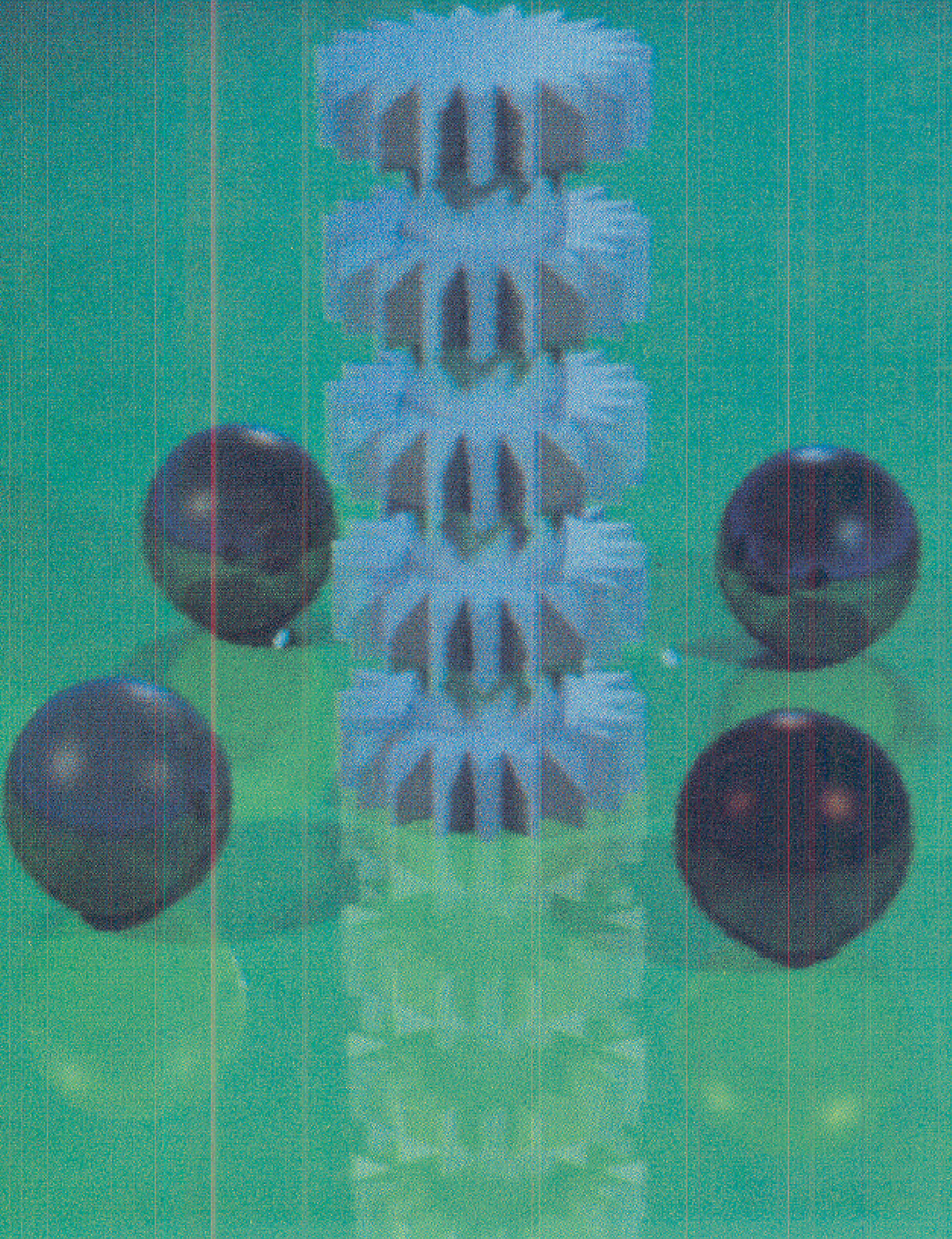
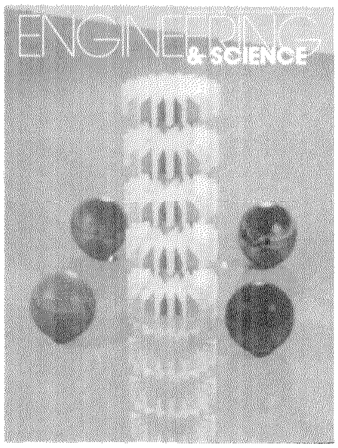
November 1984 California Institute of Technology

ENGINEERING & SCIENCE



In This Issue



Graphic Description

On the cover — a computer graphics image generated by Monte Carlo ray tracing. Ray tracing is a set of algorithms that computes reflection and refraction of individual rays of light off surfaces. More than 15 million rays were shot to make this picture, and Jim Kajiya, assistant professor of computer science, used 36 hours on an IBM 4341 to do it. The gear was specified as the boolean intersection of two solid primitives, a 20-pointed prismatic star, and a surface of revolution.

Kajiya, along with Al Barr, also assistant professor of computer science, and Jim Blinn, lecturer, who currently splits his time between JPL and "The Mechanical Universe," form the core of what is probably the most mathematically sophisticated computer graphics group in the country. Computer graphics at Caltech is relatively new (the field itself is only 20 years old), but progressing by leaps and bounds and with much enthusiasm. Some of their current work is described and illustrated in "Computer Graphics," beginning on page 11.

Just Joking

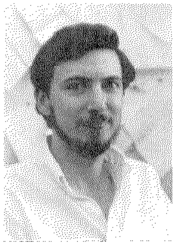
Few members of the Caltech community are likely to recognize Richard Feynman under the title "The Dignified Professor." But it's indeed Feynman, now the Richard Chace Tolman Professor of Theoretical Physics and Nobel laureate, recounting his initial experiences as a faculty member at Cornell (before his Caltech affiliation began in 1950).

"The Dignified Professor," which begins on page 4, is one chapter of a book of reminiscences, *"Surely You're Joking, Mr. Feynman," Adventures of a Curious Character*, scheduled for publication in January 1985 by W. W. Norton & Company, Inc. The stories that make up the book were taped by Ralph Leighton "intermittently and informally during seven years of very enjoyable drumming" with Feynman. Former *E&S* editor Ed Hutchings edited the collection.

In his introduction Al Hibbs, senior member of the technical staff at JPL, describes the memoirs as giving a true picture of much of Feynman's character "— his almost compulsive need to solve puzzles, his provocative mischievousness, his indignant impatience with pretension and hypocrisy, and his talent for one-upping anybody who tries to one-up him! This book is great reading: outrageous, shocking, still warm and very human."

New Wave

David Rutledge, who invented the first millimeter-wave imaging antenna array, describes his research developing this new technology of tiny antennas and detectors in "Integrated Circuits for Millimeter Waves." This work will make millimeter-wavelength radiation a much more useful resource for imaging applications in fusion research, radar, and astronomy. The article, which begins on page 19, was adapted from his Seminar Day talk last May.



Rutledge, associate professor of electrical engineering, is one of 200 scientists nationwide (eight at Caltech) selected as recipients of the first Presidential Young Investigator Awards, which subsidize promising and original research. He has also received an IBM Faculty Development Award (as has Al Barr, pages 11-18).

Rutledge has been working in the field of infrared and millimeter devices, mostly in industry, since 1976. He received his PhD from UC Berkeley in 1980, the same year he came to Caltech. His BA is from Williams College and MA from Cambridge University.

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ENGINEERING & SCIENCE

CALIFORNIA INSTITUTE OF TECHNOLOGY | NOVEMBER 1984 — VOLUME XLVIII, NUMBER 2

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Tiny antennas and detectors on semiconductor chips make millimeter-wavelength radiation useful for imaging applications.

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Richard Feynman, still dignified at Caltech in 1956.

The Dignified Professor

by Richard P. Feynman with Ralph Leighton

I DON'T BELIEVE I can really do without teaching. The reason is, I have to have something so that when I don't have any ideas and I'm not getting anywhere I can say to myself, "At least I'm living; at least I'm *doing* something; I'm making *some* contribution" — it's just psychological.

When I was at Princeton in the 1940s I could see what happened to those great minds at the Institute for Advanced Study, who had been specially selected for their tremendous brains and were now given this opportunity to sit in this lovely house by the woods there, with no classes to teach, with no obligations whatsoever. These poor bastards could now sit and think clearly all by themselves, OK? So they don't get an idea for a while: They have every opportunity to do something, and they're not getting any ideas. I believe that in a situation like this a kind of guilt or depression worms inside of you, and you begin to *worry* about not getting any ideas. And nothing happens. Still no ideas come.

Nothing happens because there's not enough *real* activity and challenge: You're not in contact with the experimental guys. You don't have to think how to answer questions from the students. Nothing!

In any thinking process there are moments when everything is going good and you've got wonderful ideas. Teaching is an interruption, and so it's the greatest pain in the neck in the world. And then there are the *longer* periods of time when not much is coming to you. You're not getting any ideas, and if you're doing nothing at all, it drives you nuts! You can't even say "I'm teaching my class."

If you're teaching a class, you can think about the elementary things that you know very well. These things are kind of fun and delightful. It doesn't do any harm to think them over again. Is there a better way to present them? Are there any new problems associated with them? Are there any new thoughts you can make about them? The elementary things are *easy* to think about; if you can't think of a new thought, no harm done; what you thought about it before is good enough for the class. If you *do* think of something new, you're rather pleased that you have a new way of looking at it.

The questions of the students are often the source of new research. They often ask profound questions that I've thought about at times and then given up on, so to speak, for a while. It wouldn't do me any harm to think about them again and see if I can go any further now. The students may not be able

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to see the thing I want to answer, or the subtleties I want to think about, but they *remind* me of a problem by asking questions in the neighborhood of that problem. It's not so easy to remind *yourself* of these things.

So I find that teaching and the students keep life going, and I would *never* accept any position in which somebody has invented a happy solution for me where I don't have to teach. Never.

But once I *was* offered such a position.

During the war, when I was still at Los Alamos, Hans Bethe got me this job at Cornell, for $3700 a year. I got an offer from some other place for more, but I like Bethe, and I had decided to go to Cornell and wasn't worried about the money. But Bethe was always watching out for me, and when he found out that others were offering more, he got Cornell to give me a raise to $4000 even before I started.

Cornell told me that I would be teaching a course in mathematical methods of physics, and they told me what day I should come — November 6, I think, but it sounds funny that it could be so late in the year. I took the train from Los Alamos to Ithaca, and spent most of my time writing final reports for the Manhattan Project. I still remember that it was on the night train from Buffalo to Ithaca that I began to work on my course.

You have to understand the pressures at Los Alamos. You did everything as fast as you could; everybody worked very, very hard; and everything was finished at the last minute. So, working out my course on the train a day or two before the first lecture seemed natural to me.

So I find that teaching and the students keep life going, and I would *never* accept any position in which somebody has invented a happy solution for me where I don't have to teach.

Mathematical methods of physics was an ideal course for me to teach. It was what I had done during the war — apply mathematics to physics. I knew which methods were *really* useful, and which were not. I had lots of experience by that time, working so hard for four years using mathematical tricks. So I laid out the different subjects in mathematics and how to deal with them, and I still have the papers — the notes I made on the train.

I got off the train in Ithaca, carrying my heavy suitcase on my shoulder, as usual. A guy called out, "Want a taxi, sir?"

I had never wanted to take a taxi: I was always a young fella, short on money, wanting to be my own man. But I thought to myself, "I'm a *professor* — I must be dignified." So I took my suitcase down from my shoulder and carried it in my hand, and said "Yes."

"Where to?"

"The hotel."

"Which hotel?"

"One of the hotels you've got in Ithaca."

"Have you got a reservation?"

"No."

"It's not so easy to get a room."

"We'll just go from one hotel to another. Stay and wait for me."

I try the Hotel Ithaca: no room. We go over to the Traveller's Hotel; they don't have any room either. I say to the taxi guy, "No use driving around town with me; it's gonna cost a lot of money. I'll walk from hotel to hotel." I leave my suitcase in the Traveller's Hotel and I start to wander around, looking for a room. That shows you how much preparation I had, a new professor.

I found some other guy wandering around looking for a room too. It turned out that the hotel room situation was utterly impossible. After a while we wandered up some sort of a hill, and gradually realized we were coming near the campus of the university.

We saw something that looked like a rooming house, with an open window, and you could see bunk beds in there. By this time it was night, so we decided to ask if we could sleep there. The door was open, but there was nobody in the whole place. We walked up into one of the rooms, and the other guy said, "Come on, let's just sleep here!"

I didn't think that was so good. It seemed like stealing to me. Somebody had made the beds; they might come home and find us sleeping in their beds, and we'd get in trouble.

So we go out. We walk a little further, and we see, under a streetlight, an enormous mass of leaves that had been collected — it was autumn — from the lawns. I say, "Hey! We could crawl in these leaves and sleep here!" I tried it; they were rather soft. I was tired of walking around, and if the pile of leaves hadn't been right under a streetlight, it would have been perfectly all right. But I didn't want to get into trouble right away. Back at Los Alamos people had teased me (when I played drums and so on) about what kind of "professor" Cornell was going to get. They said I'd get a reputation right off by doing something silly, so I was trying to be a little dignified. I reluctantly gave up the idea of sleeping in the pile of leaves.

We wandered around a little more, and came to a big building, some important building of the campus. We went in, and there were two couches in the hallway. The other guy said, "I'm sleeping here!" and collapsed onto the couch.

I didn't want to get into trouble, so I found a janitor down in the basement and asked him whether I could sleep on the couch, and he said "Sure."

The next morning I woke up, found a place to eat breakfast, and started rushing around as fast as I could to find out when my first class was going to be. I ran into the physics department: "What time is my first class? Did I miss it?"

The guy said, "You have nothing to worry about. Classes don't start for eight days."

That was a *shock* to me! The first thing I said was, "Well, why did you tell me to be here a week ahead?"

"I thought you'd like to come and get acquainted, find a place to stay and settle down before you begin your classes."

I was back to civilization, and I didn't know what it was!

Professor Gibbs sent me to the Student Union to find a place to stay. It's a big place, with lots of students milling around. I go up to a big desk that says HOUSING and I say, "I'm new, and I'm looking for a room."

The guy says, "Buddy, the housing situation in Ithaca is tough. In fact, it's so tough that, believe it or not, a *professor* had to sleep on a couch in this lobby last night!"

I look around, and it's the same lobby! I turn to him and I say, "Well, I'm that professor, and the professor doesn't want to do it again!"

My early days at Cornell as a new professor were interesting and sometimes amusing. A few days after I got there, Professor Gibbs came into my office and explained to me that ordinarily we don't accept students this late in the term, but in a few cases, when the applicant is very, very good, we can accept him. He handed me an application and asked me to look it over.

He comes back: "Well, what do you think?"

"I think he's first rate, and I think we ought to accept him. I think we're lucky to get him here."

"Yes, but did you look at his picture?"

"*What possible difference could that make?*" I exclaimed.

" Absolutely none, sir! Glad to hear you say that. I wanted to see what kind of a man we had for our new professor." Gibbs liked the way I came right back at him without thinking to myself, "He's the head of the department, and I'm new here, so I'd better be careful what I say." I haven't got the speed to think like that; my first reaction is immediate, and I say the first thing that comes into my mind.

Then another guy came into my office. He wanted to talk to me about philosophy, and I can't really quite remember what he said, but he wanted me to join some kind of a club of professors. The club was some sort of anti-Semitic club that thought the Nazis weren't so bad. He tried to explain to me how there were too many Jews doing this and that — some crazy thing. So I waited until he got all finished, and said to him, "You know, you made a big mistake: I was brought up in a Jewish family." He went out, and that was the beginning of my loss of respect for some of the professors in the humanities, and other areas, at Cornell University.

I was starting over, after my wife's death, and I wanted to meet some girls. In those days there was a lot of social dancing. So there were a lot of dances at Cornell, mixers to get people together, especially for the fresh-

Back at Los Alamos people had teased me (when I played the drums and so on) about what kind of "professor" Cornell was going to get. They said I'd get a reputation right off by doing something silly, so I was trying to be a little dignified.

men and others returning to school.

I remember the first dance that I went to. I hadn't been dancing for three or four years while I was at Los Alamos; I hadn't even been in society. So I went to this dance and danced as best I could, which I thought was reasonably all right. You can usually tell when somebody's dancing with you and they feel pretty good about it.

At Cornell, I'd work on preparing my courses, and I'd go over to the library a lot and read through the *Arabian Nights* and ogle the girls that would go by. But when it came time to do some research, I couldn't get to work.

As we danced I would talk with the girl a little bit; she would ask me some questions about myself, and I would ask some about her. But when I wanted to dance with a girl I had danced with before, I had to *look* for her.

"Would you like to dance again?"

"No, I'm sorry; I need some air." Or, "Well, I have to go to the ladies' room" — this and that excuse, from two or three girls in a row! What was the matter with me? Was my dancing lousy? Was my personality lousy?

I danced with another girl, and again came the usual questions: "Are you a student, or a graduate student?" (There were a lot of students who looked old then because they had been in the army.)

"No, I'm a professor."

"Oh? A professor of what?"

"Theoretical physics."

"I suppose you worked on the atomic bomb."

"Yes, I was at Los Alamos during the war."

She said, "You're a damn liar!" — and walked off.

That relieved me a great deal. It explained everything. I had been telling all the girls the simple-minded, stupid truth, and I never knew what the trouble was. It was perfectly obvious that I was being shunned by one girl after another when I did everything perfectly nice and natural and was polite, and answered the questions. Everything would look very pleasant, and then *thwoop* — it wouldn't work. I didn't understand it until this woman fortunately called me a damn liar.

So then I tried to avoid all the questions, and it had the opposite effect: "Are you a freshman?"

"Well, no."

"Are you a graduate student?"

"No."

"What *are* you?"

"I don't want to say."

"Why won't you tell us what you are?" "I don't want to. . ." — and they'd keep talking to me!

I ended up with two girls over at my house and one of them told me that I really shouldn't feel uncomfortable about being a freshman; there were plenty of guys my age who were starting out in college, and it was really all right. They were sophomores, and were being quite motherly, the two of them. They worked very hard on my psychology, but I didn't want the situation to get so distorted and misunderstood, so I let them know I was a professor. They were very upset that I had fooled them. I had a lot of trouble being a young professor at Cornell.

Anyway, I began to teach the course in mathematical methods in physics, and I think I also taught another course — electricity and magnetism, perhaps. I also intended to do research. Before the war, while I was getting my degree, I had many ideas: I had invented new methods of doing quantum mechanics with path integrals, and I had a lot of stuff I wanted to do.

At Cornell, I'd work on preparing my courses, and I'd go over to the library a lot and read through the *Arabian Nights* and ogle the girls that would go by. But when it came time to do some research, I couldn't get to work. I was a little tired; I was not interested; I couldn't do research! This went on for what I felt was a few years, but when I go back and calculate the timing, it couldn't have been that long. Perhaps nowadays I wouldn't think it was such a long time, but then, it seemed to go on for a *very* long time. I simply couldn't get started on any problem: I remember writing one or two sentences about some problem in gamma rays and then I couldn't go any further. I was convinced that from the war and everything (the death of my wife) I had simply burned myself out.

I now understand it much better. First of all, a young man doesn't realize how much time it takes to prepare good lectures, for the first time especially — and to give the lectures, and to make up exam problems, and to check that they're sensible ones. I was giving good courses, the kind of courses where I put a lot of thought into each lecture. But I didn't realize that that's a lot of work! So here I was, "burned out," reading the *Arabian Nights* and feeling depressed about myself.

During this period I would get offers from different places — universities and industry — with salaries higher than my own. And each time I got something like that I would get a little more depressed. I would say to myself, "Look, they're giving me these wonderful offers, but they don't realize that I'm burned out! Of course I can't accept them. They expect me to accomplish something, and I can't accomplish anything! I have no ideas. . ."

Finally there came in the mail an invitation from the Institute for Advanced Study: Einstein. . .Von Neumann. . .Weyl. . .all these great minds! *They* write to me, and invite me to be a professor *there!* And not just a regular professor. Somehow they knew my feelings about the Institute: how it's too theoretical; how there's not enough *real* activity and challenge. So they write, "We appreciate that you have a considerable interest in experiments and in teaching, so we have made arrangements to create a special type of professorship, if you wish: half professor at Princeton University, and half at the Institute."

Institute for Advanced Study! Special exception! A position better than Einstein, even! It was ideal; it was perfect; it was absurd!

It *was* absurd. The other offers had made me feel worse, up to a point. They were expecting me to accomplish something. But this offer was so ridiculous, so impossible for me ever to live up to, so ridiculously out of proportion. The other ones were just mistakes; this was an absurdity! I laughed at it while I was shaving, thinking about it.

And then I thought to myself, "You know, what they think of you is so fantastic, it's impossible to live up to it. You have no responsibility to live up to it!"

It was a brilliant idea: You have no responsibility to live up to what other people think you ought to accomplish. I have no responsibility to be like they expect me to be. It's their mistake, not my failing.

It wasn't a failure on my part that the Institute for Advanced Study expected me to be that good; it was impossible. It was clearly a mistake — and the moment I appreciated the possibility that they might be wrong, I realized that it was also true of all the other places, including my own university. I am what I am, and if they expected me to be good and they're offering me some money for it, it's their hard luck.

Then, within the day, by some strange miracle — perhaps he overheard me talking about it, or maybe he just understood me — Bob Wilson, who was head of the laboratory there at Cornell, called me in to see him. He said, in a serious tone, "Feynman, you're teaching your classes well; you're doing a good job, and we're very satisfied. Any other expectations we might have are a matter of luck. When we hire a professor, we're taking all the risks. If it comes out good, all right. If it doesn't, too bad. But you shouldn't worry about what you're doing or not doing." He said it much better than that, and it released me from the feeling of guilt.

Then I had another thought: Physics disgusts me a little bit now, but I used to *enjoy* doing physics. Why did I enjoy it? I used to *play* with it. I used to do whatever I felt like doing — it didn't have to do with whether it was important for the development of nuclear physics, but whether it was interesting and amusing for me to play with. When I was in high school, I'd see water running out of a faucet growing narrower, and wonder if I could figure out what determines that curve. I found it was rather easy to do. I didn't *have* to do it; it wasn't important for the future of science; somebody else had already done it. That didn't make any difference: I'd invent things and play with things for my own entertainment.

It was a brilliant idea: You have no responsibility to live up to what other people think you ought to accomplish. I have no responsibility to be like they expect me to be. It's their mistake, not my failing.

So I got this new attitude. Now that I *am* burned out and I'll never accomplish anything, I've got this nice position at the university teaching classes which I rather enjoy, and just like I read the *Arabian Nights* for pleasure, I'm going to *play* with physics, whenever I want to, without worrying about any importance whatsoever.

Within a week I was in the cafeteria and some guy, fooling around, throws a plate in the air. As the plate went up in the air I saw it wobble, and I noticed the red medallion of Cornell on the plate going around. It was pretty obvious to me that the medallion went around faster than the wobbling.

I had nothing to do, so I start to figure out the motion of the rotating plate. I discover that when the angle is very slight, the medallion rotates twice as fast as the wobble rate — two to one. It came out of a complicated equation! Then I thought, "Is there some way I can see in a more fundamental way, by looking at the forces or the dynamics, why it's two to one?"

I don't remember how I did it, but I ultimately worked out what the motion of the mass particles is, and how all the accelerations balance to make it come out two to one.

I still remember going to Hans Bethe and saying, "Hey, Hans! I noticed something interesting. Here the plate goes around so, and the reason it's two to one is. . ." and I showed him the accelerations.

He says, "Feynman, that's pretty interesting, but what's the importance of it? Why are you doing it?"

"Hah!" I say. "There's no importance whatsoever. I'm just doing it for the fun of it." His reaction didn't discourage me; I had made up my mind I was going to enjoy physics and do whatever I liked.

I went on to work out equations of wobbles. Then I thought about how electron orbits start to move in relativity. Then there's the Dirac Equation in electrodynamics. And then quantum electrodynamics. And before I knew it (it was a very short time) I was "playing" — working, really — with the same old problem that I loved so much, that I had stopped working on when I went to Los Alamos: my thesis-type problems; all those old- fashioned, wonderful things.

It was effortless. It was easy to play with these things. It was like uncorking a bottle: Everything flowed out effortlessly. I almost tried to resist it! There was no importance to what I was doing, but ultimately there was. The diagrams and the whole business that I got the Nobel Prize for came from that piddling around with the wobbling plate. □

Computer Graphics

Over the past 10 years an explosion of activity has occurred in computer graphics. Far more than making pretty pictures, computer graphics is one part of computer science with tremendous potential for general use. For example, one of the earliest computer graphics research problems solved in the 1970s was representing and printing text on a television-like screen. Now we take computer terminals and word processing for granted. And even though it may seem like science fiction now, in the not-so- distant future computers will be able to make color animated pictures as easily as text. We'll take this for granted too, expecting even the smallest computer to have this capability.

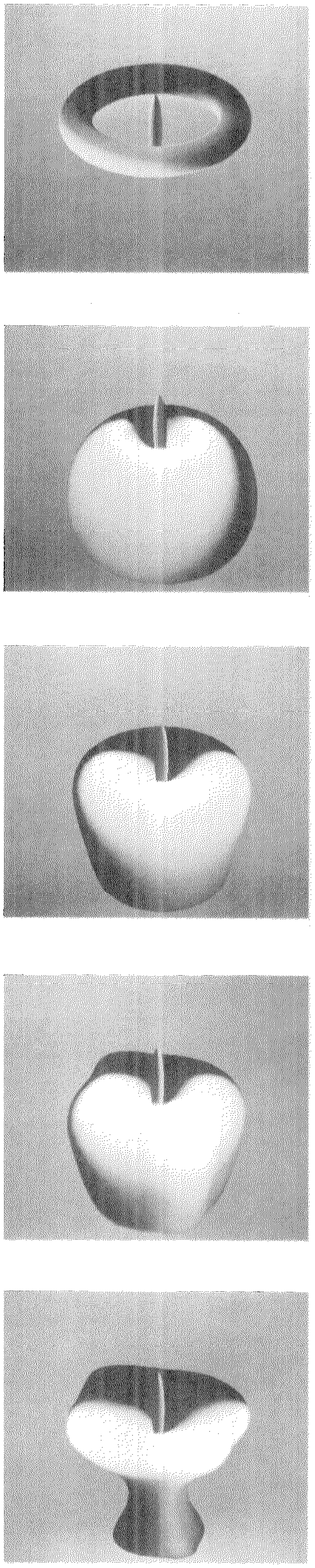
Computer graphics as a field began when Ivan Sutherland (MS 1960) combined the new concept of computer data structures with vector line drawing capability in his MIT PhD thesis in 1963. Activity in graphics started at Caltech only a few years ago, but the group is already establishing an international reputation.

Al Barr, Jim Blinn, and Jim Kajiya form the core of the Caltech research group in computer graphics, which is probably the most mathematically sophisticated computer graphics group in the country. The group is developing fundamental mathematical approaches for computationally simulated physical objects. "It's like Göttingen in the 1920s in physics," says Kajiya, assistant professor of computer science. "Exciting new ideas are surfacing almost daily."

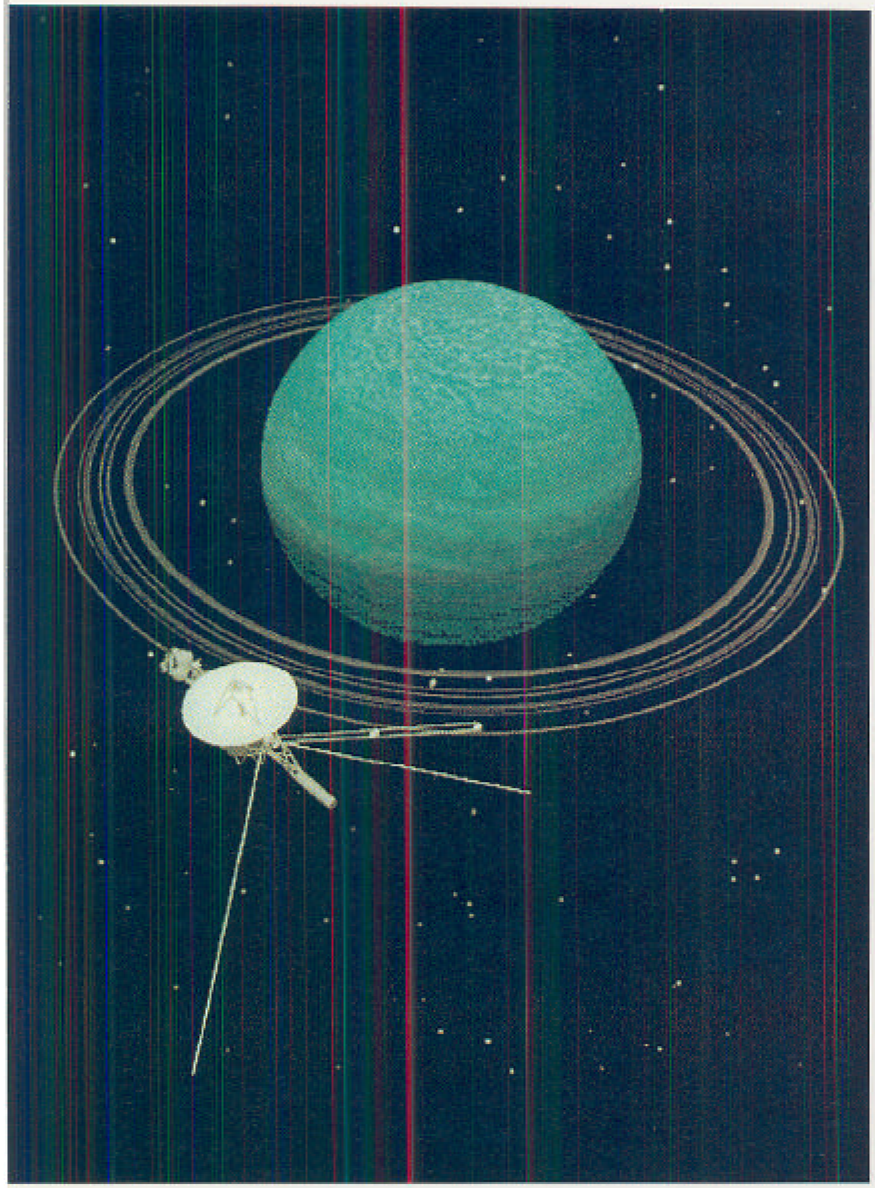
Although the three represent a wide diversity of interests, their basic research creates and uses similar mathematics. They feel that the future direction of the Caltech computer graphics group is to lay a new mathematical foundation for computer graphics. "The graphics field is maturing," says Barr, who is also assistant professor of computer science. "The next really big advances in computer graphics are going to involve a new level of mathematical knowledge and skill."

Barr's work in graphics is dedicated to creating a unified mathematical formalism for representing the shape and the behavior of objects. "Whether the task is making new computer images, creating the next generation of CAD/CAM systems, or studying form and function in biology, the idea is to work out the mathematics, which will be the language to express the new ideas," says Barr.

Kajiya is currently working toward connecting computer graphics principles to the basic equations of electromagnetism that govern the behavior of light. "Every time we apply a deeper understanding of the physics of light," says Kajiya, "a startling advance in the quality of computer-generated images is

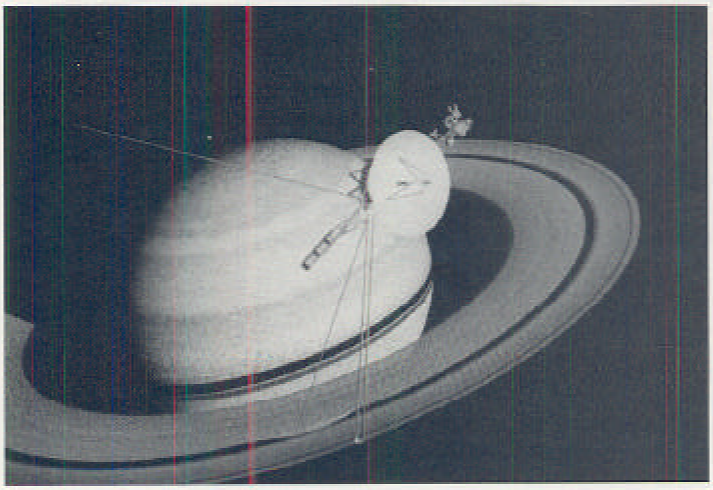


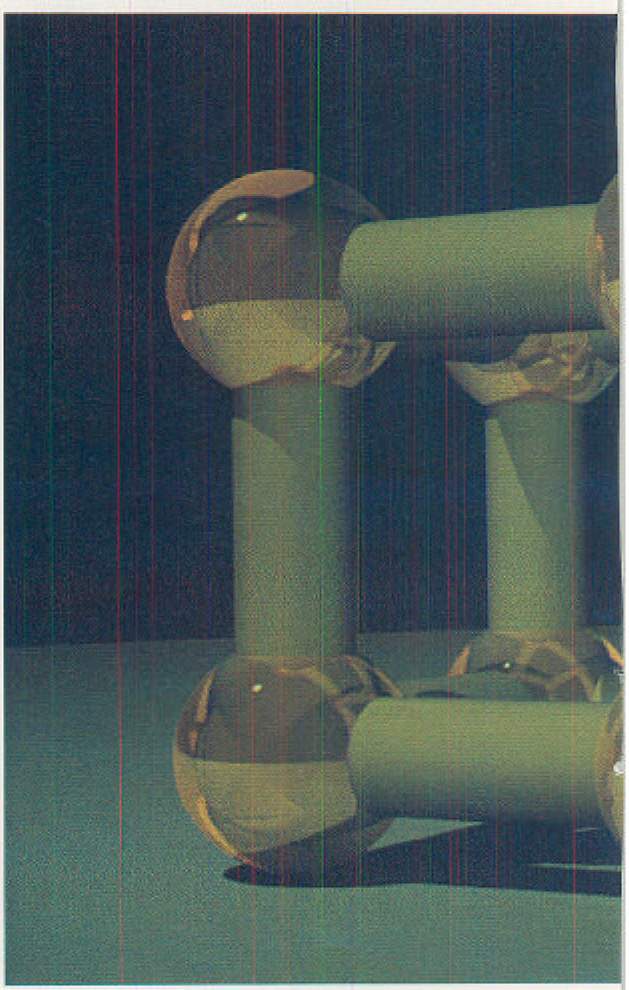
Barr's apples at right illustrate a progression made by deforming simpler shapes, starting with a torus and ellipsoid (top), which is then inflated and tapered. Next to the bottom is a radial, star-shaped deformation, and the bottom apple is just — squashed.



made. The graphics field is progressing in quantum jumps; each year the new images make last year's look primitive."

Of the three, Blinn has been involved in computer graphics the longest. He's been a major influence in the field (a "giant," according to everyone who knows his research) over the past 15 years. He's made significant contributions to a number of areas, the most important, perhaps, being his work in surface modeling and texture mapping. Currently Blinn is mainly interested in the animation of physics, the uses of animation in education, and exploring how the computer can best be used to produce quality animation. "Animation is a venture where computer graphics is especially relevant," says Blinn, "because it can reproduce unchanging parts and mechanize the movement of the



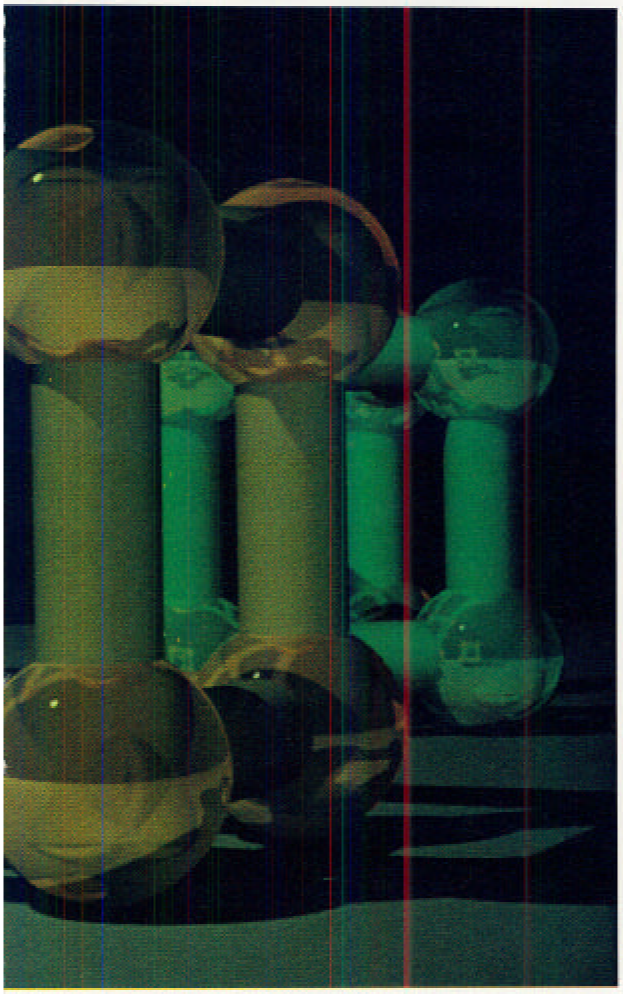


parts that do change."

Blinn's interest goes back to his undergraduate days at the University of Michigan in 1967, shortly after the birth of computer graphics. After his 1978 PhD in computer science from the University of Utah, Blinn was recruited to Caltech as a half- time research fellow (later lecturer) by Ivan Sutherland, who was the Fletcher Jones Professor of Computer Science.

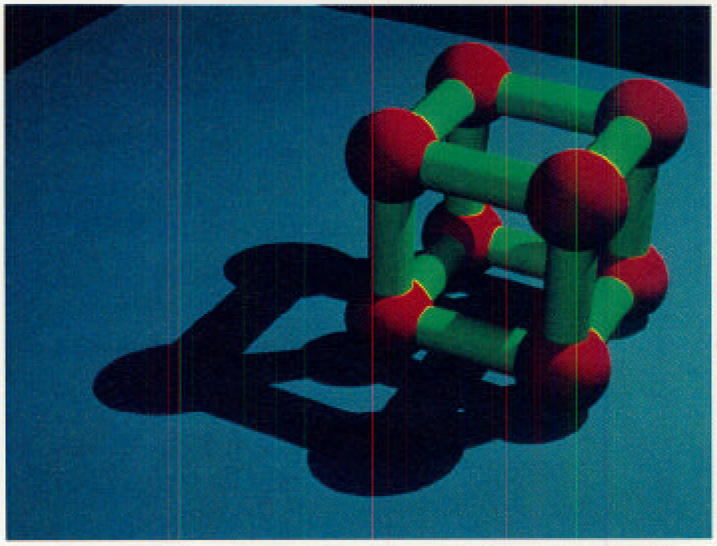
But his primary interest was in the space program, and Blinn spent the other half of his time at the Jet Propulsion Laboratory producing animated simulations of the Voyager missions to Jupiter and Saturn. His simulations, which always have to be specifically pointed out as simulations and not the real thing, won him NASA's Exceptional Service Medal in 1983 as well as a prominent national reputation in computer graphics. His research contributions were also recognized with the first Computer Graphics Achievement Award last year from the ACM SIGGRAPH organization (the special interest group in graphics of the Association for Computing Machinery). He has also produced computer graphics effects for several education programs, such as the PBS series "Cosmos" — notably the sequences on evolution and on the DNA double helix.

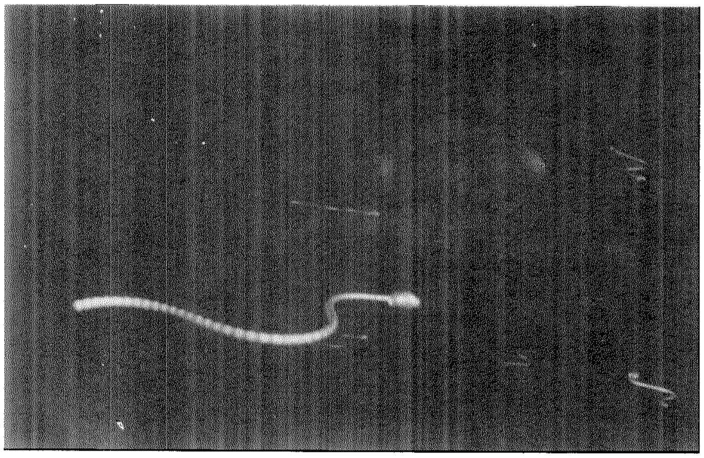
Kajiya was recruited by Sutherland in 1979. He also had a Utah PhD, but his fields of interest were very high-level programming languages, theoretical computer science, and signal processing. His interest in computer graphics began in 1981, after he presented a paper at the national SIGGRAPH conference on different ways of manipulating pixels



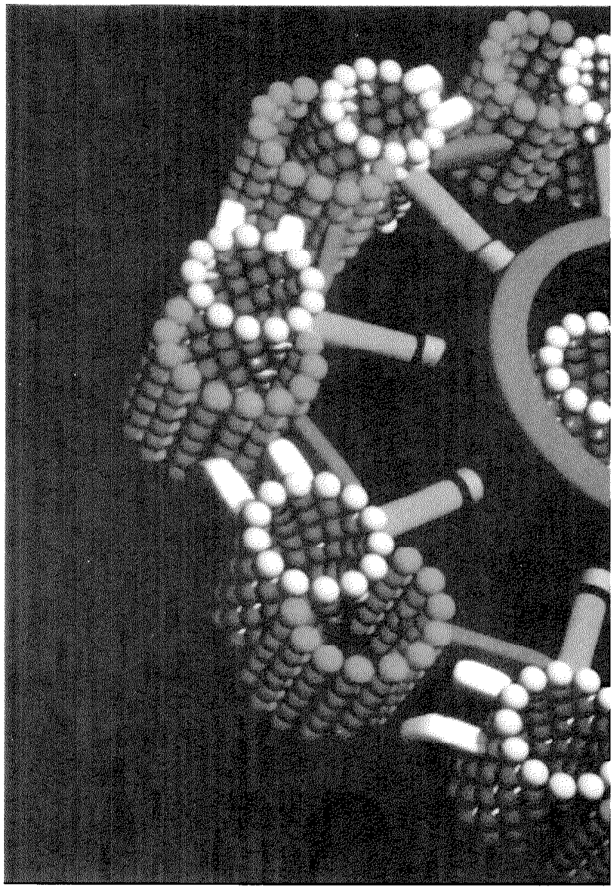
Far left: Blinn's simulated Voyager approaches Uranus (top), an encounter coming up in 1986. While both this and the picture of Saturn below it are computer simulations, Uranus with its nine rings was created out of only sketchy information about the planet; the image of Saturn was made after the fact from the data collected by Voyager.

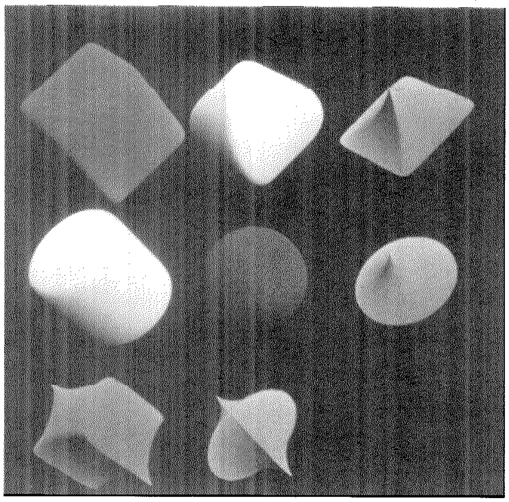
Left: Barr's cylinders and glass spheres show how accurately computer graphics can represent realistic surface properties — in this case of refractive objects. These properties can also be changed (below).



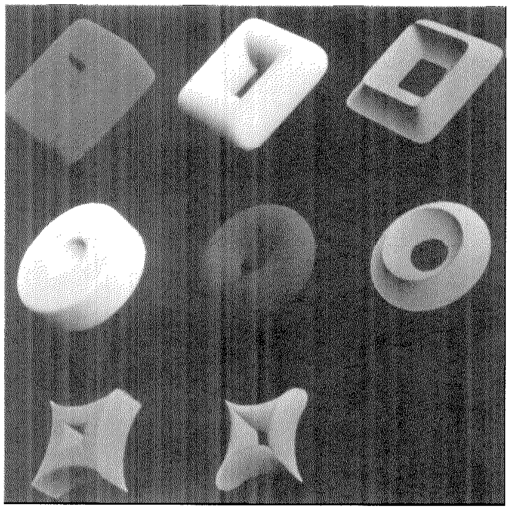


Sperm swim toward an egg in the computer animation frame above by the whip-like motion of their tails. A cross section of the tail produced by computer graphics, shown at right, illustrates the mechanism of the tail motion: The straight white pieces (dynein) ratchet outward, causing the microtubular doublets (here constructed of little spheres) to move up and down with respect to one another. This work was the subject of Barr's thesis.





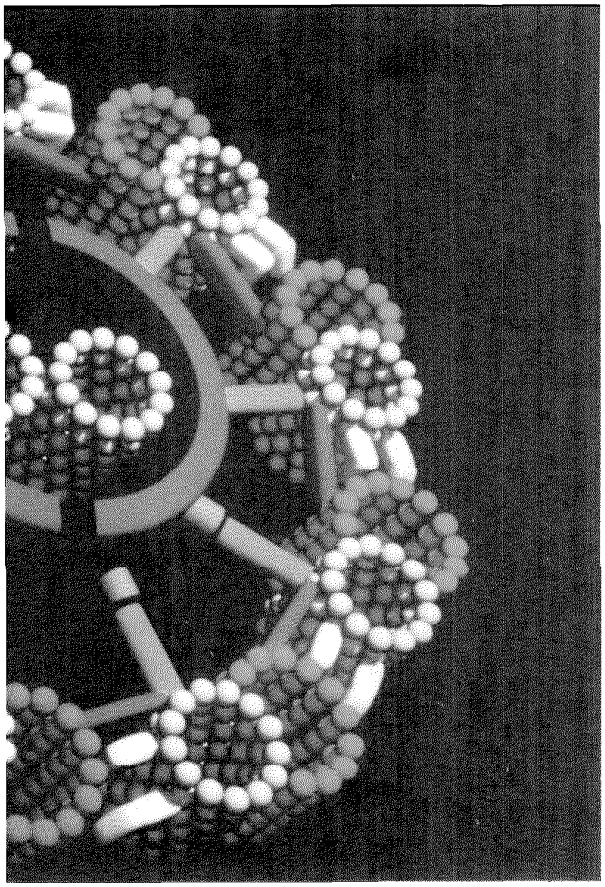
Barr's superquadrics are made of just a few sines, cosines, and exponents. A hole can be put through the middle (below) simply by adding a constant to one of the cosine terms.



(individual picture elements) to get a sharper image in the display of characters on CRT screens.

While Kajiya was in the process of becoming an enthusiastic convert, interest in computer graphics was growing rapidly on campus. Blinn's introductory course was suddenly swamped. Interest was exploding nationally as well, perhaps because of the wider availability of computers. Kajiya met Al Barr in the summer of 1983, when they were both speakers at the SIGGRAPH seminar on the state of the art in computer graphics. At that time Barr was senior research scientist at Raster Technologies, Inc. and was finishing his thesis at Rensselaer Polytechnic Institute. They hit it off quite well — so well, in fact, that Barr, who was about to accept a faculty position at MIT, ended up last January at Caltech instead. He's also one of the four Caltech recipients of IBM Faculty Development Awards — two-year renewable $30,000 grants to outstanding young faculty.

Barr had been here before — with Professor of Biology Charles Brokaw, to study the



biophysics of the flagellar tails of microorganisms, to see how they move in order to swim. The purpose of Barr's PhD thesis in mathematics was to shed some light on a problem in theoretical evolutionary biology, developing generic mathematical modeling tools in the process.

"Microscopic observations suggest that speed could be an evolutionary selection criterion that maintains the form of swimming sperm cells," Barr says. "I needed to model the shape of realistic cells as a function of time, to mathematically set up and solve the differential equations of the swimming motion of these shapes in a viscous fluid, and then race the different sperm cell designs to find optimal shapes that maximize the net forward speed. The numerical experiments provided evidence that speed is an important selection criterion affecting the evolution of sperm cell shapes."

Barr realized that the easiest and most convincing way to show that his mathematics worked and that it was correctly implemented on a computer was to make computer graphics movies of hypothetical sperm cells swimming. "Making plots and charts of the various swimming speeds was not enough," he says. "Graphics made a visual confirmation possible, to verify that the swimming simulation observed on the computer screen matched the real swimming under the microscope."

This work developed into a general interest in shapes — how to represent mathematically shapes that deform (bend, twist) over time. The same mathematics can be applied to biophysics, to CAD/CAM (computer-aided design/computer-aided manufacture), and to robotics. Barr can generate a whole family of round and square solid shapes he calls superquadrics with "just a few sines, cosines, and exponents," and put a hole through the middle by adding a constant to one of the cosine terms.

Fractals are mathematical shapes generated out of a process of constrained randomness; they're good for modeling a wide variety of natural phenomena, such as trees, mountains, and clouds. Kajiya's most recent work is on clouds, simulating how light scatters in an inhomogeneous medium. Coauthor with Kajiya on the cloud paper was graduate student Brian Von Herzen. He made the first computer graphics movie of the time evolution of clouds, rendered with Kajiya's algorithms, using a simplified meteorological model. Currently he is finishing his master's thesis with Barr on new representation of curved surfaces.

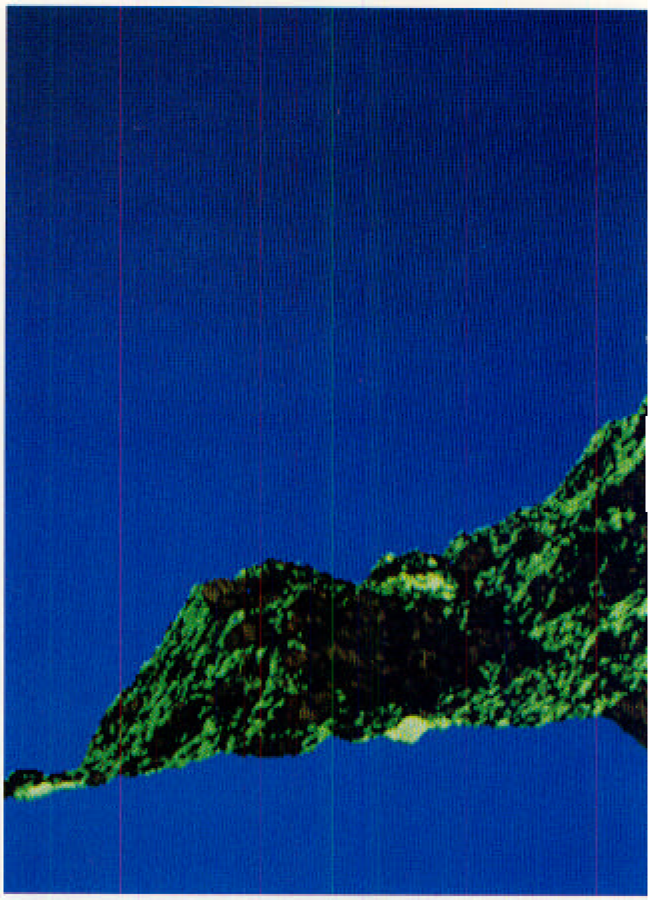
Kajiya is also interested in anisotropic reflection, that is, reflection from surfaces such as cloth, hair, or fur. What Kajiya calls the "fuzzy object problem," an open problem proposed by Blinn seven years ago, is still unsolved. Barr's geometric modeling of shape, together with Kajiya's sophisticated models of light, has the potential to set new standards in the state of the art in computer graphics realism. They are working on the mathematical methods used for the simulation of hair, fire, fabric, and splashing water, as well as simulating the shapes and appearance of plants and animals. "Computer graphics is currently able to make pictures of only a small fraction of the kinds of natural objects we encounter every day," Kajiya says. Realism is one of the major goals of the Caltech group.

Realism has evolved in stages over the history of computer graphics in the last 15 years. It was a problem just to define and make pictures of surfaces in the first place. First, all the hidden lines had to go, so that the lines defining the front surface obscured those in back. "This took about five years," says Kajiya. "Solving light reflections was at first simply a matter of picking up a freshman physics book and using Lambert's cosine law." New methods were then invented to simulate specular reflection. Highlights and shading eventually yielded realistic goblets and teapots, but all computer graphics objects still looked as though they were made of plastic. A new model, incorporating Fresnel's laws of reflection from conductors, renders different surface properties — different kinds of metals and so on. (Blinn was involved in the development of many of the reflection models.) Eventually a sophisticated, time-consuming set of algorithms called ray tracing incorporated refraction, multiple reflections, and shadows. Ray tracing basically follows the path of each photon as it bounces off surfaces. In the past year researchers at Lucasfilm have made startling advances by applying Monte Carlo techniques to graphics.

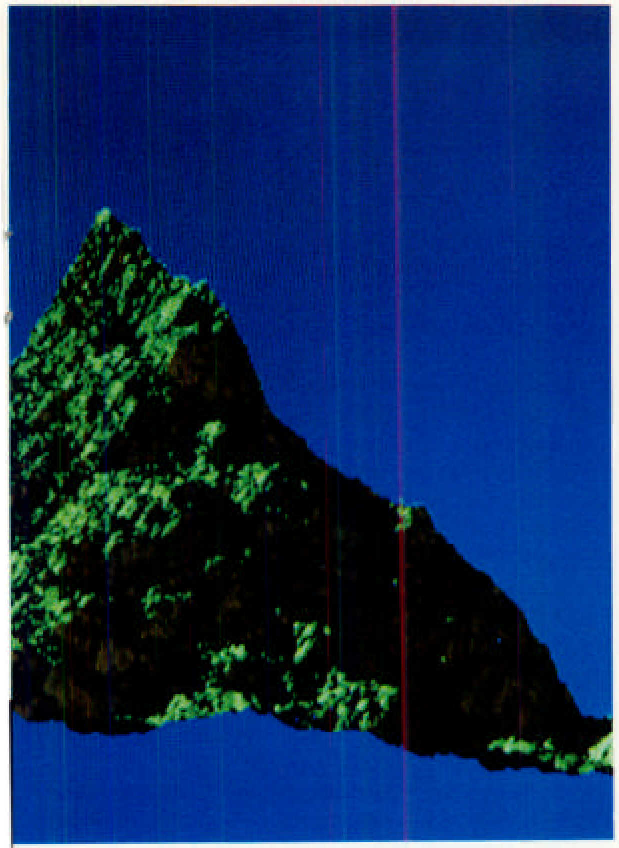
Kajiya's favorite observation about the field was made by graduate student John Platt, who came up with the dictum: "Computer graphics is just applied electricity and magnetism."

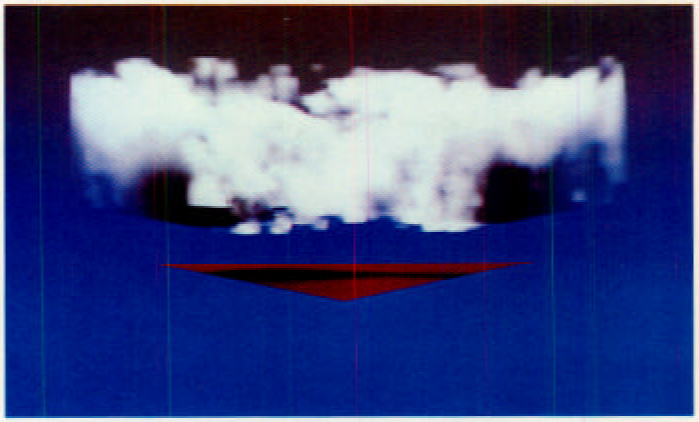
But that's not all it is. Computer graphics also offers a new medium to artists. This has generated some interesting interactions at Caltech between artists and scientists. Vibeke Sorensen is currently a visiting associate at Caltech, active in the computer graphics group. She is also on the faculty of the School of Film and Video at the California Institute of the Arts, where she teaches computer animation. For the past two years, she has led a tutorial at the SIGGRAPH conference on computer art and design, and has won a number of awards for computer animation. Sorensen is also interested in participating in Caltech's computer music group as well, to create music to accompany her digital images. This group, led by Carver Mead, the Gordon and Betty Moore Professor of Computer Science, is translating the underlying physics and differential equations of musical instruments into VLSI circuits — but that's another story.

Blinn has also been interested in bringing the art world and the computer world together and in 1982 started a course at Pasadena's Art Center College of Design. A year later this evolved into a projects course for students from both institutions. Caltech and Art Center have a reciprocal exchange program, but Blinn's course was a unique cooperation, combining the particular complementary talents of each: Art Center students mostly designed the artistic objectives, and the Caltech students came up with the software to implement them.



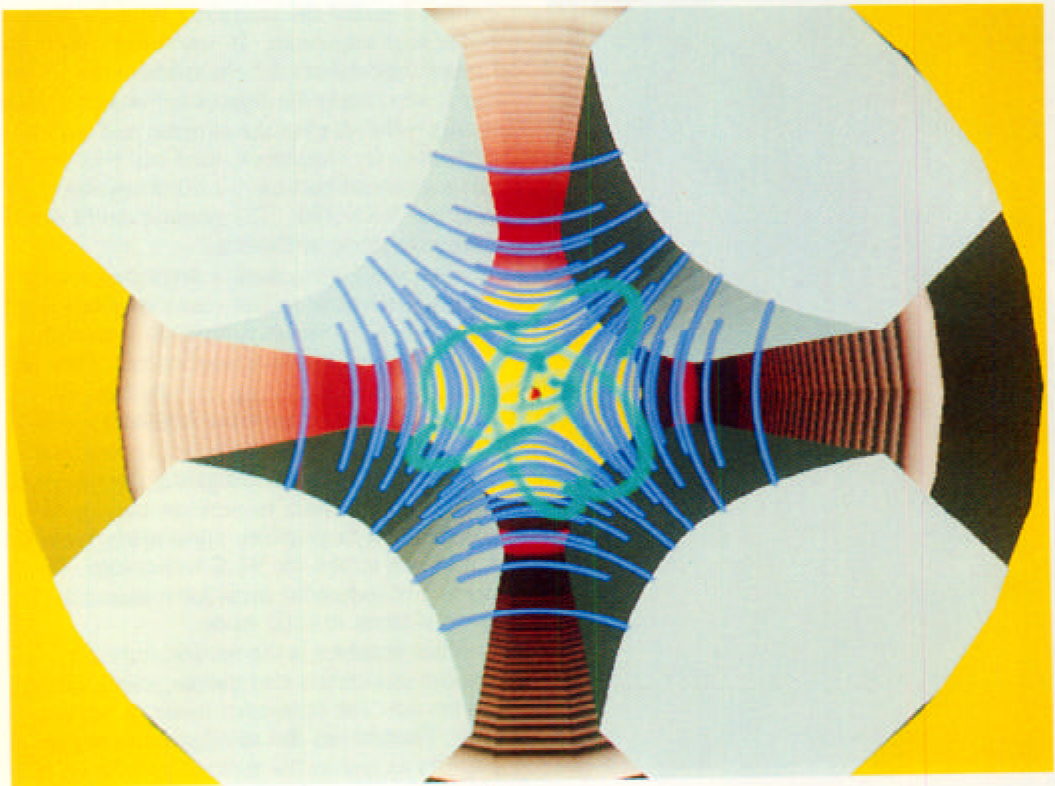
Blinn isn't teaching the introductory Caltech-Art Center course this year, but Sorensen and Bob Schaff (BS 1984) are continuing it. Instead of teaching just a few students, Blinn is currently devoting most of his time to reaching hundreds of thousands through computer graphics. He is creating animated computer graphics sequences for "The Mechanical Universe," Caltech's TV course in basic physics, developed in cooperation with the Corporation for Community College Television and funded by the Annenberg/CPB Project. Blinn's creations, which include such scenes as animated equations and physics experiments, orbiting planets, and an ion going through a particle accelerator, will take up approximately five minutes of each of the 60 half-hour programs. The series is scheduled for release in the fall of 1985.





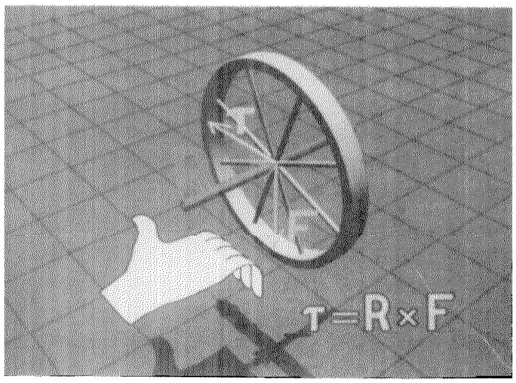
Above: A computer-generated cloud casts its shadow on the red triangle. Kajiya models clouds to simulate how light scatters in an inhomogeneous medium.

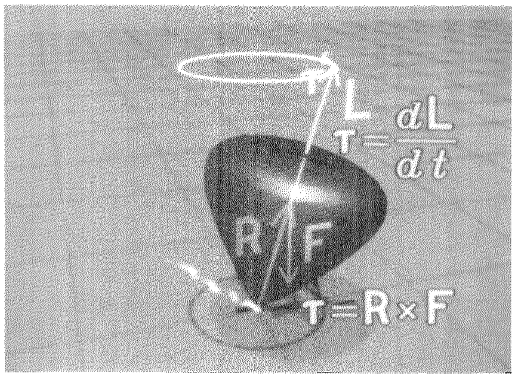
***Right:** Kajiya's fractal mountain is made from mathematical shapes generated out of a process of constrained randomness.*



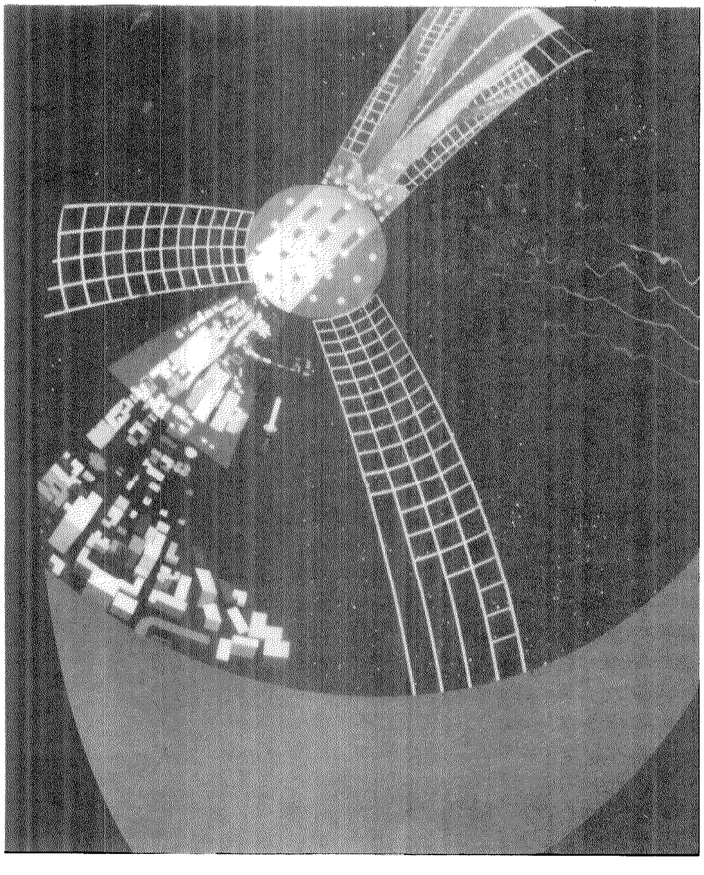
***Blinn** created this animation of an ion's path through a particle accelerator for "The Mechanical Universe." Here the ion (blue squiggle) steers into the magnetic field created by a quadrapole focusing magnet.*

Animation by Blinn for "The Mechanical Universe" illustrates the right-hand rule in finding the direction of torque (top) and the precession of a spinning top (bottom).





In the Omnimax movie flight into a space colony (below), the Caltech campus (notably Beckman Auditorium) is recognizable at center left.



This year the SIGGRAPH conference sponsored the world's first computer-generated Omnimax film, where very large-format images were projected inside an eight-story hemispherical dome. Out of 19 contributions produced nationally, 4 were from Caltech. Barr animated a giant school of graceful sperm cells swimming toward a looming, undulating egg cell, using a rendering program written for the purpose by an RPI colleague. JPL's Jeff Goldsmith, with software by Kajiya and Blinn, contributed a fly- by of Saturn. The third was Sorensen's sequence based on the constellations, with software written by Blinn. Kajiya, with computer science grad students Tim Kay and Brian Von Herzen, together with help from the Art Center students, contributed a 30-second animation of a flight into a space colony (which just happened to house the Caltech campus).

"Every frame of the space colony picture represents a state- of-the-art advance in ray tracing. Several new algorithms and techniques were invented just to make this movie possible," says Kajiya. "It's one of the most complex ray-tracing movies ever made." It was also one of the most expensive computer graphics movies ever made.

Cray Research in Mendota Heights, Minnesota, donated the computer time for three of the four sequences. It was winter when the Caltech researchers did the calculations ("One day it was nearly 40 degrees below zero," Barr recalls.) But despite the climatic and logistic difficulties in Minnesota, the Cray was absolutely essential because it's 60 times faster than the VAX 780. The projects could not have been done at Caltech.

Barr and Kajiya need a surprising amount of computer time — last year the group used about 500 CPU hours from Cray Research just to do the Omnimax sequences. They are in the process of creating an industrial consortium to help support the laboratory and obtain access to fast computers to continue their work. They're investigating the design of specialized hardware to perform large-scale computation in graphics, particularly looking at methods suited for VLSI technology. A number of industrial firms have shown a strong interest in their work.

Since graphics is the natural interface between computers and people, eventually it will become the dominant mode of interaction. Therein lies the motivation to make graphics as real as the technology allows. □

Integrated Circuits for Millimeter Waves

by David Rutledge

RADIATION IS CHARACTERIZED by its wavelength and frequency. Millimeter waves have a wavelength in the range between a millimeter, surprisingly enough, and a centimeter — fingernail size or smaller. (The submillimeter range goes from 100 microns up to a millimeter.) In the electromagnetic spectrum, the band lies between microwaves, which are longer, and infrared radiation, which comprises the shorter wavelengths. As for frequency, the millimeter waves go from 30 gigahertz to 300 gigahertz, and the submillimeter range from 300 gigahertz up to 3 terahertz. Microwaves have lower and infrared radiation higher frequencies.

The characteristics of different kinds of radiation enable us to use them to "see" different things. For example, one of the most successful uses of millimeter waves so far is in looking at the electron density and the magnetic field inside a tokamak, a device for generating a fusion reaction in a plasma (ionized gas). Microwave radiation won't work in this application because it bends in a plasma, making it difficult to tell where the microwave beam went or where it's going. And infrared systems are strongly affected by vibration; tokamaks, when they create a plasma, fire a large current pulse that tends to rattle everything. Millimeter and submillimeter waves, however, are just right.

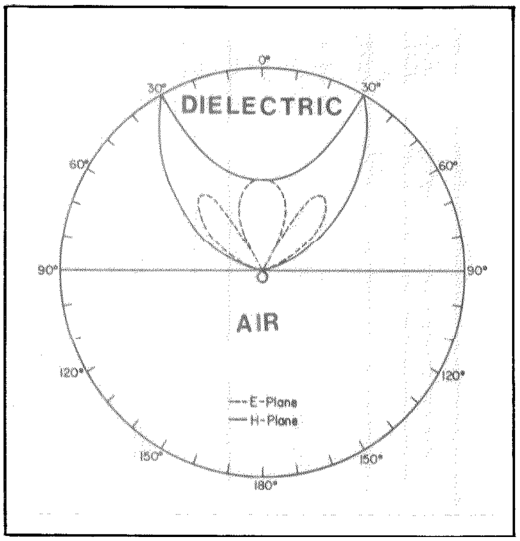
Systems using millimeter waves offer a number of advantages. They can have a higher resolution than microwave radars, and the millimeter waves penetrate smoke and dust much better than infrared systems do. This feature of millimeter radiation has suggested, especially to the military, potential applications in imaging radar and missile guidance. Several such systems have already been developed.

Astronomy is the most difficult application. Objects that are very cold (in the range 10K to 50K) radiate at millimeter wavelengths. With millimeter wave astronomy scientists can see the formation of stars in large, cool, gas clouds. There's also an active effort at Caltech's Jet Propulsion Laboratory to use millimeter wavelengths to study the chemistry of the earth's upper atmosphere, which is also very cold.

The major stumbling block in all these applications is the technology. In some ways it's competitive, but in other ways, particularly in costs, it's not. Infrared systems, which usually use natural radiation as a source of power, may be the best bargain of all. At microwave wavelengths, for a source of 100 mW of power, if you know how to design an oscillator, you can go buy a $10 transistor, wire it up, and you're in business. At millimeter wavelengths, you can't just wire it up unless you're very skilled, but even if you are, the transistor doesn't exist anyway. The device you could use, if you want, say, an FM modulated source, is called a carcinotron. Despite its name it doesn't cause cancer, but its price of $100,000 is enough to make anyone feel bad. And it lives for only about 500 hours — a good deal for the manufacturer but not for the student working in the lab.

Detectors present similar problems. You can buy a good $10 Schottky diode detector for microwaves and wire it up yourself. Similarly, at infrared wavelengths the photoconductors are reasonably priced. But assembling a millimeter-wave system is, again, very difficult and probably would require a complete $10,000 receiver.

You also need to be able to transport energy around within a system. Most



This polar plot shows antenna sensitivity at different angles in the two principal planes. The antenna is much more sensitive to power coming from the dielectric than from the air side.

modern microwave systems depend on integrated circuits that are quite reliable and reasonably priced. Infrared technology of lenses and fibers has been developed successfully. With millimeter waves, however, we've been stuck with tiny, hollow metal waveguides. The hollow metal guides become even smaller and harder to make as the wavelength gets smaller. There are only a couple of machinists in the country who can make these things; it often takes a year of waiting and can cost $1,000 per joint for the connections in a millimeter-wave system at the higher frequencies.

About five years ago several research groups, including mine, came up with the idea that integrated circuits that would work at millimeter wavelengths could solve these technological problems and make millimeter waves a more useful resource. For example, we could try to make an imaging system with antennas, diodes, and processing circuitry on the same circuit. Such a system would be small, light, rugged, and much cheaper than previous approaches. Perhaps a complete radar system could be built on a chip that would include everything: thin film, metal antennas, transistors, diodes, receive circuits, and processing circuits.

The work of my own group (which includes former grad student Dean Neikirk, now assistant professor at the University of Texas, and Professor Neville Luhmann at UCLA) has concentrated on three parts of the problem. We've developed antennas that work on a dielectric substrate, such as quartz, or a semiconductor substrate, such as silicon or gallium arsenide. We've also made some bolometer (thermal) detectors that work with the antennas and have built arrays of antennas and bolometers that can make pictures of plasmas.

First we dealt with the antenna problem. Antennas on a substrate behave very differently from antennas in free space. They're primarily sensitive to radiation that comes from the bottom side of the substrate *through* the quartz or the silicon. This wasn't the obvious direction to expect, and when scientists began testing such antennas, this isn't where they looked. Antennas are also strongly affected by substrate modes — waves that bounce around inside the substrate. While these same modes are responsible for the success of optical fibers, they're a real problem for millimeter waves.

To get an idea of what causes the antenna to be more sensitive to radiation from the substrate, we can think of it as a small dipole antenna that's receiving power. Let's assume it's lying on a quartz (or silicon or gallium arsenide) substrate, and a wave comes in that is incident on the antenna. The antenna responds to the wave's electric field in the sense that the field that's left over is the sum of the wave that comes in and the wave that bounces off — the incident wave and the reflected wave.

When you go through the mathematics, it turns out that when the wave comes from the top, or the air side, the reflected wave has the electric field pointed in the opposite direction. Because of a phase change, the reflected wave tends to cancel the incident wave, making the leftover field small, and the antenna responds weakly. In a wave coming from the dielectric, or substrate, side, the electric field is modified by the dielectric, and the opposite situation occurs. The electric field and the reflected wave have the same sign as the incident wave, resulting in a big field and a strong antenna response. So it's obvious that you need to focus power in from a lens on the back side of the substrate.

Not only does this enable us to come in from the side where the antenna is most sensitive, it also deals with the problem of the substrate modes. One way to understand this problem is to think of the antenna as on top of a substrate transmitting rays of energy to propagate out. There is a particular angle, known as the critical angle, where all the energy at larger angles is completely reflected. This is what makes optical fibers work — the light just bounces around inside the substrate.

But this is inefficient for a transmitting antenna, since 80 percent of the energy may be lost to power that's just bouncing around inside. With the lens on the back of the substrate, however, waves coming out from the antenna hit the curved surface at a small angle of incidence. So the reflection is small, and the radiation goes right through.

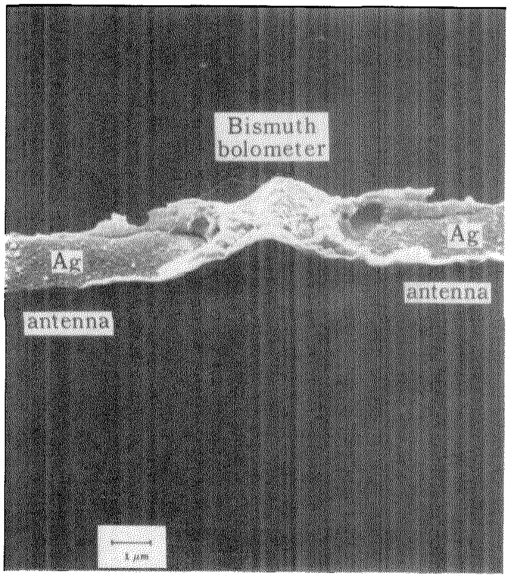
The antennas we use for most of the circuits are shaped like bow ties turned on their sides. The detector is at the apex of the bow, and we can think of the wings of the bow as collecting the energy and putting it down into the diode. Because there aren't any good computer models of the antennas, we design them by first making large microwave models. We model at longer wavelengths (say, 30 mm) where we can cut the metal shapes out quickly with scissors and try a large number of shapes to see how they work. The successful ones can then be made in smaller sizes; for example, the integrated circuits that we built had a wavelength of 1 mm.

Making a detector that would work with the antennas was the next problem. We developed a variety of thermal detectors called bolometers, which are temperature-sensitive resistors. The resistor will absorb power and heat up, and the power can be measured by the rise in temperature of the bolometer. A good thermal detector makes it hard for the heat to get out, so that the temperature rise will be larger and more easily detected. Our bolometers, which are made of bismuth, can detect a power of about 10 picowatts (10^{-11} watts). They can detect a change in about a microsecond — fairly fast for thermal devices.

Dean Neikirk came up with an ingenious idea for preventing heat from escaping from the bolometers. Using a photosensitive material called photoresist, a standard material for making integrated circuits, he developed a quite unstandard procedure. His idea was to get the bismuth resistor to go up in the air a little bit to keep the heat from getting out into the substrate. He did this with a pattern of little bridges, first of silver, then bismuth, finally arriving at a structure where the silver is left with bismuth on top of it, so that the bismuth can pick itself up off the substrate about ½ to 1 micron and then come back down. It's a very sensitive device; for each microwatt of power received the temperature of the bolometer rises about 1 degree. It's also fragile; a lot of power will burn it up.

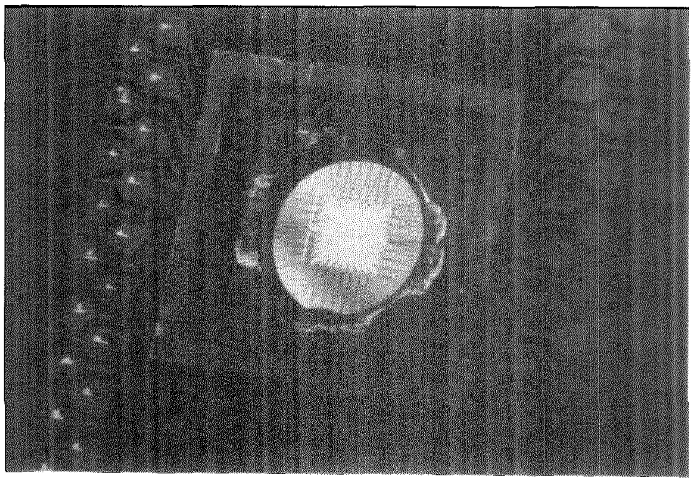
When we had worked out both the antennas and the detectors, we combined them into imaging arrays to look at plasmas. Conventional imaging at millimeter wave frequencies has been done with one detector (partly because they're so expensive) and the optics moved around to point at different areas in order to make the image. But when things are happening fast, as they do in plasmas, you don't have time to move the mirrors around to see the different parts. In our arrays each antenna has its *own* detector, and once we measure the power at each antenna, we can plot it out and get an image.

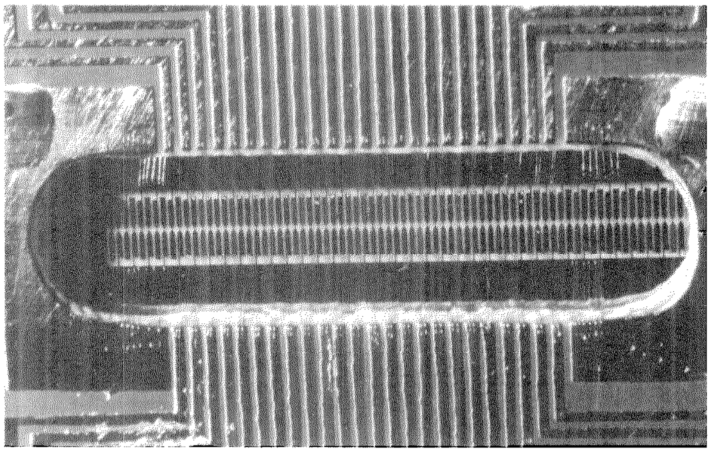
The main design question for such an imaging array is how far apart to put the antennas. If you put them too close together you need a lot of extra electronics; if you put



The air-bridge, bismuth detector (left), seen through a microscope, lifts to keep heat from escaping into the substrate. The silver antenna, which shorts out the bismuth, is a couple of microns across and several microns long.

Below is an imaging array for 0.1 mm wavelengths. The 40 bow-tie antennas occupy the middle of the circuit — about 1 mm across.





The antennas above are the narrow vertical metal strips, spaced about ¼ mm apart. This is an imaging array for a wavelength of 1 mm for tokamak measurements.

them too far apart, you may miss some of the picture. This is just a sampling problem, fortunately the sort of problem that's common in electrical engineering, and there are criteria we can use to determine how close together the antennas should be. It turns out that for most of our systems, the antenna spacing should be about half the wavelength in the lens.

We have already set up our imaging arrays on a tokamak and have made some initial plasma measurements. We've gotten some good first images out of the system and expect the array to be of use when experimental changes are made in the tokamak — to be able to tell what the plasma is doing and even where it is, which has been difficult to determine before.

We've developed other arrays that we haven't yet applied to real- life measurements. There is one for a smaller wavelength — about .1 mm. The antennas get smaller as the wavelength gets smaller, ending up in this case looking like mere fuzz on the chip. The bow tie antennas are not much bigger than the detector. We've also made a two-dimensional array of antennas (bow ties with detectors that form rows and columns) that can follow point sources around rather than provide an image. We can look for the largest signal on a particular row and column, and this will tell us where the source is in the far field.

Still another array, which will soon be applied to plasma measurements, is basically a polarimeter: It tells us which way the electric field is pointing. There are two sets of bow ties in this array — one set alternating with another set pointing at right angles to it. This arrangement effectively creates two imaging arrays — one leans to the right and responds to the electric field pointed toward it, and the other leans to the left and responds to the other component. When we take all the measurements and compute a bit, we can determine which way the electric field is pointed at any particular place. This can be important in radar applications, because you can often tell from the polarization characteristics of the target whether it's manmade or natural; manmade targets tend to have more smooth surfaces that preserve the polarization.

In a plasma this can be applied to the magnetic field. The magnetic field in a plasma vessel, such as a tokamak, causes the electric field to turn a little bit. If you can measure how far the electric field is turned, then you can figure out how big the magnetic field is. This is a crucial item because the magnetic field is what holds the plasma together.

It's important in a millimeter-wave system to have an array that delivers the full resolution allowed by the optics, because, while the resolution of the millimeter-wave systems is better than that of microwaves because the wavelengths are smaller, it's also obviously worse than infrared. Tests on our imaging system have shown that the array itself is not limiting what we can see, but that we are diffraction limited.

Given the progress we've made so far in making antennas and detectors and combining these in imaging arrays, it should be possible to make integrated circuits that also include processing circuits and that act as complete millimeter-wave circuits. And if transistors can be made fast enough to work at millimeter wavelengths, we should be able to make small, inexpensive radar with receiver and oscillator on a single chip as well.

In the future our work will concentrate on improving these arrays. One of the obvious ways is to put more sensitive detectors in them. Over the last year we've been working to incorporate Schottky diodes, which are more sensitive detectors than bolometers but much harder to make. We think this will provide the sensitivity necessary for military applications. Tom Phillips, professor of physics, and others have developed a superconducting tunnel detector, which is even more sensitive than Schottky diodes. This detector might make possible an array sensitive enough to use for millimeter-wave astronomy. □

Research in Progress

Quadraphonic Brain

THE HUMAN BRAIN, which has doubled in size at least once in its evolutionary history, could be said to have undergone a comparable transformation in the 1960s and 1970s, with the discovery by Caltech's Roger Sperry, Board of Trustees Professor of Psychobiology, Emeritus, and 1981 Nobel laureate, that each hemisphere has its own distinctive capacities and functions. Working with split-brain patients whose corpus callosi — the dense network of fibers linking the two halves of the brain — had been severed, Sperry was able to demonstrate that the right hemisphere is predominant for spatial-holistic thought, pattern recognition, and music, while the left hemisphere is specialized for verbal, numerical, and analytical tasks.

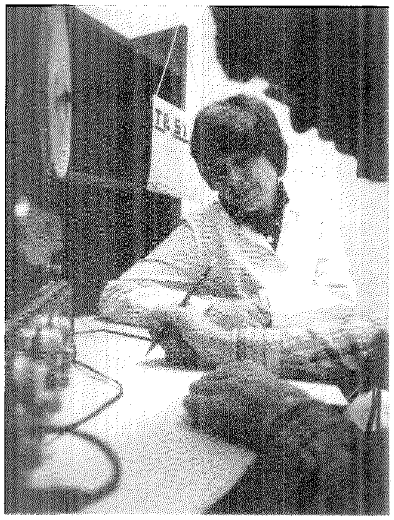
Now Polly Henninger, 1982-1984 research fellow in psychobiology and currently a visiting associate at Caltech and assistant professor at Pitzer College in Claremont, has uncovered evidence that could again double the brain's attributes. Her findings, based on two years of experimental work with normal and split-brain subjects, suggest that each hemisphere may itself house a hemisphere — a secondary center that to an as-yet undetermined degree mimics the role and functions of the opposite half of the brain.

This notion is based on data that surfaced during Henninger's research into suppression — a phenomenon exploited by auditory tests that examine lateralization, or the hemispheric division of labor. In hearing, input to one ear is carried by the primary contralateral pathways to the opposite hemisphere for processing and to the hemisphere on the same side as the receiving ear by the secondary ipsilateral pathways. The dichotic technique makes use of the fact that when both ears simultaneously hear competing stimuli, the input entering the brain from the ipsilateral pathways is inhibited, or suppressed, by the input to the stronger, more numerous contralateral pathways.

Verbal dichotic tests of the type Henninger is currently using illustrate how this process works. Test subjects hear a different word, syllable, or number in each ear and are asked to report it. Since verbal material is more effectively processed by the left hemisphere, which governs the right side of the body, subjects usually show a right-ear advantage, meaning that they hear and report more correct right-ear (contralateral) than left- ear (ipsilateral) input. In normal subjects, ear differences are small. But in split-brain subjects, where communication between the two hemispheres has been cut off, almost all reported input is from the right ear, reflecting suppression of the ipsilateral pathways.

Henninger's dichotic study focuses on the unanswered question of the relative influence of cognitive as opposed to perceptual demands on left-hemisphere processing (more difficult task versus more intense stimulus). In tests she has developed, the cognitive element is task-loading; the perceptual factor variation in volume. Subjects listen to a dichotic digits tape in which the one-syllable digits 1 through 12 (less 7 and 11) are presented to both ears as practice trials. Then, in the actual tests, different digits are presented simultaneously to each ear, either with the volume equal in both ears or raised or lowered five decibels in one ear. In both the practice and actual trials, subjects write what they hear.

The test was initially administered to one of the split-brain subjects, with results that raised a completely new issue. With the volume up in the left ear (to offset the right-ear advantage), the subject first wrote down left-ear digits but shifted to writing only right-ear digits after the task load increased from three digits to four. Three additional split-brain subjects were tested, with similar results. Even with the higher volume, such verbal fluency on the part of an isolated right hemisphere was so unlike dichotic results generally obtained from splits that Henninger questioned whether there had been any right hemisphere involvement at all. Was it possible that the shift had not occurred between hemispheres but rather within the single left hemisphere, from the ipsilateral to the contralateral pathways?



Psychobiologist Polly Henninger administers a new dichotic listening test that she developed to examine hemispheric action in the brain to senior Tad White.

To explore this possibility, the test was given to a subject who at the age of seven had undergone a hemispherectomy that removed his entire right hemisphere. Input to both ears was processed in his left hemisphere. Nevertheless, like the split-brain subjects, he successfully reported more left- than right-ear digits at lower levels of task difficulty before shifting dramatically and almost entirely to right-ear reporting at the higher levels. How could two mindless pathways induce a cognitive shift as the task got harder? And where had the initial "right hemisphere" advantage come from?

Henninger showed these data, and the results of an earlier music test she had given the hemispherectomy subject, to Sperry, who came up with an interesting interpretation. He suggested that just as the contralateral pathways led to the left hemisphere, the ipsilateral pathways led to a secondary center within the left brain that constituted a "pseudo" right hemisphere.

In investigating this theory,

Henninger's first step was to determine whether the left-to-right-ear cognitive shift observed in the clinical subjects was unique to non-intact brains or occurred also in subjects with intact brains. To examine this, she administered dichotic tests to 41 normal subjects. Computer analysis and tabulation of the raw data was performed by Tad White, a math major and SURF (Summer Undergraduate Research Fellowship) student working on the project.

According to the data, 46 subjects and nearly 46,000 digits agree — cognitive factors outweigh perceptual ones in inducing ipsilateral suppression in the left hemisphere. Normal subjects showed a significant increase in the relative right-ear advantage as the task became harder, although their threshold of difficulty, not surprisingly, was considerably higher than either the splits' or the hemispherectomy subject's.

On the basis of these findings, Henninger concludes that the engagement of the left hemisphere in verbal dichotic listening depends on the extent to which cognitive tasks use left-brain resources. The test is currently being given to a new round of subjects to further substantiate this hypothesis. She is also developing a new listening test to examine whether this model holds true for functions associated with the right hemisphere.

To examine the hypothesis of a secondary center and its possible features, Henninger turned again to the hemispherectomy subject. With the volume up in the left ear, he was asked to report left-ear digits only. During the practice trials (numbers 1 through 12, less 7 and 11, presented to both ears), instead of writing 8 after 6, he wrote 7, suggesting, says Henninger, that he was unable to focus on the (ipsilateral) input and was guessing at the meaning of sounds he could hear but not interpret. He then reported 8, 9, and 10 for 9, 10, and 12, leading her to believe he was recalling from earlier testings that the practice trials were sequential. On the first dichotic pair that presented different digits to each ear, he reported a single right-ear digit, further evidence that he could not focus on the ipsilateral input. For the rest of the level-one trials, he wrote down digits to both ears, indicating that despite his aim to report only left-ear data, he could not suppress contralateral input to the right ear.

When the test was repeated, the subject identified the practice digits correctly, and reported left-ear digits only at the "ones" level but shifted to the right-ear digits as the task load increased. This test clearly showed his lack of voluntary control over the shifts.

In another testing, whose results Henninger considers particularly significant, the subject was asked to report only right-ear digits, with the volume raised in the left ear. During the practice trials in which the same numbers are presented to both ears, he said "I can't. They're all coming in this (pointing to his left) ear."

On the first dichotic trial, he still didn't distinguish the right-ear digit (10), but reported a left-ear 9 for a 5 (a common error) — behavioral evidence that he was accurately able to identify the sound's location. On the second trial, however, he said, "Oh, now it's coming in this (right) ear," and reported the right-ear digits for the remaining trials.

Henninger's analysis is that initially the higher volume in the left ear shifted him to the ipsilateral pathways and secondary center, but as soon as he perceived competing numbers, he could distinguish different locations in space, and his intent to report only right-ear digits engaged the contralateral center. The fact that his awareness of the input's locale shifted along with his switch from left- to right-ear input points to the existence of two distinct centers within his lone hemisphere, oriented toward opposite sides of space. Furthermore, the proposed ipsilateral center appears clearly oriented to the left side, normally the domain of the right hemisphere.

Finally, in monaural testing of the left ear, on the practice trials, the subject accurately reported digits 1 through 10, skipping 7. On the final trial, however, he wrote a second 10, then changed it to 11, apparently recalling that there were not two 10's in a row. Later, during the first four dichotic pairs he reported a digit order that was similar but not identical to the actual test ("1,3,2,3" for "10,2,3,2"), then shifted to left-ear digits and finished the rest of the test without difficulty, correctly identifying left-ear digits.

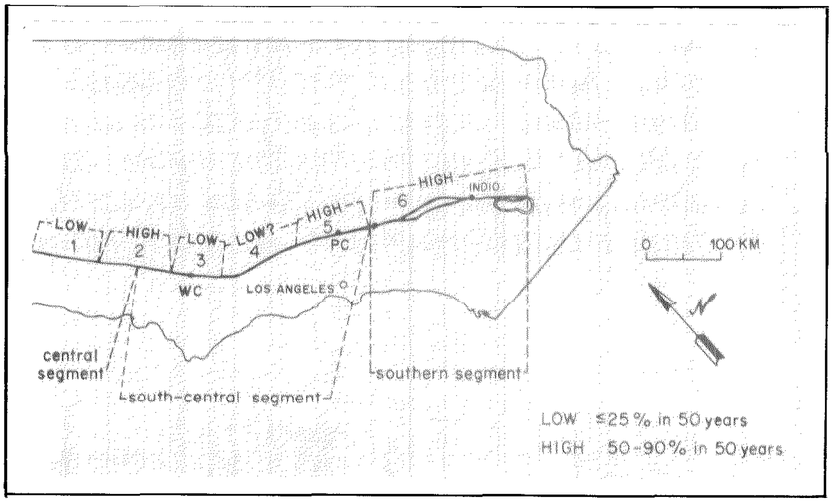
Henninger's conjecture is that initially the subject could not locate the center for the incoming left-ear digits, and was instead in the major hemisphere's area of long-term memory and recalling data from earlier tests. However, once he realized these digits were not what he was hearing, and that the information he needed was unavailable, he shifted to the subsidiary center, where the ipsilateral input from the left ear could be easily identified.

The subject's ability to identify ipsilateral input when the contralateral was out of the picture suggests that the secondary center is as capable as the primary center of processing the full sequence of numbers. The difference appears only when they are in competition. Henninger surmises that both centers are tied in to the cerebral equivalent of a mainframe processor, which performs the actual cognitive activity. However, the contralateral center has preferred access, and the subsidiary ipsilateral center cannot get online while this unit is monopolized by the primary center.

Henninger's thesis, of course, raises enough questions to keep any central processor tied up for quite a while. She is currently working on a new series of tests designed to help distinguish between secondary center functions and those that can only be performed by the more powerful contralateral hemisphere. Another area to be explored is whether the subsidiary hemisphere is capable of complex cognitive functions, and whether these are functions commonly associated with the opposite primary hemisphere.

There is also the intriguing question of whether secondary center activity is unique to clinical subjects or is a widespread phenomenon. In normal subjects, Henninger speculates, the secondary center(s) may function as a rarely used backup system. On the other hand, she says, the evidence also supports the possibility that intrahemispheric activity is a standard feature of the normal brain that up to now has been exclusively interpreted as inter-hemispheric behavior. What does seem certain is that if new research reinforces and extends Henninger's findings, current theories of lateralization could themselves be in for a major shift. □ — *Heidi Aspaturian, Publications*

Quake Forecast



The map above (with California lying on its side) shows potentials for earthquakes along the San Andreas fault over the next 50 years.

AFTER SEVEN YEARS of excavation and geological analysis of the San Andreas fault, Kerry Sieh, associate professor of geology, has developed a clear enough picture of that complex structure to attempt forecasts of which portions are likely to rupture next and which are not. He has recently revealed new data and given a comprehensive overview of the potential for fault movement from central California to the Mexican border.

Sieh's forecasts of the future of the San Andreas are based largely on studies of sediments and landforms that record the prehistoric behavior of the fault (*E&S,* April 1981). Radiocarbon dating of these disruptive features has proven critical in dating the ancient earthquakes that resulted from these dislocations.

A superquake the entire length of the San Andreas probably can be ruled out, said Sieh, because the central reach of the fault creeps annually at a rate equal to the movement of the fault segments to the north and south. This creeping segment of the fault (marked 1 on the map) effectively isolates the northern reaches of the fault near San Francisco — which last broke in 1906 — from the southern parts, much of which last broke in 1857.

The short segment of the fault (2 on the map) centered on Cholame, however, is likely to produce magnitude 6 to 7½ earthquakes before the turn of the century. "This segment has historically been a zone of transition between the fully creeping and fully locked portions of the fault," said Sieh. "Based on its previous behavior, this segment is a prime candidate for generating a large earthquake in the near future." Fortunately, the region transversed by this volatile segment of the fault is only very lightly populated.

Another far more dangerous segment is the southernmost 300 kilometers of the fault. The probability of a large earthquake within the next 50 years along this stretch (5 and 6 on the map) is between 50 and 90 percent, Sieh estimated. The effects of such an earthquake, which might range between magnitude 7½ and 8½, would be quite severe in Los Angeles, San Bernardino, Palm Springs, Indio, and nearby communities.

Sieh's new excavations across the fault at Pallett Creek (PC on the map), near Palmdale, have revealed evidence that large earthquakes occur about every 145 years on the average, the latest being 127 years ago in 1857. This is a refinement of data collected and reported several years ago. These earthquakes have typically occurred after 3 to 4½ meters of strain have been stored in rocks adjacent to the fault. This is about the amount that appears to have accumulated there since 1857.

Sieh bases his conclusions that portions of the San Andreas can be ruled out as near-term large earthquake sites on his studies of the offset of Wallace Creek (WC), a stream channel crossing the fault in the region east of San Luis Obispo, about halfway between San Francisco and Los Angeles. Over the past 13,500 years, the channel has been offset by about 475 meters, yielding an average slip rate of about 35 millimeters per year. Other measurements show that within the past 3,700 years, about 130 meters of slip has taken place per year (mm/yr) for this time period as well.

"Small channels in this area indicate that when the fault slips there it slips suddenly," Sieh said, "with shifts of about 10 to 13 meters." For example, the latest great earthquake in southern California, in 1857, was associated with about 10 meters of offset near Wallace Creek. Sudden shifts of such size would be spaced 240 to 450 years apart. "At the rate we believe strain to be accumulating, it will be at least a century before the area around Wallace Creek is likely to rupture again," he said.

Sieh's warning that the San Andreas fault is not the only important player in the seismic future of California stems from studies of the movement of the San Andreas compared to the overall relative movement of the Pacific and North American plates. Over the past several millenia, the San Andreas fault has moved at a rate of only 35 mm/yr, which represents only about 60 percent of the total relative movement of the two plates. Other large faults, in particular the San Gregorio-Hosgri, must make up the 200 mm/yr deficiency. This fault parallels the coast offshore from Point Reyes, north of San Francisco, to Point Conception, west of Santa Barbara.

Sieh and his students are now engaged in studies along the southern reaches of the fault, near Indio, where the fault has not broken during the entire two centuries of recorded history and where considerable strain is thus believed to be stored. His research is sponsored by the U.S. Geological Survey.□ — *Dennis Meredith, News Bureau*

The Equilibrium Solution

Rapid, reliable methods for solving chemical equilibrium equations have long been sought by scientists asking fundamental questions about systems as varied as the atmosphere, the human body, and the internal combustion engine. An interdisciplinary collaboration at the General Motors Research Laboratories has produced a breakthrough with potentially universal applications.

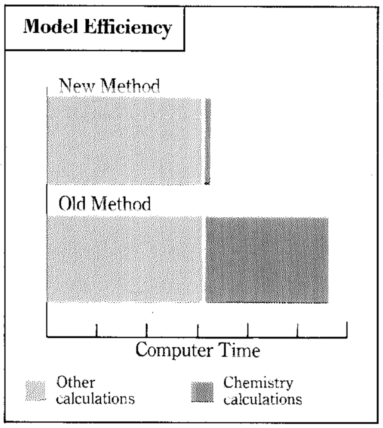


Figure 1: Computer time required by an engine combustion model. Time required for chemical calculations decreased greatly with the new methodology.

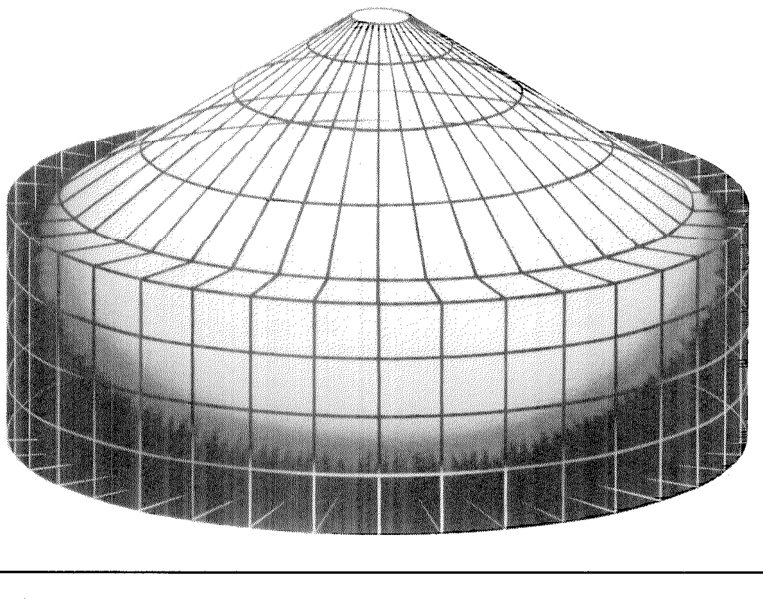


Figure 2: Artist's illustration of a chemically reacting flow. The physical space is divided by a latticed network into units of volume, and the solution must be recalculated for each grid point at each instant of time.

WHEREVER CHEMISTRY is involved, the need to solve chemical equilibrium equations arises. Although methods for solving such equations have existed for some time, they do not offer the speed demanded by the most challenging problems. For example, predicting the composition of gases inside an engine cylinder may require as many as a million equilibrium calculations per cycle. Two researchers at the General Motors Research Laboratories have developed a systematic way to reduce the mathematical complexity in these problems, thus making it possible to solve them rapidly.

Chemical equilibrium occurs when the rates of a forward and reverse reaction are equal. Mathematically, this statement usually translates into a system of nonlinear polynominal equations. Until now, there has been no fast reliable method for solving such systems. Solutions to particular problems have demanded thorough familiarity with the physical conditions. In most cases, this means partial knowledge of the answer.

Dr. Keith Meintjes of the Fluid Mechanics Department and Dr. Alexander Morgan of the Mathematics Department began their research by considering recent advances in the theory of continuation methods. They concluded that a suitable continuation algorithm could be relied on to solve the nonlinear polynomial equations that make up chemical equilibrium systems. In this insight lies the realization that the solution can be obtained without any knowledge of the physical nature of the problem.

In seeking the most efficient implementation of the continuation method, the researchers discovered that chemical equilibrium equations can always be systematically reduced to a substantially simpler mathematical form. The reduced systems have fewer unknowns and a smaller total degree. The total degree of any system is the product of the degrees of each of its equations. Reducing the total degree makes a system easier to solve. A typical combustion problem with ten equations and total degree of 192 was reduced by the researchers to two cubic equations with a total degree of nine.

The reduced systems can then be systematically scaled to fit within the limits imposed by computer arithmetic. The range of coefficients in chemical equilibrium systems tends to be too large or too small for the arithmetic of the computer. Consequently, the solution process can fail. By construction of an effective scaling algorithm, this arithmetic constraint can be eliminated. Suitably reduced and scaled, the equilibrium systems can then be solved reliably by the continuation method.

THUS, Drs. Meintjes and Morgan accomplished their original goal of developing an innovative reliable approach to solving chemical equilibrium equations. They also made a final, unexpected discovery. Certain standard solution techniques, which fail on the original systems, can be made absolutely reliable when applied to the reduced and scaled systems. These methods, which are variants of Newton's method, are also many times faster than continuation.

This research has produced an extremely effective solution strategy—reduction of the equations, followed by scaling of the reduced systems, followed by the application of a suitable variant of Newton's method. The simplification of the systems, which was originally formulated to facilitate the implementation of the continuation method, proved to be the critical factor enabling the use of fast techniques.

In one application, the chemical equilibrium calculations are part of a model which predicts details of the flow, turbulence, and combustion processes inside an engine. By using their methodology to develop an equilibrium solver for this application, the researchers greatly increased the model's solution efficiency (see Figure 1).

"It was the characteristic structure of equilibrium equations," says Dr. Meintjes, "that allowed us to perform the reduction. The unexpected mathematical simplicity of the reduced systems suggests that even more efficient solution methods may be discovered."

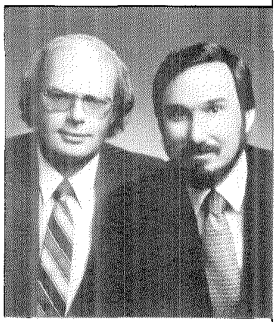
"Critical to this research," says Dr. Morgan, "was the dialogue between disciplines. I hope that this dialogue will continue as scientists and engineers in diverse fields explore the capabilities of this new methodology."

General Motors

GM

MARK OF EXCELLENCE

THE MEN BEHIND THE WORK

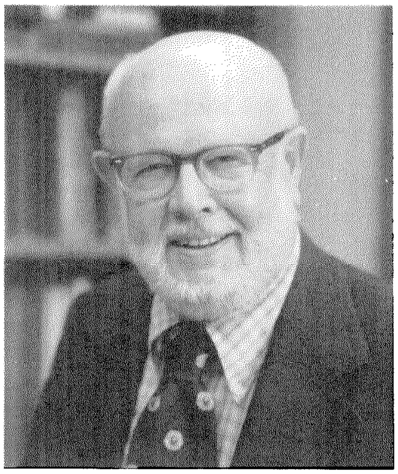


Dr. Keith Meintjes, a Staff Research Engineer in the Fluid Mechanics Department, joined the General Motors Research Laboratories in 1980. Dr. Alexander Morgan, a Staff Research Scientist in the Mathematics Department, joined the Corporation in 1978.

Dr. Meintjes (left) was born in South Africa. He attended the University of Witwatersand, where he received a B.Sc. and M.Sc. From 1973 to 1975, he taught fluid mechanics and engineering design at the university. He then went on to study at Princeton University, where he received an M.A. and Ph.D. in engineering. His doctoral thesis concerned numerical methods for calculating compressible gas flow.

Dr. Morgan (right) received his graduate degrees from Yale University in differential topology. His Ph.D. thesis concerned the geometry of differential manifolds. Prior to joining General Motors, he taught mathematics at the University of Miami. His book, "Applications of the Continuation Method to Scientific and Engineering Problems," will soon be published by Prentice-Hall.

Opinion



by William A. Fowler
Nobel Laureate
Institute Professor of Physics, Emeritus

The following remarks were excerpted from the Oral History Project of the Caltech Archives.

As physics becomes more and more sophisticated, requiring larger and larger facilities, it seems almost inevitable that physics is going to be done in big central locations. The trend is to form university teams, or users' groups, working at large national laboratories; the federal agencies claim they cannot continue to support expensive projects at individual universities. This has already happened in elementary particle physics; you just cannot perform the actual experiments at a university anymore. The Caltech synchrotron was shut down years ago along with many other on-campus university installations, and elementary particle physicists now do their work at CERN or DESY in Europe, or at Fermilab, SLAC, or Cornell in the United States. What goes on at SLAC (Stanford) or Cornell is not on-campus research in my use of the term.

Now the same thing is happening in nuclear physics. There is enormous pressure to cut down on National Science Foundation support for accelerator groups. University accelerator labs have been closed down all over the country. Kellogg Laboratory here at Caltech is an exception. At 52 years old it is one of the last ones left, and it has continued to be enormously successful. I can't really complain about the tremendous amount of support for our work there. When we decided that we needed a new low energy accelerator a few years ago, the NSF provided a million dollars, and Caltech built a new million-dollar laboratory for us.

Kellogg will continue to do low energy nuclear astrophysics, using established techniques to accumulate more and more information. But low energy nuclear physics is no longer quite the glamorous subject it once was, and younger people entering the field now are attracted to the intermediate energy accelerators that exist only at national laboratories such as Los Alamos and the new electron accelerator proposed for construction near Norfolk, Virginia. It's thought to be more "exciting" than the work in low energy physics as applied to astrophysics.

National labs may well be an efficient use of resources, but the trend still worries me. I think the trend should not be allowed to happen just by default without at least giving serious thought to the consequences. What will happen is that university campuses will become places where research is done in chemistry, geology, biology — but more and more branches of physics will have to be done at the big central installations. Hands-on physics research — research with actual results — will disappear from university laboratories. Yes, users' groups still do use university laboratories; they build a lot of equipment on campus before taking it to the national labs. But that's a completely different mode of operation from graduate students actually doing their work and getting their results at a university. If graduate students do their course work in a couple of years and then disappear to Fermilab for three years to do their theses, I think this is going to change the whole character of university research, and I'm not sure it's for the better.

I think we've *got* to keep physics alive in the university laboratories. In the system that has been developed in the United States since World War II, physics is done in a three-way partnership — in universities, in industrial labs, and in national labs. All three have made substantial contributions, and for us to give up one of them may turn out to be disastrous. The comparison that always comes up is with the Soviet Union, which may not be a very good example since they don't have any industrial labs anyway. But they also have pratically no laboratories in their universities. Students go to the university to learn graduate work and then to one of the big institutes of the Soviet Academy of Sciences to do experimental work. And, quite possibly as a consequence of this, while the Russians are tops in theoretical work, the contributions in experimental physics that have come out of the Soviet Union in the last decade have not been first class in my opinion.

The Nobel Prize that was awarded to me last year was essentially an award to the Kellogg Laboratory. I am convinced that I was chosen among a great number of other candidates because of the *experimental* work (on the nuclear reactions that produce the chemical elements in the universe) performed in Kellogg by Charles Lauritsen, Thomas Lauritsen, Charles Barnes, Ralph Kavanagh, Tom Tombrello, Ward Whaling, myself, and our many graduate students and postdoctoral fellows. I now want to use any influence I have to urge the National Science Board and the Department of Energy to conduct a study of the funding of on- campus university laboratories and then decide what is best for future generations of physicists. □

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