glaze over when we started talking about state transition diagrams and finite-state machines. Finally Feynman told us to explain it like this:

> We have noticed in nature that the behavior of a fluid depends very little on the nature of the individual particles in that fluid. For example, the flow of sand is very similar to the flow of water or the flow of a pile of ball bearings. We have therefore taken advantage of this fact to invent a type of imaginary particle that is especially simple for us to simulate. This particle is a perfect ball bearing that can move at a single speed in one of six directions. The flow of these particles on a large enough scale is very similar to the flow of natural fluids.

This was a typical Feynman explanation. On the one hand, it infuriated the experts who had worked on the problem because it did not even mention all of the clever problems that they had solved. On the other hand, it delighted the listeners because they could walk away with a real understanding of the calculation and how it was connected to physical reality.

We tried to take advantage of Richard's talent for clarity by getting him to criticize the technical presentations we made in our product introductions. Before the commercial announcement of the first Connection Machine, CM-1, and all of our subsequent products, Richard would give a sentence-by-sentence critique of the planned presentation. "Don't say 'reflected acoustic wave.' Say *echo*." Or, "Forget all that 'local minima' stuff. Just say there's a bubble caught in the crystal and you have to shake it out." Nothing made him angrier than making something simple sound complicated.

Getting Richard to give advice like that was sometimes tricky. He pretended not to like working on any problem that was outside his claimed area of expertise. Often, when one of us asked for him advice, he would gruffly refuse with, "That's not my department." I could never figure out just what his department was, but it didn't matter anyway, because he spent most of his time working on these "not my department" problems. Sometimes he really would give up, but more often than not he would come back a few days after his refusal and remark, "I've been thinking about what you asked the other day and it seems to me. . . ." This worked best if you were careful not to expect it.

I do not mean to imply that Richard was hesitant to do the "dirty work." In fact he was always volunteering for it. Many a visitor at Thinking Machines was shocked to see that we had a Nobel laureate soldering circuit boards or painting walls. But what Richard hated, or at least pretended to hate, was being asked to give advice. So why were people always asking him for it? Because even when Richard didn't understand, he always seemed to understand better than the rest of us. And whatever he understood, he could make others understand as well. Richard made people feel like children do when a grown-up first treats them as adults. He was never afraid to tell the truth, and however foolish your question was, he never made you feel like a fool.

The charming side of Richard helped people forgive him for his less charming characteristics. For example, in many ways Richard was a sexist. When it came time for his daily bowl of soup, he would look around for the nearest "girl" and ask if she would bring it to him. It did not matter if she was the cook, an engineer or the president of the company. I once asked a female engineer who had just been a victim of this treatment if it bothered her. "Yes, it really annoys me," she said. "On the other hand, he's the only one who ever explained quantum mechanics to me as if I could understand it." That was the essence of Richard's charm.

A kind of game

Richard worked at the company on and off for the next five years. Floating-point hardware was eventually added to the machine, and as the machine and its successors went into commercial production, they were being used more and more for the kind of numerical simulation problems Richard had pioneered with his quantum chromodynamics program. Richard's interest shifted from the construction of the machine to its applications. As it turned out, building a big computer is a good excuse for talking with people who are working on some of the most exciting problems in science. We started working with physicists, astronomers, geologists, biologists, chemists—each of them trying to solve some problem that couldn't have been solved before. Figuring out how do such calculations on a parallel machine required understanding their details, which was exactly the kind of thing Richard loved to do.

For Richard, figuring out these problems was a kind of game. He always started by asking very basic questions like, "What is the simplest example?" or "How can you tell if the answer is right?" He asked questions until he had reduced the problem to some essential puzzle he thought could solve. Then he would set to work, scribbling on a pad of paper and staring at the results. While he was in the middle of this kind of puzzle-solving, he was impossible to interrupt. "Don't bug me. I'm busy," he would say without even looking up. Eventually he would either decide the problem was too hard (in which case he lost interest), or he would find a solution (in which case he spent the next day or two explaining it to anyone who would listen). In this way he helped work on problems in database searching, geophysical modeling, protein folding, image analyzing and the reading of insurance forms.

The last project I worked on with Richard was in simulated evolution. I had written a program that simulated the evolution of populations of sexually reproducing creatures over hundreds of thousands of generations. The results were surprising, in that the fitness of the population made progress in sudden leaps rather than by the expected steady improvement. The fossil record shows some evidence that real biological evolution might also exhibit such "punctuated equilibrium," so Richard and I decided to look more closely at why it was happening. He was feeling ill by that time, so I went out and spent the week with him in Pasadena. We worked out a model of evolution of finite populations based on the Fokker–Planck equations. When I got back to Boston, I went to the library and discovered a book by Motoo Kimura on the subject. Much to my disappointment, all our "discoveries" were covered in the first few pages. When I called Richard and told him what I had found, he was elated. "Hey, we got it right!" he said. "Not bad for amateurs."

In retrospect I realize that in almost everything we worked on together, we were both amateurs. In digital physics, neural networks, even parallel computing, we never really knew what we were doing. But the things that we studied were so new that none of the others working in these fields knew exactly what they were doing either. It was amateurs who made the progress.

Telling the good stuff you know

Actually, I doubt that it was "progress" that most interested Richard. He was always searching for patterns, for connections, for a new way of looking at something, but I suspect his motivation was not so much to understand the world as it was to find new ideas to explain. The act of discovery was not complete for him until he had taught it to someone else.

I remember a conversation we had a year or so before his death, walking in the hills above Pasadena. We were exploring an unfamiliar trail, and Richard, recovering from a major operation for his cancer, was walking more slowly than usual. He was telling a long and funny story about how he had been reading up on his disease and surprising his doctors by predicting their diagnosis and his chances of survival. I was hearing for the first time how far his cancer had progressed, so the jokes did not seem so funny. He must have noticed my mood, because he suddenly stopped the story and asked, "Hey, what's the matter?"

I hesitated. "I'm sad because you're going to die."

"Yeah," he sighed, "that bugs me sometimes too. But not so much as you think." And after a few more steps, "When you get as old as I am, you start to realize that you've told most of the good stuff you know to other people anyway."

We walked along in silence for a few minutes. Then we came to a place where another trail crossed ours and Richard stopped to look around at the surroundings. Suddenly a grin lit up his face. "Hey," he said, all trace of sadness forgotten, "I bet I can show you a better way home."

And so he did. ■