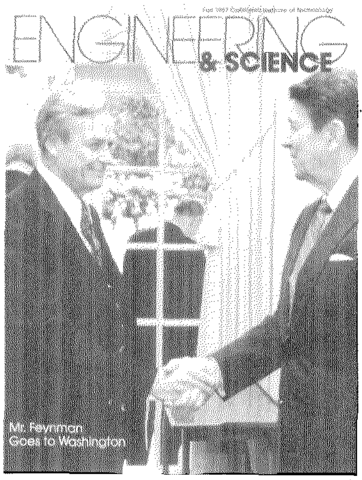
Fall 1987 California Institute of Technology

ENGINEERING & SCIENCE

Mr. Feynman
Goes to Washington

In This Issue



Surely He's Joking . . .
On the cover — President Ronald Reagan thanks Richard Feynman for serving on the presidential commission to investigate the Challenger disaster, in which the Space Shuttle exploded after takeoff, killing all on board. This meeting occurred in June 1986 as the commission presented its final report to the President in a ceremony in the White House Rose Garden.

Appended to that report was Feynman's own special report — a sort of dissenting opinion based on his own investigations. His irreverent account of those investigations, his dealings with engineers, managers, and government officials (as well as the now-famous ice water experiment), appear in "Mr. Feynman Goes to Washington," which begins on page 6. The article is adapted from a talk given to the Caltech Management Association last May.

Feynman, the Richard Chace Tolman Professor of Theoretical Physics, has been a member of the Caltech faculty since 1950. He won the Nobel Prize in 1969 and is more recently famous as author of the 1986 bestseller, *Surely You're Joking, Mr. Feynman.*

Fewer but Fatter
Beginning with this issue *E&S* will appear quarterly instead of five times per year. But no one is getting gypped. It will be bigger, 44 pages instead of 32 — a net annual gain of 16 pages.

Other Authors
In April last year the American Chemical Society presented the Priestley Medal, the country's highest award in chemistry, to Jack Roberts, one of the pioneers of nuclear magnetic resonance (NMR) applications to chemistry and biochemistry. His acceptance speech on that occasion, "Priestley and me," is reprinted here beginning on page 28. Roberts, who joined the Caltech faculty in 1952, is now the Institute Professor of Chemistry, and he has also served as chairman of the Division of Chemistry and Chemical Engineering and as provost and vice president of the Institute.

Paul MacCready's innovative thinking has produced human-powered airplanes and re-created giant flying reptiles, but these projects have also drawn his attention and concern to the larger issues of technology's role in civilization and its survival. His article about these issues and what an individual can do about them — "Technology, in Perspective and under Control," starts on page 23. MacCready is an alumnus of Caltech (MS '48, PhD '52). The most recent project of his firm, AeroVironment (in collaboration with GM and Hughes) is the solar-powered car, Sunraycer, which will compete in the Australian transcontinental World Solar Challenge next month. *E&S* hopes to publish an article on Sunraycer in a future issue.

Warm Welcome
In September Caltech welcomed Tom and Doris Everhart with a round of receptions, and in October the Pacific plate greeted them with a 6.1 earthquake, which left some of the bricks of their new residence scattered on the lawn.

Everhart is no stranger to earthquake country, however. His years as chancellor of the University of Illinois at Urbana-Champaign and dean of Cornell's College of Engineering were preceded by 20 years at Berkeley and in the 1950s graduate school at UCLA. A number of Caltech faculty, past and present, have known him since his years in California and were willing to offer their recollections and opinions for the article that begins on page 2.

More about the earthquake can be found on page 45.

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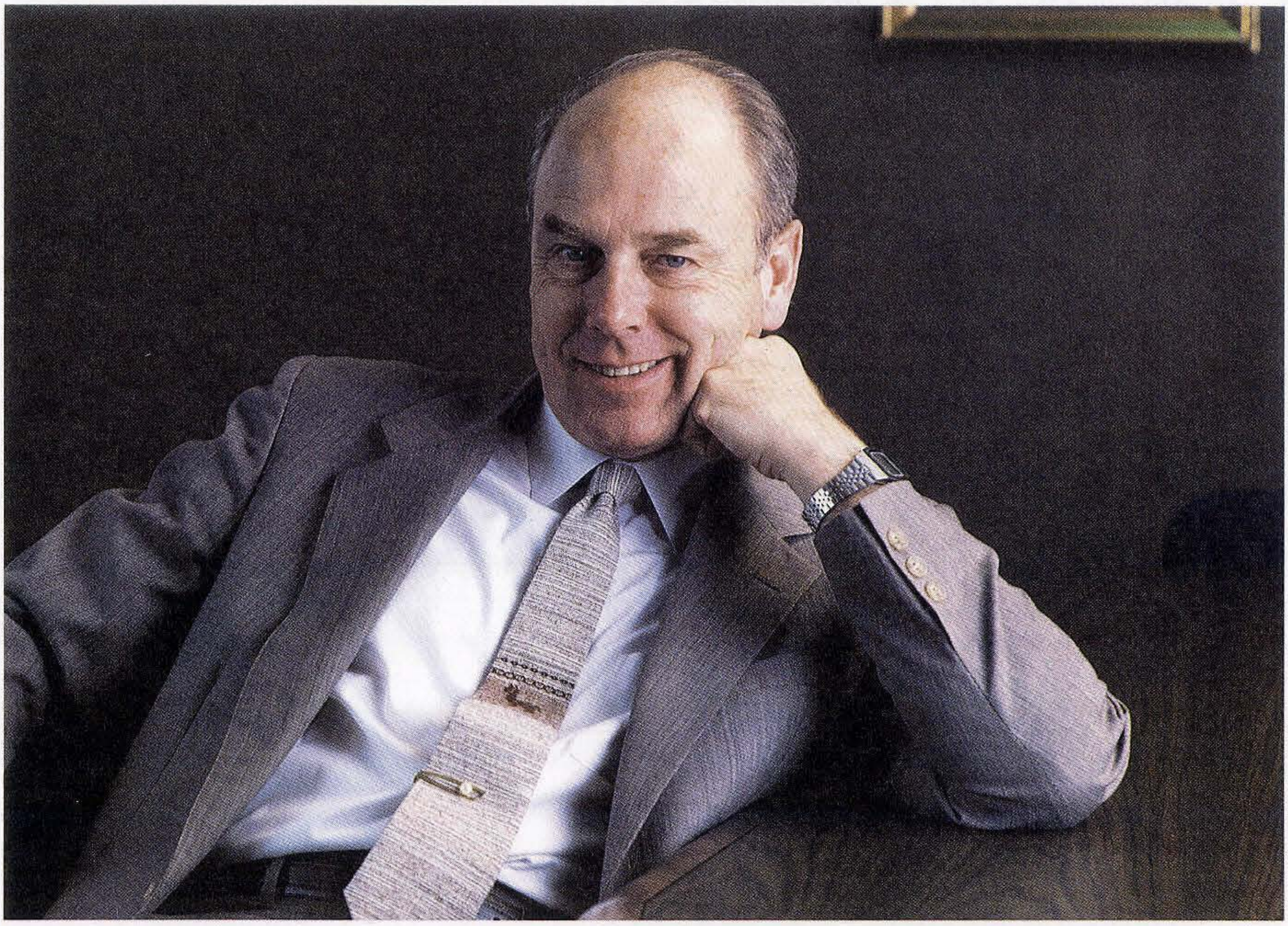


1987 Paul Merideth:CLICK/Chicago

Caltech's New President

Tom Everhart, former chancellor, dean, department head, and professor of electrical engineering, starts the new academic year — and a new era.

THOMAS E. EVERHART began his new job as Caltech's president on September 8. His timely arrival marks the happy outcome of an ideal search and also represents a little bit of luck. The search committee for a new president had been formed last year after "Murph" Goldberger announced his decision to retire as Caltech's president by June 1988. But by last February, when Goldberger was appointed director of the Institute for Advanced Study in Princeton, effective September 1987, the search committee had only pared its original list of some 175 candidates down to a "manageable number" — 15 to 20 names.

Don Cohen, professor of applied mathematics, led the seven-member faculty search committee. "Caltech is exceptional among educational institutions in the substantial role its faculty plays in choosing a president," according to Cohen. At most places the trustees do the job essentially alone. The Institute faculty and trustee search committees were in constant contact during the search ("We're privileged at Caltech to have superb relations with the trustees," says Cohen), and it was decided that in the end the faculty committee would present a short list of three to five names to the trustees. That seemed possible, but by late spring the committee members could not imagine that they would actually be able to get anyone here by fall. "We wanted the best person even if we had to wait," says Cohen. Fortunately they didn't have to.

At the time of the announcement of Goldberger's imminent departure, the committee was rolling along pretty well, even though "none of us had ever hired a president before," says Cohen. Their task was a bit easier (or perhaps a bit harder) because Caltech's requirements are somewhat specific. No rule existed that the president had to be a scientific person, but it was certainly highly probable that he would be, because the committee was looking for someone with a deep commitment to science and an understanding of it. But the prime requirement on the committee's list of criteria was academic and intellectual ability that would command the respect of the Caltech community. "Otherwise, our faculty would just chew him up," says Cohen.

Everhart's background leaves him in no danger of getting chewed up. After earning his AB in physics from Harvard in 1953, he came out to the other coast for graduate school. While at UCLA Everhart also worked at the Hughes Electron Tube and Microwave Laboratory in Culver City on the Hughes Fellowship Program. Also working at Hughes was Roy Gould, then earning his PhD at Caltech and now the Simon Ramo Professor of Engineering. (Both continued to consult at Hughes for many years thereafter.) That particular lab was a very exciting place to be in the mid-1950s, because it was in the vanguard of research on microwave devices, and "people were involved in some really interesting things," recalls Gould. The problem Everhart was working on — a backward-wave amplifier — was an exceedingly interesting one, making him one of the pioneers in that area even as a grad student, according to Gould. It was from Gould that Everhart got his first impressions of Caltech —"a small school over in Pasadena — very smart students."

After Everhart earned his MSc degree in applied physics from UCLA in 1955, a Marshall scholarship took him abroad to Cambridge University for his PhD in engineering, which was granted in 1958. At Cambridge he was one of the early students of and collaborators with Sir Charles Oatley, pioneer in the development of scanning electron microscopy. This technology, which sweeps a beam of electrons over a surface and then measures the intensity of the electrons bouncing back (secondary electrons), can produce a high-magnification, high-resolution image of that surface in relief. The Everhart-Thornley detector, a secondary-electron detector, is a principal component of the imaging electronics of the scanning electron microscope.

Broadly described, Everhart's field is "physical electronics," according to Bill Bridges, who considers him more of an applied physicist, even though Everhart went to UC Berkeley in 1958 as an assistant professor of electrical engineering. Bridges, now the Carl F Braun Professor of Engineering at Caltech, and Amnon Yariv, the Thomas G. Myers Professor of Electrical Engineering and professor of applied physics, were both graduate students at Berkeley when Everhart got there. Since they were all about the same age, Yariv wasn't sure at the time whether Everhart was a faculty member or another grad student.

Bridges and Everhart shared two sections (and lecture notes) of the course, "Introduction to Communication." Their backgrounds fortunately complemented each other. "Tom was helpful to me in teaching the newly invented transistor, which had just been introduced into the engineering curriculum," says Bridges, "and I helped him with some of the communication ideas."

During his two decades at Berkeley, Everhart's research represented a consistent thrust in advanced electron beam applications. His early contributions to the understanding and development of scanning electron microscopy (particularly for semiconductor device applications) and to the physics of electron beam energy loss and scattering were of enormous importance. He was one of the first investigators of electron beam lithography (which uses a focused beam of electrons to etch patterns on microdevices such as semiconductors), an early and active proponent of computer-controlled scanning electron beam systems, and co-inventor of the erasable electron beam addressable MOS memory. He also did a comprehensive analysis of the electron optics of the field-electron emission source for scanning electron microscopy.

Everhart became associate professor at Berkeley in 1962 and full professor in 1967. He served as chairman of the department of electrical engineering and computer science from 1972 to 1977, during which time all of computer science was consolidated under its jurisdiction. Robert Middlekauff, director of the Huntington Library, knew Everhart at Berkeley in the early 1970s, when both served on a university committee that dealt with budget and faculty personnel issues. It was a small committee of "great spirit and closeness," says Middlekauff, and its members, from various parts of the university, got to know each other well. He remembers Everhart as a very cultivated person, "sympathetic to activities and fields outside his own and interested in a whole range of scholarship. He had a fine reputation on the Berkeley campus, both as a scientist and a colleague. Everyone thought well of him."

Everhart's administrative experience, which began with the department chairmanship at Berkeley, met another requirement that was high on the presidential search committee's list of necessary qualities — proven ability as an administrator. In 1979 he left Berkeley to become dean of Cornell's College of Engineering, bringing with him a new openness to the college administration, according to Ed Wolf, professor of electrical engineering there. Everhart played an important role in the development of the National Research and Resource Facility for Submicron Structures there (now the National Nanofabrication Facility). This interdisciplinary laboratory was established by the National Science Foundation as a collaboration among government, academia, and industry to catalyze the exploration of submicrometer science and technology — exploring the limits of miniaturization.

Wolf, who is also director of the National Nanofabrication Facility, has known Everhart for 20 years, starting with collaboration at Hughes. When the Cornell facility played host to a group of scholars from the People's Republic of China, Everhart and Wolf together acted as advisers to the visitors, discussing their research plans with them every Saturday morning. Everhart also traveled to China with Cornell's president to secure scholarly exchange agreements with the Chinese.

"At Cornell he turned outward as well as inward," says Wolf. "He was willing to be committed to making things happen on a national level."

Perhaps the most telling assessment of Everhart as an administrator comes from Jim Mayer, former professor of electrical engineering and master of student houses at Caltech, who arrived at Cornell at about the same time as Everhart, and has also known him for about 20 years. ("*Even* Jim Mayer likes him," says Carver Mead, the Gordon and Betty Moore Professor of Computer Science, "and Jim has less use for administrators than any human being I have ever known.") At Cornell there wasn't an opportunity for Mayer and Everhart to work together, but "we talked in depth about particle and solid interactions, and his suggestions were always germane to the issue," says Mayer. They also talked a lot about "education and the role of undergraduate and graduate students in science and technology."

"He's a marvelous person to have as a colleague," says Mayer, "incisive, knowledgeable. He's absolutely a bright cat."

From Cornell Everhart went on to become chancellor of the University of Illinois at Urbana-Champaign in 1984. During his tenure there (before his discovery by the Caltech search committee), two national supercomputing centers were established, and the campus was named the site of a federally funded biotechnology building; construction began on the Arnold O. and Mabel M. Beckman Institute for Advanced Science and Technology and on the Kinkead Pavilion of the Krannert Art Museum.

Everhart had a serious commitment to the University of Illinois. But Caltech, which offered, among other things, a national platform on issues of science and technology, as well as the autonomy of a less complex and more focused institution, was very attractive to him.

And the attraction was mutual. Besides meeting all the criteria the committee had established, Everhart impressed the committee members with his astute perceptions. "He's a good listener, but he had interesting things to say and asked us some terrifically good questions," says Cohen. "It was clear he had the sense of values and the gut instincts that the Caltech faculty looks for."

When Cohen's committee presented its short list to the trustees, it didn't have to recommend one in particular. "But we did," says Cohen. "We said we really liked Everhart best." After conducting their own interviews, the trustees concurred. "There was complete harmony between the trustees and the faculty," Cohen adds.

Those who know Everhart praise his integrity, honesty, forthrightness, fairness, thoughtfulness, and good sense. As Carver Mead puts it, "He has a lot of appreciation for science, but he honest-to-God has his feet on the ground."

Mead also tells an anecdote that illustrates an adventurous side of the new Caltech president. In the late 1970s he and Everhart were part of a task force assigned to travel around the country conducting an assessment of the future of lithography as applied to microstructures. Mead credits Everhart with much of the insight that came out of that trip ("he knew what to look for and how to think about it"), but the incident that Mead remembers best involved getting stuck in Boston in a terrible snowstorm. "We had to get to Yorktown Heights, New York, and the planes weren't flying and the trains weren't running. So Tom says, 'Let's drive!'" And drive they did, not even stopping to put on chains. "It was really coming down, but Tom had a ski cabin in the mountains and knew how to drive in the snow," recalls Mead, laughing.

And the message to Caltech from Jim Mayer is: "You will have fun." □ — *JD*

Mr. Feynman Goes to

by Richard P. Feynman

(edited by Ralph Leighton)

I WAS INVITED TO WASHINGTON to investigate the Challenger accident, which you presumably all know about.

First of all, NASA has many projects. In this lecture I'm going to use the word "NASA" always to mean just that work associated with the Shuttle, and I don't imply any other connections.

Before I tell you about the Shuttle, I thought it would be interesting to you all to see the costume that I assumed in order to move among the natives without being too conspicuous in Washington. They wear this kind of coat because it's a little bit cold there — there's snow sometimes. They *think* it's because it's cold there, but, as a matter of fact, they wear such coats on the inside of their buildings, which are well heated.

Further, it turns out that you can put this coat on to walk short distances — from one building to another — or from a building to a taxi, if it's any longer distance. However, they are not satisfied with this. They seem to have a strange fear of the cold, because on top of this they put other coats if they wish to step outside. Now that you've seen the equipment, I'm going to take it off.

This briefcase is not quite accurate. It's what *they* have, and I tried buying one in order to complete my disguise when I first started out. But I discovered, first, that they're expensive, and second, that they can't contain a great deal of material. So I bought instead a kind of soft-covered traveling case which carried enough stuff so that when I'd leave, I could have everything in my case — whereas they all left with their cases under their arms, carrying big books in their hands.

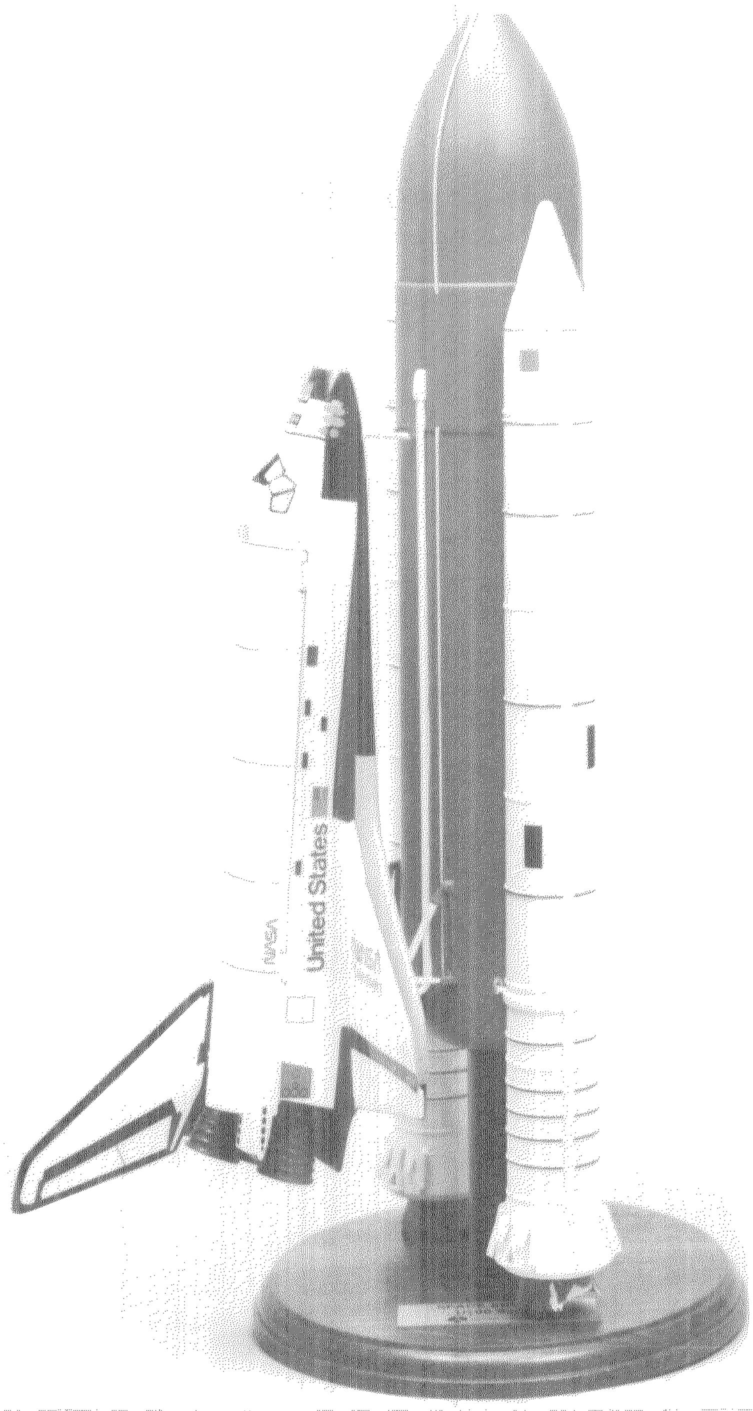
To remind you for just a moment about

Feynman's special report on the reliability of the Shuttle, which appeared as an appendix to the commission report, may be obtained by requesting a copy from E&S.

Washington

the Shuttle (below), the central part is the tank for fuel (liquid hydrogen and liquid oxygen); the engine, which burns that fuel, is at the back end of the orbiter, which looks something like an airplane. The crew sits in the front of the orbiter. In order to boost the Shuttle in the beginning, there are two solid-core rockets, called "SRBs" (for solid rocket boosters). They are ignited for about two minutes before they are discarded and later recovered in the sea. As most of you know,

For liftoff the Shuttle is joined to a large tank (the central portion) containing liquid fuel. On either side of the tank are the solid rocket boosters, one of which failed, causing the Challenger disaster.



the story is that one of the SRBs failed. There was a leak in a joint between two sections. Hot gas leaked out of the joint and ultimately burned a hole in the side of the tank where the hydrogen was, and the flight was a failure.

I'm making this part of my talk relatively short, because most of you already know this. There's putty and other things, but the ultimate seal is supposed to be two rubber rings, called O-rings, which are approximately a quarter of an inch thick and lie on a circle 12 ft. in diameter — that's something like 37 ft. around (top right). When the SRB was originally designed by the Morton Thiokol Company, it was expected that the pressure from the rocket would squash the O-rings so the joint would be securely sealed. What happened instead is, the joint is stronger than the wall (it's three times thicker), so that under pressure the wall bows outward, causing the joint to open a little — enough to lift the rubber O-rings off the seal area. This phenomenon is called "joint rotation" in the lingo the engineers use, and it was discovered very early, when they were still designing, before the Shuttle flew.

Although the pieces of rubber are called O-rings, they're not used the way O-rings are normally used. In ordinary circumstances, such as for sealing oil in the motor of an automobile, although there are sliding parts and rotating shafts, the gaps are always the same. An O-ring just sits there in a fixed position. But in the case of the Shuttle, the gap *expands* as the pressure builds up in the rocket. And to maintain the seal, the rubber has to expand fast enough to close the gap — in fractions of a second. Thus the resilience of the rubber became a very essential part of the design. When the Thiokol engineers were discovering these problems, they went to the Parker Seal Company, who manufactures the rubber, to ask for advice. The Parker Seal Company told Thiokol that O-rings were not meant to be used that way, so they could give no advice.

Although it was known from nearly the beginning that the joint was not working as it was designed to, Thiokol kept struggling with the device. They made a number of makeshift improvements. One was to put shims in to keep the joint tight (bottom right). At first they thought they would adjust each shim to the right thickness as they went around (the rocket would become slightly out of round after each use), but that was expensive, so they made all the shims the same thickness. Of course, it wasn't enough. The joint still leaked, and they were thinking how to fix it, and the Shuttle kept flying. That is one of things you have to understand: The program kept going, no matter what.

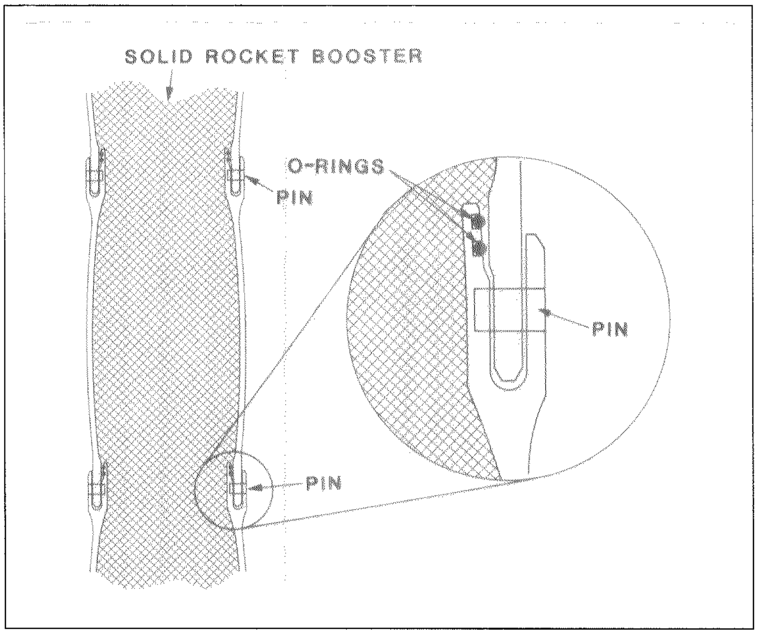
Now I want to tell about my own experiences in connection with this. A few days after the accident, on a Friday, I got a call from William Graham, who was the Acting Director of NASA. Mr. Graham had been a student of mine — at Caltech, and also at the Hughes Aircraft Company, where I gave a series of lectures — and thought maybe I would be of some use to the investigation. When I heard it would be in Washington, my immediate reaction was not to do it. I have a principle of not going anywhere near Washington or having anything to do with government.

So I called various friends like Al Hibbs and Dick Davies, trying to find an excuse why I shouldn't accept, but they all said I should. Then I spoke to my wife. "Look," I said. "Anybody could do it. They can get somebody else." "No," said Gweneth. And she explained how she thought I would make a unique contribution — in a way that I am modest enough not to describe. Nevertheless, I believed what she said. So I said, "OK. I'll accept."

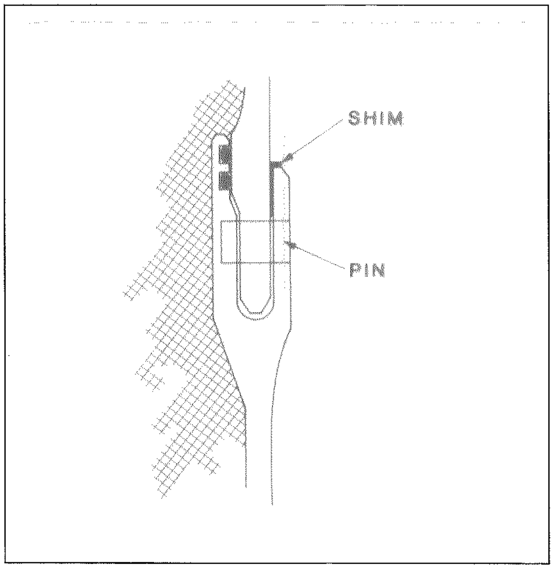
So on Sunday, as I went to the telephone to call Mr. Graham, I announced to Gweneth, "I'm going to commit suicide for six months. I won't be able to do any work with this physics problem I've been having fun with; I'm going to do nothing but work on the Shuttle — for six months." I want you to understand my attitude at the time: I hadn't realized that it would take two years to get the Shuttle flying again. I was going to try to work very hard so we could get everything straightened out as quickly as possible.

The next day, Monday, I got a telephone call at 4 pm: "Mr. Feynman, you have been accepted onto the commission" — which by that time was a "presidential" commission, headed by former Secretary of State William P. Rogers. The first meeting would be in Washington, on Wednesday. So Tuesday, I asked Al Hibbs to get people at JPL who knew something about the Shuttle project to brief me on it right away. I want to say right now that I got nothing but wonderful cooperation from JPL, and that briefing was fantastic.

In order to prove how successful it was,



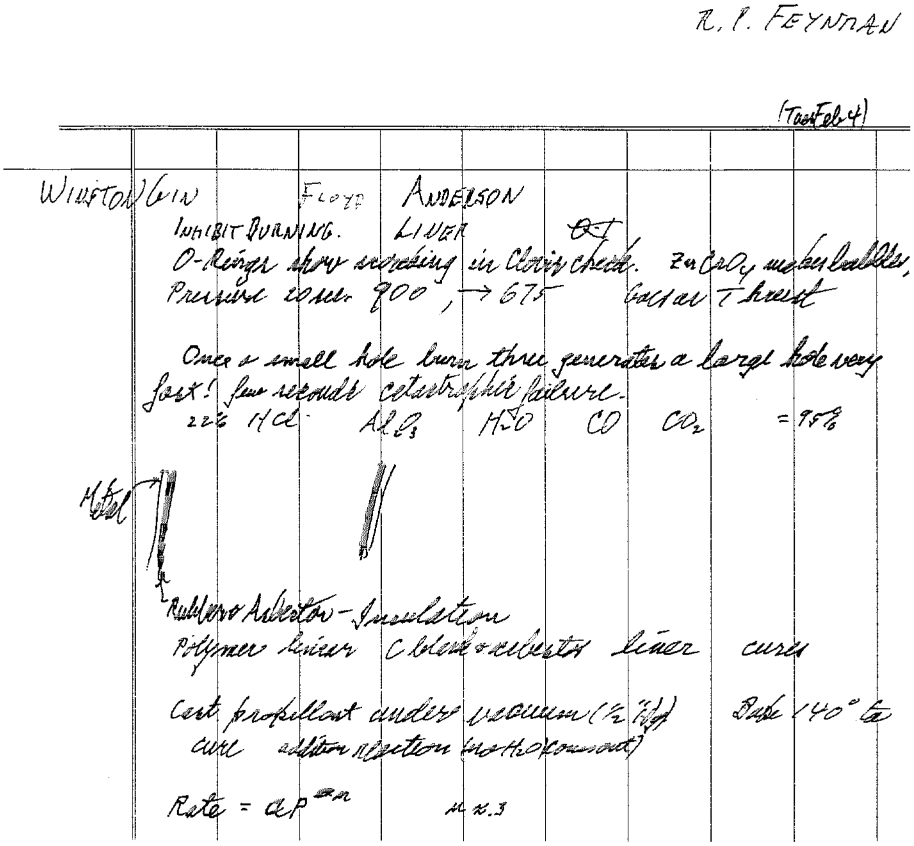
This cross section of the solid rocket booster (top, left) shows the rubber O-rings that encircle the rocket at the joints. Because the joint is stronger than the wall, the wall bows outward, causing the joint to open slightly, which lifts the O-rings off the seal.



One solution to this problem was to insert shims to keep the joint tight, as shown at left.

I'll show you the first page of the notes I made in the briefing (see next page). You'll find that on the *second line* it says, "O-rings show scorching in clevis check." That means hot gas had burned through the O-rings on several occasions. Furthermore, they told me that the zinc chromate putty had bubbles, or holes. It turned out that yes, indeed, through those holes the gas came in to erode the O-rings. So already, on the second line of my briefing, I was told what was the matter with the Shuttle.

The guys at JPL gave me a lot of other information. They told me about the engines, which are remarkable devices in the sense that the engineering involved is very good. They are way beyond normal. They are the most powerful engines for their weight


R. P. FEYNMAN

(Tues Feb 4)

WINSTON GIN FLOYD ANDERSON

INHIBIT BURNING. LINER

O-Rings show scorching in Clevis check. $ZnCrO_4$ makes bubbles,

Pressure 20 sec. 900, → 675 back at T thrust

Once a small hole burns thru generates a large hole very fast! few seconds catastrophic failure.

22% HCl Al_2O_3 H_2O CO CO_2 = 95%

Metal

Rubber or Asbestos - Insulation

Polymer liner Chlorinated? asbestos liner cures

Cast propellant under vacuum (½" Hg) Bake 140° to cure addition reaction (no H_2O comes out)

Rate = aP^n n ≈ .3

The first page of Feynman's notes from the February 4 JPL briefing shows suspicion of the O-rings in the second line.

that have ever been built. NASA was claiming that the engines were in the regular range of engineering, but they're not; the engines had many difficulties that the guys at JPL told me about. (I found out later that the people who worked on the engines always had their fingers crossed on each flight, and the moment they saw the Shuttle explode, they were all sure it was the engines. But of course, the TV replay showed a flame coming out of one of the solid rocket boosters.)

Anyway, the point is that I got briefed. And this was done with lots of energy, just like the old days at Los Alamos, one guy after the other: first the rocket, then the engines, and so forth. A guy would say, "We don't know about that; Lifer knows about that. Let's get Chuck Lifer in on this." So it was a very intensive briefing, the kind of thing I love, and I *sucked up* all the information like a sponge. I'm all set to go to Washington, and I go to Washington. (By the way, I took the "red-eye" across the country so I could stay here on Tuesday to learn about the Shuttle. But the red-eye I never took again — you're so sleepy when you get there.)

I check into the Holiday Inn early Wednesday morning, I get into a taxi, and read the address of Mr. Rogers' office to the driver. We start off. Mr. Rogers' office was supposed to be near the hotel somewhere — the hotel was located near the Capitol and near everything big — but we go on and on, further and further, into worse and worse territory, until we finally find the address — by interpolation, between two numbers. It was an empty lot there, with no number on it.

So now, what to do? I asked the taxi driver to go all the way back over this whole distance. (Meanwhile, my secretary tells me, she got a call from Washington: "Where is he?") Then I noticed that my hotel was right across the street from NASA. Perfect. Right across the street. (In fact, it was also across a different street, on the other corner, from where the commission later had its offices.)

I thought, "What the hell, NASA's right across the street. I'll go to NASA. Somebody there must know where the meeting is." So I went into NASA, up to Mr. Graham's office, and somebody knew. They showed me the room. There, the room was full of people. There were television lights and everything, and all I could do was squash in the back and think, "How the hell am I gonna get to the front where I belong?" I worried about this for awhile. Then I overheard a little bit about what they were saying, and it was evidently a different subject!

In the meantime, somebody from Mr. Graham's office had found the location of Mr. Rogers' office by phoning around and came down to get me. I finally made it to Mr. Rogers' law offices a few blocks away, where I met the other commissioners. Over the course of the commission, we all became very good friends. We worked very hard together. This first meeting was the beginning of a very effective commission — with the exception of Mr. Chuck Yeager, who came to one meeting for about half an hour, and then absented himself from the commission in order to be free so he could make criticisms of it.

Well, this first meeting was just a get-together. But Mr. Rogers did discuss the importance of our relationship to the press and how we have to be very careful with the press. "I know Washington," he kept saying. "We have to proceed in an orderly manner and be careful of leaks to the press."

The next meeting we had, on Thursday, was a public meeting — to start things off right with the press. By the way, we arrived at that meeting in limousines. We never got limousines again, but this time we arrived in limousines. I sat in the front seat. The driver says to me, "I understand a lot of very important, famous people are coming to this meeting . . ."

"Yeah, I s'pose . . ."

"Well, I collect signatures," he says. "Could you do me a favor . . ."

"Sure," I say.

I'm reaching for my pen when he continues, ". . .and find Mr. Armstrong for me, so I can get his signature?" There are always greater people.

That meeting was a public briefing. A briefing in a public meeting is almost impossibly inefficient, because other people ask questions, and they're not the questions you want to ask, and you've got to sit through all that, and so on, and so on. It's very ineffective, and I began to learn how boring such things can be. The NASA officials were telling me only a small fraction of all the things I had learned at JPL two days before.

We had all come to the meeting in limousines, and when we came out, some of the limousines were still there. One of the commissioners was a general, General Kutyna, who looked very handsome and very impressive in his uniform. But what impressed me was his request: "Where is the nearest Metro station?" Right away I liked him, and I found out that my judgement in this case was excellent.

That night I wrote out for myself what kinds of questions I thought we should ask and all the things I wanted to study. I laid out the whole business, hoping to see what the rest of the commission wanted to do in our next meeting.

The next day, Friday, was more effective. General Kutyna told us in considerable detail what an accident investigation was like and how it was done, using the Titan missile as an example. I was very impressed with this. I was happy to learn that most of the questions I was going to ask *were* the kinds of questions one should ask, except that the investigation should be done in a much more methodical fashion than I had imagined.

At the end of this discourse, Mr. Rogers, who is not a technical man, said, "Yes, your investigation was a wonderful success, but we can't use those methods on our flight because we can't get as much information as you had on yours." That was patently false, because the Shuttle, having people in it, was monitored much more carefully, so we had enormously more information than they had on the Titan. So there wasn't any doubt that we could do it.

In the meeting Mr. Rogers asked each of us how much time we could spend working on the commission. Many of the commissioners were retired, so they could spend 100 percent of their time. I also said I could spend 100 percent; I had everything arranged here at Caltech. (Nobody at Caltech ever said a word to me that I was shirking my work here, and I appreciate that.)

I tried very hard to get something to do. In the meeting I kept explaining that public briefings don't work with me; I have to talk to the technical people directly. Mr. Rogers explained that we were going down to Kennedy Space Center in Florida on the following Thursday. Then we would start our investigation.

Next Thursday? I wanted to get going much quicker than that, and kept explaining that I could work much more efficiently if I went on my own and talked to people directly, and I kept mentioning different things I'd like to do. Then the meeting would be interrupted by a letter coming in for Mr. Rogers, or something. He would read it — during which time various other commission members would whisper to me, "I'd like to work with you if you get a job" — and then Mr. Rogers would look up, apparently forgetting that I had been talking, and call on somebody else.

Finally, I would get the floor again. I would start my stuff again, and there would be another "accident." The meeting stopped while I was still talking, and the last words were by Mr. Armstrong, the vice chairman. He said we wouldn't be doing any of the detailed investigative work. Well, the only thing I'm any good at is detailed work!

I was devastated. I was depressed and very uncomfortable. After the meeting I went up to Mr. Rogers. "Look," I said. "We've got nothing to do for *five days*!"

He said, "Well, what would you have done if you hadn't been on the commission?"

"I would have gone to Boston to consult for the Thinking Machine Company."

"Well, you go to Boston to consult, and come back in five days."

I couldn't take that. I was wound up like a spring, ready to go to work. I had intended to "commit suicide" — do nothing else but work for the commission — for six months, and I had nothing to do. I was very depressed. I left that meeting feeling terrible.

Soon I thought of something. I called up Mr. Graham, and said, "Listen, Bill, We're not doing anything for *five days*! I want to get *started*! I want to DO something!"

He says, "Sure! You could go to Johnson, where they take the telemetry; you could go to Marshall, where they make the engines; or you could go to Kennedy."

I didn't want to go to Kennedy, because it would look like I was trying to get information before the rest of the commission did. That was not what I was trying to do; I just wanted to get started. Sally Ride had said she wanted to work with me if I got something to do, and I knew she was at Johnson, so I said I'd go there.

So Graham says, "That's fine, you can do that. I know David Acheson, who's on the commission. He's a good friend of Rogers. I'll call him and see what he thinks." About half an hour later, Mr. Acheson calls me: "I think it's a great idea, but I can't convince Rogers. Rogers refuses to say why he's against it, and I just don't know why I can't convince him that you should get started."

Meanwhile, Mr. Graham thought of a compromise: He would bring people into NASA headquarters, there in Washington, to brief me the next day, on Saturday. But Mr. Rogers called me up and said he didn't want me to do that. He kept explaining that we have to proceed in an orderly manner. I tried to explain how a technical person can talk to another technical person and get information very quickly, and that I wanted to DO something! I complained that we had had several meetings by now, but we hadn't yet discussed who was going to do what, or how to get started on the investigation.

Mr. Rogers said, "Well, do you want me to bother everybody and bring them together again for a meeting on Monday to discuss this?"

I said, "Yes!"

So he dropped the subject. Then he said, "I've heard you don't like your hotel. Let me put you in a good hotel."

I told him everything was fine with the hotel, and that I was perfectly satisfied with it. I just wanted to get to work! But he tried again, so I had to tell him, "Mr. Rogers, I am not interested in my personal comfort, only in the ability to do something!"

He said, "OK, go to NASA. It's OK." That's where our conversation ended.

So, I went. I got a private briefing all day at NASA on the engines and on the seals. The briefing on the seals was by Mr. Weeks. It was a continuation of my JPL briefing, with many more details, including the history of these matters: how the problem had been discovered very early, how there had been "burn-throughs," "erosion," "blow-bys," and what-not, on flight after flight — how many there were, and how each flight readiness review had looked at the information and decided it was all right to fly.

At the end of this long report on the problem of the seals, there was a page with recommendations (see below). This is how all information is communicated in NASA — by writing everything down behind little black circles, called "bullets."

The NASA report on the seals indicates a contradiction between the first and sixth recommendations.

Recommendations

- The lack of a good secondary seal in the field joint is most critical and ways to reduce joint rotation should be incorporated as soon as possible to reduce criticality
- The flow conditions in the joint areas during ignition and motor operation need to be established through cold flow modeling to eliminate O-ring erosion
- QM-5 static test should be used to qualify a second source of the only flight certified joint filler material (asbestos-filled vacuum putty) to protect the flight program schedule
- VLS-1 should use the only flight certified joint filler material (Randolph asbestos-filled vacuum putty) in all joints
- Additional hot and cold subscale tests need to be conducted to improve analytical modeling of O-ring erosion problem and for establishing margins of safety for eroded O-rings
- Analysis of existing data indicates that it is safe to continue flying existing design as long as all joints are leak checked with a 200 psig stabilization pressure, are free of contamination in the seal areas and meet O-ring squeeze requirements
- Efforts needs to continue at an accelerated pace to eliminate SRM seal erosion

When I looked at the recommendations, the thing that struck me was the contradiction between two of the bullets: The first one says, "The lack of a good secondary seal in the field joint is most critical. Ways to reduce the effects should be incorporated as soon as possible to reduce criticality." Then, further down the page, it says, "Analysis of existing data indicates that it is safe to continue flying with existing design . . ."— with some other conditions, such as using 200 lbs. of pressure in the leak test. (By the way, we discovered later that the leak test itself was causing the holes in the putty and was part of the reason for the failure of the seals!)

I pointed out this contradiction and said, "What analysis?" It was some kind of computer model. A computer model that determines the degree to which a piece of rubber will burn in a complex situation like that — is something I don't believe in!

I also found out that the matters that were causing trouble were brought up only at the "flight readiness review," where they were deciding whether to fly or not. There are so many considerations in deciding whether to fly, yet they brought up these critical matters only under those circumstances. In between the flights, there was no discussion of the problem — how it's going along, or whether there's some progress.

So, what was really happening was that NASA had developed an attitude: If the seals leaked a little and the flight was successful, it meant that the seal situation wasn't serious. Therefore, the seals could leak and it would be all right — it was no worse than the time before.

Such an attitude is, of course, extremely dangerous. One or two out of five seals leaked — and only some of the time — so it's obviously a probabilistic matter, a thing you have no control over, an uncertainty. And it's *not* obvious that the next time you fly, the uncertainty won't click over a little bit more, statistically, and the seal will fail. And it did, in fact, fail.

The next morning, Sunday, Mr. Graham took me with his family to the National Air and Space Museum. There we saw a moving picture about NASA, and it was so well done that I almost cried when I saw all the people involved at every level, how enthusiastic everybody was, and how eager they were to make things work. That made me even more determined to help straighten things out as quickly as possible and to talk to the Shuttle assembly people, the engineers, and everybody else low enough down.

Later that day, General Kutyna called me up on the telephone. "I was working on my carburetor, and I was thinking. You're a professor," he says. "What, sir, is the effect of cold on the rubber seals?"

I caught on immediately to what he was thinking of. The temperature was 29° when the Shuttle flew, and the coldest previous launch was 53°. I said, "You know as well as I do. It gets stiff and loses its resiliency." That gave me a clue. Of course, that's all he had to tell me, and it was a clue for which I got a lot of credit later. But it was his idea. The professor of physics always has to be told what to look for. You just use your knowledge to answer the questions.

That weekend, the *New York Times* put out an article about a man named Cook, who was in the budget department of NASA. Mr. Cook had written a letter to his superior a year earlier, saying that the engineers knew there was something wrong with the seals, that they might have to fix the problem, and it might be expensive. Mr. Cook was working out the budget and recommended that NASA prepare for the contingency that it would suddenly need a big load of money to fix this problem of the seals.

This gets into the *New York Times*, and so we have to have a special meeting. It's the press, you see; we have to match the press. So on Monday, everybody was called to a meeting anyway! But I remind you, we still hadn't had any meetings in which we did any work. At this emergency closed meeting, we got some interesting information: The NASA people who had been looking at the television pictures of the launch saw preliminary indications that there was smoke coming out of one of the joints just at lift-off.

More interesting still was a report by a man named MacDonald from the Thiokol Company, who came to the meeting on his own. He said that the Thiokol Company engineers had *noticed* the low temperature, had been *worrying* about their seals, had *known* about the resilience not being there. Furthermore, they knew that when it is cold, the grease in the seals is very viscous so it can't move fast enough to close the gaps. The engineers were very, very worried about it just before the flight and reported to the people at Marshall that they should not fly below 53° temperature, and that night it was 29°. But the engineers were told that that was an appalling decision, that they should think it over again, and they were given some apparently logical reason.

(By the way, there were *lots* of apparently logical reasons *all over* this business, but a little common sense shows you that they're only *apparently* logical. For example, the succession of blow-bys was getting more serious, so they kept changing the criteria of what they accepted, saying, "It flew before, so it must be OK." Try playing Russian roulette that way: You pull the trigger and it doesn't go off, so it must be OK to do it again, right?)

We later learned that in the discussions inside Thiokol, the engineers were *still* saying, "We shouldn't fly," but the managers made a decision nevertheless to go ahead and fly, and then they gave the usual, apparently logical reason, which was — never mind, I couldn't ever understand it. It's hopeless.

At any rate, that morning I had asked the question about how resilient the rubber is, and, as always, NASA was very cooperative at giving me information. That afternoon I got a stack of papers, the first page of which said, "Mr. Feynman of the commission wants to know about the resiliency of the O-ring rubber at low temperatures . . ."— and it's sent to the next subordinate. The subordinate writes to another subordinate, "Mr. Feynman of the presidential commission wants to know . . ." and so on, down the line. In the middle there's a paper with the answer, and then there's a series of papers — the submission papers — which explain that "this is in answer to your request at such-and-such a time."

So I get this stack of papers, just like a sandwich, and in the middle the answer is given to the wrong question! The answer I got was: When you squeeze the rubber for two hours at a certain temperature and pressure, what happens when you let go — how long it takes to creep back — over *hours*. And I was talking about *fractions of a second* during launch when the gap in the field joint is suddenly changing. So the information was of no use.

We were going to have a public meeting the next day. I was already getting tired of these public meetings and briefings because they were so time-consuming and of so little use. I thought, "Now we're going to have an open meeting, and we're going to say exactly the same things that we did in the closed meeting." (It was a good idea: Mr. Rogers wanted to keep the public informed, so every time we discovered something, we would quickly have an open meeting to bring out the new material.) But I thought, "It's like an act: We have to hear the same things in the open meetings as in the closed meetings, and we won't learn anything new. And the information I got from NASA about the rubber is useless."

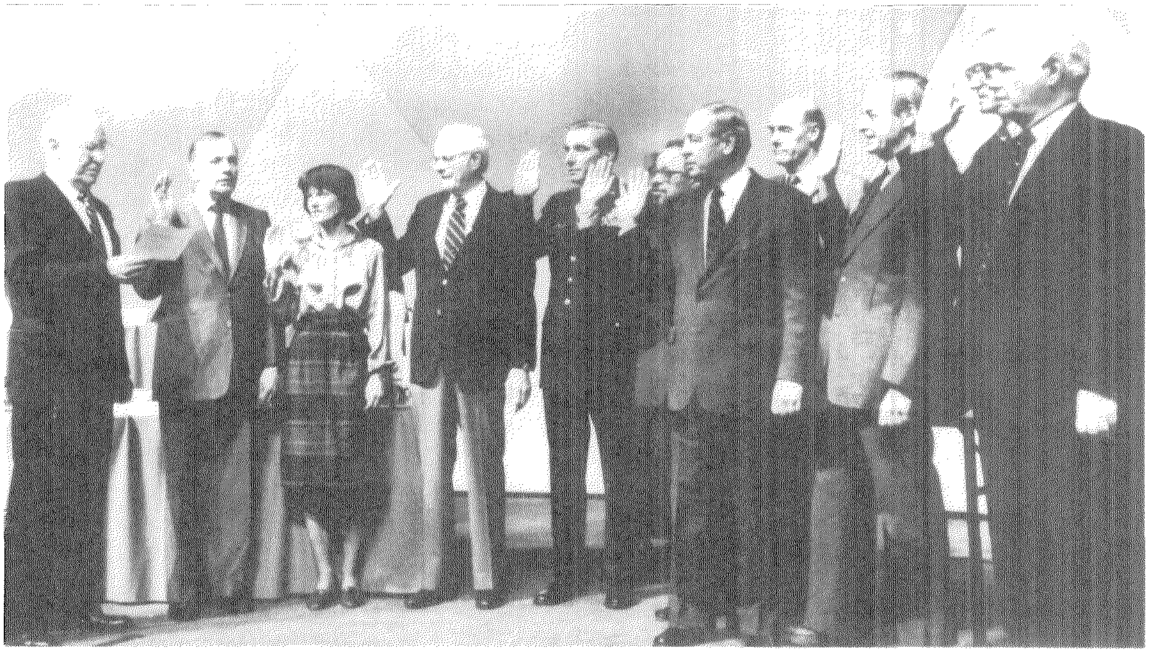
I'm feeling lousy and I'm eating dinner; I look at the table, and there's a glass of ice water. I think, "Damn it, *I* can find out about that rubber *without* sending notes to NASA and getting back a stack of papers; all I've got to do is get a sample of the rubber, stick it in ice water, and see how it responds when I squeeze it! That way, I can learn something *new* in a public meeting!"

I ask NASA for a piece of the rubber. It's impossible to get; they're very, very careful, and every piece of material is checked and counted and everything else, so you can't just go down to the stockroom and pick up a piece of rubber. But Mr. Graham remembered there were two pieces of the rubber in the field joint model NASA had shown us before and was going to use again in the open meeting. The two pieces of rubber were the real thing about an inch and a half long each. We decided to meet in Mr. Graham's office the next morning before the meeting to see if I could take the model apart. (In the open meeting I would have to take the model apart quickly.)

The next morning I get up early. I come out of the hotel — it's snowing a little bit — and I'm dressed up in that outfit (my suit) because I'm going to the public meeting later. A taxi comes up, and I say to the driver, "I want to go to a hardware store."

He says, "A hardware store? There's no hardware stores here. The Capitol is just up the street — we're in downtown Washington!" Then he remembered where he had seen a hardware store once, some distance away, and we went there. I waited around for it to open, and then I bought myself some screw drivers, pliers, clamps, and so on, because I wasn't sure exactly what I would need.

When I got to NASA I began thinking the clamps were too big to put into a glass. So to get some small clamps I went to the medical department of NASA, where I had gone several times before (my cardiologist was trying to take care of me by telephone). I went up to Graham's office. He was very cooperative, as always, and we saw that I could open



AP/Wide World Photos

William Rogers (left) swears in the commission at the National Academy of Sciences. From left are Neil Armstrong, Sally Ride, Robert Rommel, Major General Donald Kutyna, Arthur Walker, Jr., Joseph Sutter, David Acheson, Feynman, Albert Wheelon, and Robert Hotz.

the model very easily with just a pair of pliers. So there was the rubber, right in my hand, and although I knew it would be more dramatic and honest to do the experiment directly in the meeting, I cheated — I couldn't resist. I tried it. And, after all, it would be quite a flop if it didn't work! So, following the example of having a closed meeting before an open meeting, I must tell you I discovered it worked before I did it in the open meeting.

I kept wanting to do my experiment all during the meeting, but General Kutyna, who was sitting next to me, gave me advice. He had given me advice before. At the first public meeting he had leaned over and said, "Copilot to pilot: Comb your hair." So now he was saying, "Copilot to pilot: Not now!"

So when he told me, "Now!" I did it, and everything went all right. As you probably know, I demonstrated that the rubber had no resilience whatever when you squeezed it at that temperature, and that it was very likely a partial cause of the accident. We all agreed later that that, in fact, was true.

On Wednesday, February 12, we had no meeting, so I wrote a letter home. I told my wife she was right, that in certain ways I *was* unique. One of the ways I was unique was that I was not connected to any organization — I had no weakness from that point of view. I was, of course, connected with Caltech, but that's not a weakness! For example, General Kutyna was in the Air Force, so he couldn't say everything exactly the way he wanted, because he might get in trouble with the Air Force. Sally Ride still had a job at NASA. Everyone on the commission had some kind of connection and therefore some kind of weakness, but I was apparently invincible.

But General Kutyna warned me that when they fly airplanes, they have a rule: Check six. Most airplanes are shot down this way: A guy is flying along, looking in all directions, and feeling very safe. An airplane flies up behind him (at "six o'clock"; "twelve o'clock" is directly in front), and he gets hit. So you always have to check six o'clock. So I began to write, "Check six!" on every note paper I had and developed a kind of paranoia.

For example, I have a cousin who previously had been with the Associated Press as White House correspondent and is now with CNN; I also have a nephew who works for the *Washington Post*. When I had some time I would visit with them — eating dinner, and so on. It was very pleasant, but we made sure we never said a *word* about anything I was doing, because I didn't want to be responsible for any leaks. I told Mr. Rogers that I had these associations with the press. He smiled and said, "It's perfectly all right. I used to work for so-and-so"— he had some connection with the press too. He just laughed; there was no problem. But my paranoia had developed to such a point that I thought, "That was too easy; he's going to get me that way!" So I stopped seeing my cousin. That was stupid: There were no problems; it was just my state of mind.

I did, however, keep talking to the press — openly, always giving my name. (I didn't want any hocus-pocus about "unidentified sources," or anything.) My cousin had taught me that the press is not something to be afraid of, and it turns out to be true. I found that out several times. The first time was when the *New York Times* put out an article after I did the ice water experiment; during the public meeting I had no time to explain what its meaning and importance were, but *they* had it all explained perfectly.

Another time, NBC interviewed me — they caught me in the lobby of my hotel. They interviewed me for 15 to 20 minutes — the lady reporter was very short and very nice — and I talked in my usual, careful, professorial way, with all the caveats and so forths and so ons. I saw the interview later on the "Nightly News": I was on for about two seconds — I say something, and BOOM! — it's over. But it was good: The report carried the line of what I said, and the reporter put the context around it, saying things like, "The professor went on to say that this was only the result of a mathematical model and might be uncertain"— stuff like that. It was excellent. It was very short, carefully put together, and excellent — except for one thing: Because I'm not experienced, I didn't look into the camera when I spoke. Instead, it looked like I was talking to my dog.

Well, finally, on Thursday, we get to Kennedy. The main briefing turned out to be the way I thought it would be — we didn't get any useful information just looking around at the "gee-whiz" place. But before that, we had two meetings in which we got a lot of information. We got a detailed look at the pictures of the smoke, which made it very apparent that the leak of gasses through the seal had started immediately after ignition, then somehow plugged itself up temporarily, and finally ended up with a flame coming through. We also got all the details on the Thiokol-Marshall discussions, in which the engineers never changed their minds; only the manager did, under pressure from Marshall.

After two days at Kennedy, we were supposed to return to Washington. I thought, "Now, at last, here I am. Now I've got a chance to talk to everybody."

I told Mr. Rogers I wanted to stay at Kennedy, and he said, "I'd prefer that you didn't stay down here, but of course you can do whatever you want."

I said, "Well, OK, then, I'll stay."

So I stayed at Kennedy a few more days. I ran around and found out more about the pictures from the photograph guys; I found out about the ice that had been on the launch pad from the ice crew. They told me they had gotten some funny numbers for the temperature on the morning of the launch, and we discussed what was wrong. We called up the people who made the instrument, and tried to find out how the instrument was built so we could understand the errors, but they suddenly clammed up, obviously afraid that they were going to be blamed for the Shuttle disaster.

I explained to the manufacturer that the instruments were not used in accordance with their manual (they had been used too soon after being taken out of the box), and we wanted to know what the effect of that misuse would be on the apparent temperature readings, and so forth. I finally got them to explain it all. They said our errors were reproducible. So we set up an experiment in which we reproduced the circumstances, and we corrected the temperature readings. I'm only trying to say I was working hard.

Another thing came up while I was running around down there at Kennedy. I had predicted that Mr. Rogers was going to try to fix me by overloading me — by giving me a lot of stuff to do. Sure enough, it happened; the commission staff in Washington kept sending me things to do. But as the instructions came in, I had done them already — they didn't realize how fast I am at getting information and understanding it and going on to the next thing.

The only thing they sent me that I didn't do had to do with a certain memo whose existence they had discovered. During the assembly of the SRBs, someone had written cavalierly, "Let's go for it!" The staff didn't like that attitude on the part of the workers, and they wanted to find that piece of paper. By that time I knew how much paper there was in NASA so I was sure it was a trick to make me get lost and to do nothing. So I did nothing about it.

I talked to Mr. Lamberth, who was in charge of the assembly of the SRBs. He told me about the problems he had with the workmen. They had had a little accident earlier, and he had to discipline them about it, and then he told me about another incident: The SRBs become a little bit out of round after each use. When the workers were trying to make the rocket round again with the rounding machine — a rod with a hydraulic press on one end and a nut on the other — they were only supposed to go up to 1250 lbs., according to the manual. But they couldn't get it squashed enough that way, so they took a wrench and tightened the nut on the other end of the rod to squeeze it some more. That made the rocket round, all right, but one of the workmen noticed that the pressure had gone up to 1350 lbs. that way. Well, a gauge measures the force applied to a rod from *either* end, so tightening the nut increases the pressure past 1250 lbs., of course! So Mr. Lamberth admonished the workers to follow the manual. He said the workers weren't like they used to be, and he was very disturbed.

So I go down and talk to the workers. First of all, I'm surprised to find that the foreman doesn't know anything about this admonishment. He knew about the 1350 lbs., but he didn't know he had been admonished. He said, "No, we weren't admonished; we were following the procedures in the manual." Sure enough, the manual said to tighten the nut after the pressure reaches 1250 lbs. — it said so in black and white! It didn't say that tightening the nut would increase the pressure; the people who wrote the manual probably weren't quite aware of that. So the workmen had, in fact, followed the manual perfectly. (I later found out that as a result, the manual was revised to allow for higher pressure, and that only the hydraulic jack was to be used to increase the pressure. The step about tightening the nut was eliminated.)

So Mr. Lamberth really didn't know what happened underneath. He said he had admonished the workmen, but he never talked to them directly. So he had the idea that his workmen were no longer like they used to be, but I tell you, they really were. They had a lot of information but no way to communicate it. The workmen knew a lot. They had noticed all kinds of problems and had all kinds of ideas on how to fix them, but no one had paid much attention to them. The reason was: Any observations had to be reported in writing, and a lot of these guys didn't know how to write good memos. But they had very good knowledge, they worked very hard, and they were very enthusiastic.

While I was doing my work down at Kennedy, Mr. Rogers was in Washington appearing before a Congressional committee. (Congress was considering whether to set up its own investigation of the accident.) Senator Hollings said, "So who have ya got, there, on your commission? Ya got a couple of astronauts, a Nobel prizewinner, a general, some businessman, and a couple of lawyers. What you really need is gumshoes, who will be right down there at Kennedy, eating lunch with the very guys who do the work on the Shuttle."

And Mr. Rogers was able to reply, "You'll be interested to know, Senator, that the Nobel prizewinner is down there at Kennedy, right now, doing exactly that!" (Although Mr. Rogers couldn't have known it, I was actually eating lunch with some of the engineers at exactly that time.) So Mr. Rogers gradually realized I wasn't quite so useless. We got to respect each other very much — I think he ultimately respected me, and I certainly do respect him for his abilities.

I went back to Washington, and I got into more and more difficulties. The next meeting we had was a public meeting, and I was questioning Mr. Lund of the Thiokol Company, who had changed his mind about launching the Shuttle. Somebody at Marshall had told him to put on his "management hat" instead of his "engineering hat," and so he changed his opinion. I was asking him, "Don't you understand the principles of probability?" when suddenly I had this feeling of the Inquisition.

Mr. Rogers had pointed out to us that we ought to be careful with these people, whose careers depended on us. He said, "We have all the advantages. We're sitting up here, they're sitting down there; they have to answer our questions, we don't have to answer their questions. It isn't fair." Suddenly all this came back to me and I felt terrible. I couldn't do it the next day, so I went back to California, just for a day or two, to rest up.

While I was in Pasadena, I went over to JPL and discussed the enhancement of the pictures with Jerry Solomon and Meemong Lee; they were studying the flame that had appeared on the side of the SRB just before the main fuel tank exploded. I had just been in Washington, hearing the NASA managers talk through a fog. What a difference — just like with the photograph guys and the ice crew at Kennedy, everything was so direct and simple at Caltech and JPL. What a difference!

We finally split up into working groups, and I went to Marshall with General Kutyna's group. The first thing that happened there was, a range safety officer by the name of Ulian came to tell us about a discussion he had had with NASA higher-ups about safety. Mr. Ulian had to decide whether to put explosive charges on the side, so ground control could destroy the Shuttle in case it was falling onto a city. The big cheeses at NASA said, "Don't put any explosives on, because the Shuttle is so safe. It'll never fall onto a city."

Mr. Ulian tried to argue that there *was* danger. One out of every 25 rockets had failed previously, so Mr. Ulian estimated the probability of danger to be about one in 100 — enough to justify the explosive charges. But the higher-ups at NASA said that the probability of failure was one in 100,000. That means if you flew the Shuttle *every day*, the average time before your first accident would be *300 years* — every day, one flight, for 300 years — which is obviously crazy! Mr. Ulian also told us about the problems he had with the big cheeses — how they didn't come to the meetings sometimes and all kinds of other details.

Then I thought of this question: By now we had found out that the flight failed because one of the seals had broken, and the higher-ups had told us they didn't know anything about the seals problem — even though I was able to find out about it right away at JPL, before I even went to Washington. We saw that NASA had no system for fixing the problem, even though engineers were writing letters like, "HELP!" and "This is a RED ALERT!" Nothing was happening. My question was: Does this lack of communication between engineers and management also exist in other places? I thought, "I oughta find out whether this is a characteristic of the whole system, or whether it's true just for Morton-Thiokol, and we happened to find out about it because the O-rings busted." So I told the people at Marshall I wanted to find out about the engines. I wanted to talk to a couple of engineers without any managers around.

"Yes, sir, we'll fix it up. How about tomorrow morning at 9:00?"

The next day I come in, and there's engineers, all right, but there's also managers, and a great, big book: *Presentation Made on February Such-and-Such to Commissioner Richard P. Feynman* — all prepared during the night.

"Geez! It's so much work!" I said.

"No, it's not so much work; we just put the regular papers in that we use all the time."

The engine is extremely complex and hard to understand, and the engineers were explaining to me how it worked, showing slide after slide. I asked my usual dumb-sounding questions.

After a while, Mr. Lovingood, a middle manager there, said, "Mr. Feynman, we've been going for two hours now. There are 123 pages, and we've only covered 20."

"It's all right, don't worry," I said. "I'm confident that it'll go faster as we go along, but I want my questions answered at the beginning. Otherwise, I can't understand it."

Suddenly I got an idea. I said, "All right, I'll tell you what. In order to save time, the main question I want to know is this: Is there the same misunderstanding, or difference of understanding, between the engineers and the management associated with the engines, as we have discovered associated with the solid rocket boosters?"

Mr. Lovingood says, "No, of course not. Although I'm now a manager, I was trained as an engineer."

I gave each person a piece of paper. I said, "Now, each of you please write down what you think the probability of failure for a flight is, due to a failure in the engines."

I got four answers — three from the engineers and one from Mr. Lovingood, the manager. The answers from the engineers all said, in one form or another (the usual way engineers write — "reliability limit," or "confidence sub so-on"), almost exactly the same thing: one in about 200. Mr. Lovingood's answer said, "Cannot quantify. Reliability is determined by studies of this, checks on that, experience here"— blah, blah, blah, blah, blah.

"Well," I said, "I've got four answers. One of them weaseled." I turned to Mr. Lovingood and said, "I think you weaseled."

He says, "I don't think I weaseled."

"Well, look," I said. "You didn't tell me *what* your confidence was; you told me *how* you determined it. What I want to know is: After you determined it, what *was* it?"

He says, "100 percent." The engineers' jaws drop. My jaw drops. I look at him, everybody looks at him — and he says, "Uh . . . uh, minus epsilon!"

"OK. Now the only problem left is, what is epsilon?"

He says, "One in 100,000." So I showed Mr. Lovingood the other answers and said, "I see there *is* a difference between engineers and management in their information and knowledge here, just as there was in the case of the rocket, but let me not bother you about it; let's continue with the engine."

So they continued telling me about the engine, and soon I understood how it worked. Then they told me about all the problems they had had with it — blades cracking, and all kinds of other difficulties. And I discovered the same game, just as in the case of the solid rocket boosters, of reducing criteria and accepting more and more errors that weren't designed into the device.

Later I also checked the avionics, the software NASA uses on its computers for controlling the Shuttle from launch to landing, to find out if a similar situation existed there. But in this case, on the contrary, everything was very good; the engineers and the managers communicated well with each other, and they were all very careful not to change their criteria of acceptance during flight reviews. I found the avionics completely satisfactory.

I wrote up what I found out about these things into a special report, hoping that the other members would see it for discussion. I sent it to Al Keel, the executive officer whom Mr. Rogers had selected to coordinate everything on the commission. He told me on the telephone that he had received it and that he would show to everybody.

By this time we were beginning to write up our part of the main report about the accident. General Kutyna had set up a whole system at Marshall for doing so. It lasted about two days before we got a message from Mr. Rogers: "Come back to Washington. You shouldn't do the writing down there." So we went back to Washington, and Mr. Graham lent me an office and a secretary who was very, very good. I helped our group write up its part of the main report — with a lot of input from Mr. Keel.

All this time I had expected that we would be meeting in Washington to discuss what we had found out so far, to think it out together and look at it from different perspectives — in addition to the astronauts there were lawyers and industrialists, there were scientists and engineers, and so on — and to discuss with each each other where to go next. But in our meetings, all we ever did was what they called "word-smithing"— correcting punctuation, refining phrases, and so on. We never had a real discussion of ideas!

Besides the word-smithing, we discussed the typography and the color of the cover. At each meeting we were asked to vote, so I thought it would be efficient to vote for the same color we had decided on in the meeting before — but it turned out I was always in the minority! We finally chose red. It came out blue.

At any rate, after one of the meetings I was talking to Sally Ride about my experiences investigating the engines and the avionics, and I noticed that she didn't seem to know about the special report I had written — the one Mr. Keel told me he would show to everybody. So I said to Mr. Keel, "Sally hasn't seen my report."

He says to his secretary, "Oh, make a copy of Mr. Feynman's report and give it to Ms. Ride."

Then I discovered Mr. Acheson hadn't seen it.

"Make a copy and give it to Mr. Acheson."

I finally caught on, so I said, "Mr. Keel, I don't think anybody has seen my report."

So he said to his secretary, "Make a copy for all the commissioners and give it to them."

Then I said, "I thought you told me you showed it to everybody."

"I meant I showed it to the entire staff."

Needless to say, when I asked the members of the staff about it, none of them had seen it either.

When the commissioners read my report, all of them thought there was a lot of good stuff in it, and it ought to be in the commission report somewhere. But we couldn't discuss it, because all we were doing was this word-smithing stuff on what was already writ-

AP/Wide World Photos

ten — not adding anything new. We were working on the summary report for the President — I'll call it the main report — which was relatively brief. Later, as back-up data and other information, we were going to put out a series of appendices. So, I thought, there are two possibilities for my report. It could be in the main report — but it would have to be rewritten in that case, because the style of the main report was different — or it could be put out later as an appendix.

Although some of the members felt strongly that it ought to go in the main report, I thought I'd compromise, and let it go in as an appendix. But in order to get my report in as an appendix, it had to be put into the document system computer, which was quite elaborate and very good, but different from the computer system I had written my report on at home. They had an optical scanner for transferring it, so I asked them to do that, and they said, "Of course."

I'd go away for a while, and when I'd come back, it would be lost. But I kept pushing on it, watching it, nursing it along, and I finally got it through to the point where it was, at last, in the hands of a real editor, a capable man by the name of Hansen, who changed all my whiches to thats and thats to whiches.

Mr. Hansen fixed up my report without changing the sense of it. Then Mr. Keel fixed it up so it could go in as an appendix: He put all kinds of big circles around whole sections, with Xs through them; there were all kinds of thoughts left out. He explained to me that my report was repetitious with the main report, and I argued that it's much easier to read something that's all together, and because it was going to be an appendix, repetition didn't matter.

Finally, the commission had its last meeting. It was about the recommendations we would make to the President. We made nine recommendations. The next day, I'm standing around in Mr. Rogers' office when he says, "I thought we would add a tenth recommendation: "The commission strongly recommends that NASA continue to receive the support of the Administration and the nation . . ." In our four months of work as a commission, we had never discussed that issue. It wasn't in our directive from the President. We were only to look at the accident, find out what caused it, and make recommendations to avoid such accidents in the future.

So I thought this tenth recommendation wasn't appropriate and said so. We argued back and forth a little bit, but then I had to catch a plane to New York, where I was going for the weekend. While I was in the airplane, I thought about it some more, and the more I thought about it, the more I thought what a mistake it looks like — just like one of the NASA reports, like the one I had seen back at the beginning, with the contradictory bullets: There's all these troubles, but in the end we recommend to keep on flying!

I knew I didn't like it. Furthermore, we hadn't discussed it at a meeting! It was just Mr. Rogers' idea. I didn't want to call up Mr. Rogers and argue with him on the telephone, so I quietly and thoughtfully wrote out a letter to him, carefully explaining why I didn't like the tenth recommendation. To make sure it got there right away, I dictated my letter over the telephone to Mr. Rogers' secretary, who typed it up and handed it to him right in his office!

When I came back from New York, Mr. Rogers told me that he had read my letter. He said he agreed with it, but that I was out-voted.

I said, "How was I out-voted, when there was no meeting?" I thought my ideas about this were worth discussing with the other commissioners, and I wanted to know what they thought about my arguments.

"I know, but I called each one of them up," he said, "and they've all agreed. They've all voted for it."

So I said, "Well, I'd like a copy of this recommendation," and I went off to make a copy of it. When I came back, Mr. Keel said he forgot that they hadn't talked to Mr. Hotz — Mr. Hotz was there, you see, so I could ask *him* right away. They forgot that they hadn't talked to Mr. Hotz. I went to lunch with Mr. Acheson and Mr. Hotz, and it seemed like Mr. Hotz agreed with me. When we went back to Mr. Rogers' office, Mr. Acheson explained to me, "It's only 'motherhood and apple pie.' If this were a commission for the National Academy of Sciences, your objections would be proper. But since this is a presidential commission, we should say something for the President."

"I don't understand the difference," I said. (Being naive at the right time is often a good idea.) "I just don't understand. Why can't I be careful and scientific when I'm writing a report to the President?" (Being naive doesn't always work: My argument had no effect.)

I was very concerned by all this, and I came home for a while, very disturbed. I then got the idea — which I hadn't had before — to call up some of the other commissioners. I'll call them A, B, and C.

I call A. He says, "What tenth recommendation?"

I call B. He says, "Tenth recommendation? What are you talking about?"

I call C. He says, "Don't you remember, you dope? I was in the office when Rogers first told us, and I don't see anything wrong with it."

Although some of the commissioners agreed with the tenth recommendation, I still thought we should have discussed it in a meeting. I had also been railroaded into modifying my report, even though it was going to appear only as an appendix. I talked to my sister, who used to work in Washington.

She said, "Well, if they do that to your report, what happens to all the work you did on the commission? Your contribution wouldn't be seen. It would appear as if you didn't do anything."

I said, "Aha!" and I sent a telegram to Mr. Rogers:

"PLEASE TAKE MY SIGNATURE OFF THE FRONT PAGE OF THE REPORT UNLESS TWO THINGS OCCUR: 1) THERE IS NO TENTH RECOMMENDATION, AND 2) MY REPORT APPEARS AS AN APPENDIX WITHOUT MODIFICATION FROM VERSION #23 OF MR. HANSEN."

I knew by this time I had to define everything carefully! (By the way, *everything* had 23 versions. It has been noted that computers, which are supposed to increase the speed at which we do things, have not increased the speed at which we write reports. We used to make only three versions — because they're so hard to type — and now we make 23 versions!)

The result of this telegram was that Mr. Rogers and Mr. Keel tried to compromise. They asked General Kutyna to be the intermediary, because they knew he was a friend of mine. What a *good* friend of mine he was, they didn't know.

The general calls me up, and right away he says, "Hello, professor, I'm in the Pentagon, and nobody can listen to this call. Let me first tell you, I'm with you. But I've been given the job of convincing you to change your mind, and I have to give you all the arguments."

"Fear not!" I said, "I'm not gonna change my mind. Just give me the arguments, and fear not."

So he gave me all the arguments, none of which had any effect. The arguments were all kinds of crazy things. For example, "If you don't accept the tenth recommendation, they're not going to accept the compromise they already made about putting your report in as an appendix." I didn't worry about that one, because I didn't have to sign the main report, and I could always put out my report by myself.

Another argument was that they noticed I was always talking to the press and would claim I was doing this as a publicity stunt to sell more copies of my book. That one made me smile, because I could imagine the laughter it would produce from my friends at home. I knew that nobody I cared about would believe it.

But finally, I did compromise. I said, "Instead of making it a recommendation, just make it a concluding thought and change the wording from 'strongly recommends' to simply 'urges.'"

They accepted that.

A little bit later, Mr. Keel calls me up: "Can we say '*strongly* urges?'"

I said, "No. Just 'urges.'"

So I put my name on the main report, my report got in as an appendix, and everything was all right. We gave our report to the President on a Thursday in a ceremony at the White House in the Rose Garden. The report was not to be publicized until Monday, so the President could study it.

During those three days the newspaper reporters were working like demons. They knew the report was finished, and they were trying to scoop each other to find out what was in it. They kept calling me up because I had been so cooperative before. I told my secretary to say that I had no comment on anything; I would answer all their questions on Tuesday at my news conference.

Well, I didn't know it, but someone had leaked that this argument had gone on. The only man who knew about it, I think, was Mr. Hotz. He may have thought it would help me in pushing my point, but for whatever reason, it leaked. Some paper in Miami started it, and soon the story was running all over about this argument between me and Mr. Rogers. So when the reporters called me up, they'd get the message, "He has nothing to say; he'll answer all your questions at his

President Reagan accepts the commission's report in the Rose Garden of the White House. Feynman stands at right.

press conference on Tuesday."

That sounded very suspicious, so my press conference turned out to be very popular. That's what most of the questions at the news conference were about. So I would like to say again that I don't have any problem with Mr. Rogers. In fact, I have a very good attitude towards him. I think he is a wonderful man, and he really ran the commission well — although in a way that at first I didn't understand. And I think I was a real problem for him much of the time.

Finally, I would like to say something about the general deterioration of NASA — and the fact that there was no information coming up from the engineers to the management. Just the other day I was reading a book by Harvey Brooks in which he talked about innovation. He explained that innovation doesn't have to be the direct invention of a machine; an innovation could be the way things are made, such as the Ford mass production line or, as in another of his examples, the management system developed at NASA for the Apollo program, which involved the cooperation of so many contractors and subcontractors. The system they evolved was an innovation, a great development. This was more than 20 years ago. But in the meantime, something happened that happens to many human innovations — it deteriorated. The question is: How and why? I don't know.

I invented a theory, which I have discussed with a considerable number of people, and many people have explained to me why my theory is wrong. But I don't remember their explanations as to why it's wrong — you never can, because that's the way you're built! I am a weak human, too, so I cannot resist telling you what I think is the problem.

When NASA was trying to go to the moon, it was a goal that everyone was eager to achieve. Everybody was cooperating, much like the efforts at Los Alamos. There was no problem between the management and the other people, because they were all trying to do the same thing. But then, after going to the moon, NASA had all these people together, all these institutions, and so on. You don't want to fire people and send them out in the street when you're done. So the problem is what to do.

You have to convince Congress that there exists a project this organization can do. In order to do so, it is necessary (at least it was *apparently* necessary in this case) to exaggerate — to exaggerate how economical the Shuttle was going to be; to exaggerate the big scientific facts that would be discovered. (In every newspaper article about the Shuttle there was a statement about the useful zero-gravity experiments — such as making pharmaceuticals, new alloys, and so on — on board, but I've never seen in any science article any results of anything that have ever come out of any of those science experiments which were so *important*!) So NASA exaggerated how little the Shuttle would cost, they exaggerated how often it could fly, to such a pitch that it was *obviously incorrect* — obvious enough that all kinds of organizations were writing reports, trying to get the Congress to wake up to the fact that NASA's claims weren't true.

I believe that what happened was — remember, this is only a theory, because I tell you, people don't agree — that although the engineers down in the works knew NASA's claims were impossible, and the guys at the top knew that somehow they had exaggerated, the guys at the top didn't want to *hear* that they had exaggerated. They didn't want to hear about the difficulties of the engineers — the fact that the Shuttle can't fly so often, the fact that it might not work, and so on. It's better if they *don't* hear it, so they can be much more "honest" when they're trying to get Congress to OK their projects.

So my theory is that the loss of common interest — between the engineers and scientists on the one hand and management on the other — is the cause of the deterioration in cooperation, which, as you've seen, produced a calamity. □

Technology, in Perspective and under Control

by Paul B. MacCready

WE ALL SHARE the goal of humans reaching a sustainable and comfortable accommodation with each other and with the global environment. Technological, environmental, and societal factors intermingle and collide with each other as we move toward this goal. Because pressures are increasing so rapidly, I feel that if this goal is to be reached, it must be reached within our lifetimes or those of our children. In particular, I find myself concerned that a number of technological advances, each beneficial to its developers and users, will be found to have a negative impact, especially when viewed over a time scale of several decades. This article explores challenges we all face, some from technological advances, and suggests ways for an individual to make a significant difference.

Genetically, we are still the gatherer-hunters of the pre-agricultural era, but now we are operating airlines, computers, television, and robotic factories, and we have a finger on nuclear energies. Our technology rockets ahead, introducing in a year more innovation, and more global impact on the environment and civilization, than took place in a century just a few hundred years ago, or in a millenium just a few thousand years ago. This has its obvious good side, but the negatives, particularly those we don't even suspect yet, could prove overwhelming in the future. We are rapidly eliminating fellow species of flora and fauna and their habitats, and in general making unsustainable demands on the limited resources of the earth. We find robots displacing workers, new weapons arming terrorists, and improved communications being appropriated to facilitate dictatorial control. Our modern culture, our institutions, and our individual reward systems are poorly matched to the tasks of moving toward man getting along with the millions of species of flora and fauna — and man getting along with man. We have more intelligence than wisdom, and technology is evolving toward becoming our master rather than our servant.

For the past few decades most of us have accepted that we are all passengers on "spaceship earth." Recently we have realized that we are not just passengers but also crew. Still more recently we have noted that some of the crew may be very bright, but overall the crew is quarrelsome, unmanaged, and not very effective at plotting a desirable destination and a sustainable living style.

The responsibility scientifically competent individuals have for operating in a broader arena is succinctly reviewed by Carl Sagan in

Cosmos, while discussing the Library of Alexandria and the reason that science slept for a thousand years after the library's demise: ". . . there is no record, in the entire history of the Library, that any of its illustrious scientists and scholars ever seriously challenged the political, economic, and religious assumptions of their society. The permanence of the stars was questioned; the justice of slavery was not."

I have no credentials for handling such broad issues — only a belief that it is appropriate to be involved in subjects you deem important rather than just those that fit comfortably into your narrow area of expertise. My connection with global challenges arose unexpectedly over the past decade. The catalyst was the development of the *Gossamer Condor* and the *Gossamer Albatross*, which in 1977 and 1979 won the Kremer prizes for human-powered flight. The motivation for undertaking those projects was simply to win the prize money, but the greater, and wholly unanticipated, result of those and subsequent projects featuring the development of unusual vehicles was to stimulate my interest in broader issues. I found myself often giving presentations at corporations, museums, and educational institutions. Preparing for these presentations, answering questions from the audience, and interacting with a wide range of new acquaintances forced me to think about how such projects fit into a broader context. Was there real value in these impractical vehicles that operated at the border of biological and mechanical flight? What is the role of competitions? Do the developmental techniques have more general validity? This thinking edged into the subjects of invention/innovation by man, evolutionary invention/innovation by nature, how our minds work, the teaching of thinking skills, and how all of these relate to civilization's challenges and future.

One insight seemed especially significant. It dawned on me that what I considered big problems, such as overpopulation, starvation, carbon dioxide buildup, the disappearance of rain forests and top soil, the fanaticism of cults, 20th-century weapons in the hands of 10th-century cultures, nuclear proliferation, and so on, were merely consequences of the real problem, the human mind, individually and collectively — how we build up belief systems, why we follow certain leaders, how we perceive present and future problems, and how we organize to resolve them. There is no more important subject than the human mind, both to humans and to all the life forms with which we share this fragile globe.

The mind's creativity, logic, and other wonderful attributes make us optimistic about the future, but these attributes are severely limited by negative characteristics, particularly the narrowness of our thinking. Our mental blinders are particularly pernicious because we are usually unaware of them. Inputs to and processing by our brains involve a filtering from prior experiences. On the plus side this results in efficiency: we see patterns from just a few clues and waste little time on unproductive avenues. On the minus side, this narrowing or prejudice closes off options. A simple example is when we ask for the solution to a problem. That innocuous word "the" immediately narrows our thinking, and we instinctively search for a *single* solution. Our language, and all our cultural institutions, narrow the way we think in some respects while broadening the way we think in others. We do not realize how thoroughly our culture molds us; we believe we are the puppeteers of our actions, not comprehending how our individual backgrounds pull our strings. We usually do not appreciate the experiences of others or perceive that if we had a similar upbringing we would probably think and act as they do. We also instinctively assume the human perspective on every subject and thus manifest a conceit for our human ability and destiny. We worry about the demise of rain forests primarily because *we* may lose some potential medical discovery or because an increase in the CO_2 problem may imperil civilization's convenience. We have a hard job perceiving ourselves as relative newcomers to, and fellow animals in, an interdependent world of delightful biological diversity.

Before exploring actions that an individual might take for upping the odds on a comfortable global future, there should be an assessment of the problems and their underlying causes. There is no dearth of information on the subject of pressures on the environment. There are television documentaries, newspaper articles and editorials, the annual *State of the World* book from the Worldwatch Institute, popular books and articles by authors such as Carl Sagan, Isaac Asimov, and Stephen Jay Gould, and many contributions to the professional literature. Some of the same sources, as well as many others, also treat the subject of technology's confrontations with ethics and social institutions, confrontations that directly produce pressures on civilization and hence also pressures on the natural environment.

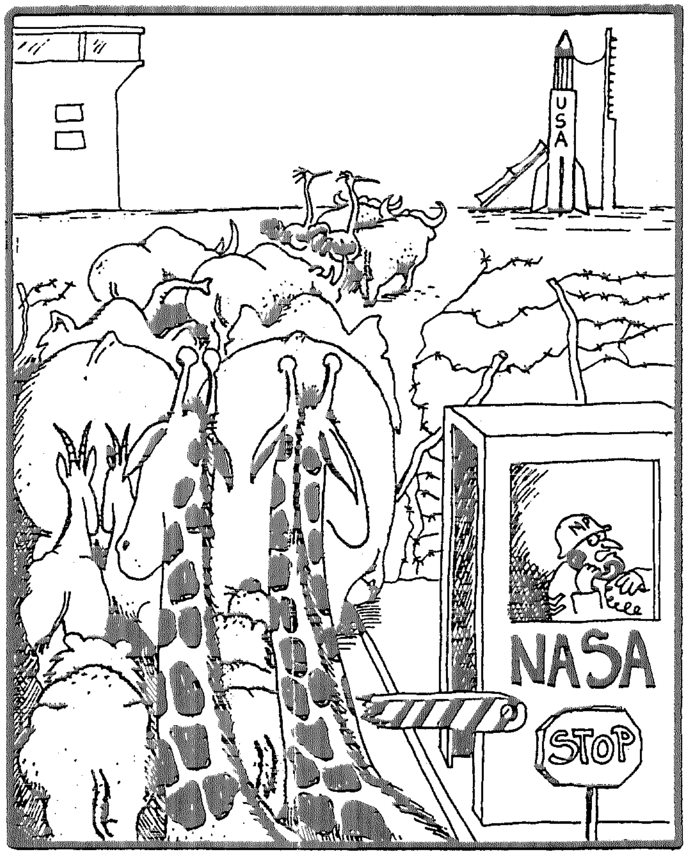
People who viewed the future with alarm, such as Malthus, have in the past been wrong — or at least premature. But there are limits to the carrying capacity of the earth. A mere 100 years ago, after 3.5 billion years of life on earth, the human population reached 1 billion. Now, in the next 10 years or so we will add another billion to the present 5 billion, and the growth will not stop. In addition, there are increasing per-capita expectations, demands, and consumption. Further, pressures from human activity are quickly distributed around the globe — by advanced communications and travel, as well as by atmospheric and oceanographic transport. A simple analogy to the problem of limits is to represent the earth as a balloon. Each breath, each new pressure, makes the balloon more beautiful, and experience with all the prior breaths shows there is nothing to worry about. The pessimist who says, "Don't put in another breath," or "Don't put more stress on the global ecosystem," is continually shown wrong, but will one day be right. The wisdom of the past indicates that growth and increased pressure are not to be feared. But we have only *one* balloon and no experience with others, and we cannot afford to make a mistake. Our predictions about popping must be based on rational evaluation rather than on experience; unfortunately, the stresses are building so fast that the wisdom of the past is an inadequate basis for solutions.

In the past, agricultural and mining cultures often consumed their resource base. Then, whether or not they had listened to their "Malthus," they collapsed or moved elsewhere. For our present global civilization, which mines nonreplenishable resources and strains the carrying capacity of the atmosphere and oceans, there is really no "elsewhere." Inevitably, civilization will reach an accommodation with this limited earth. The question is whether this accommodation will be comfortable or catastrophic. Incidentally, some people think of space as the safety valve to let future generations decrease the pressure on the earth, but I have never met anyone who seriously thought that a century from now space will actually absorb even 1/100 of a percent of the annual population increase. I believe that the challenges and solutions for us all are tied to this fragile globe and that space technology is a tool and a catalyst for broadening perspectives; it will not provide an escape during the next few decades, before the pressures become uncontrollable.

Some of the most troublesome future challenges posed by advances in science and engineering relate to ethics and philosophy. Albert Einstein put it depressingly: "Technological progress is like an axe in the hands of a pathological criminal." In particular, robotics, artificial intelligence, and medical breakthroughs now pose ethical questions beyond the reach of even a mythical Solomon's wisdom. For example, robotics and the artificial intelligence revolution assure that in several decades the material needs of this country can be met by relatively few workers. Will there be a satisfying role for the unnecessary workers? Will the haves and the have-nots become more polarized? What about the dangers to the democratic system as new scientific, interactive studies of audience response give the charismatic politician even greater impact through television?

As man increasingly becomes master of life and genetic evolution, the questions get

Paul MacCready poses with the solar-powered Sunraycer, which his company, AeroVironment, designed in collaboration with General Motors and Hughes. The vehicle, which will compete in a 2,000-mile race in Australia at the beginning of November, scored the lowest drag coefficient ever recorded in Caltech's 10-foot wind tunnel.



"The Far Side" cartoon by Gary Larson, reprinted by permission of Chronicle Features, San Francisco

"Something big's going down, sir . . . they're heading your way now!"

tougher. Should we encourage procreation in countries where starvation from overpopulation and a dwindling resource base await the children? Should we use expensive and extraordinary means to prolong the lives of terminally ill patients? Is the creation of test-tube babies to be welcomed? Should information from amniocentesis be used as the basis for terminating a defective embryo? How do we respond as developments in artificial intelligence, robotics, gene splicing, and organ substitution blur the dividing line between what is natural and what is technological? Our culture is ill-equipped to comprehend and assimilate, or plan and control, such technological benefits. Our political, economic, social, and religious institutions have great inertias and respond slowly to sudden new pressures.

No single technological advance will be the key to a safe and comfortable long-term future for civilization. Rather, the key, if any exists, will lie in getting large numbers of human minds to operate creatively and from a broad, open-minded perspective, to cope with the new challenges. I have some optimism because I think minds can be opened more readily and quickly than is usually assumed. The minds of scientific and technological professionals are especially important because of the leadership roles of such people, but in the long run it is the minds of young students, those who will be the solvers or sufferers, that are critical. It is most important that students learn how the human mind works and that they develop broad thinking skills, not just the ability to store facts and react. Schools in the United States are edging toward fostering the important but hard-to-quantify skills, attributes, and abilities such as creativity and problem solving, seeing two (or more) sides of an issue, realizing why others perceive differently, having healthy skepticism and an ability to sort out fact from fiction, comprehending the big picture and the dominant factors, developing an instinct for questioning, and evaluating the consequences of actions. All of these promote an enthusiasm for both the natural and the man-made world. I think they are essential for giving civilization a chance; fortunately, because they also help individuals to be happier, to be more productive, and to make more money, such training can be "sold" to schools, individuals, and businesses.

Such thoughts suggest that one socially useful action for an individual is to support (and use) organizations and activities that are directed at stimulating thinking and broadening perspectives. In the last few years in an unsystematic way I have directly encountered many groups that deserve support, and I will cite a few here. I am aware that many other worthy ones exist. One thinking-skills training program that provides especially simple but ingenious techniques for deleting mental blinders was developed for schools and businesses by Edward de Bono of England. Innumerable courses in creativity are available, probably all useful, but the de Bono method deals with broader skills than most and, relatively independent of the IQ or the socioeconomic circumstances of the trainee, appears to yield surprisingly high returns per hour invested. The program doesn't tell you what to think; it's somewhat like cleaning your glasses — you see better, but you still determine what to do with what you see.

The OM Association — formerly known as Olympics of the Mind — is another organization that effectively fosters thinking skills. This rapidly growing group cooperates with more than 5,000 schools, challenging youngsters to work together in hands-on activities that emphasize teamwork and divergent thinking in friendly, humorous competitions. Teams select subjects from fields such as engineering, computers, art, and history, among others, and participate in state and national competitions. OM, which receives support from IBM, was featured in an episode on Bill Moyer's "Creativity" television series.

Another effective group is the Lindbergh Fund, a foundation dedicated to perpetuating (through grants and symposia) the mission to which Charles Lindbergh devoted the last half of his life: seeking a balance between technology and nature. Incidentally, the wide-screen IMAX film, *On the Wing,* sponsored by Johnson Wax and the National Air and Space Museum, suits the foundation's philosophy by dramatically showing the connection between the evolution of natural flight and the evolution of aircraft. Our flying replica of a giant pterodactyl (*E&S*, November 1985) was created to fit into both portions of the film. We also felt that this dinosaur-like flying reptile could harness the enthusiasm for dinosaurs that all youngsters have, an enthusiasm that can help lure a few more people into science or at least into comprehending evolution.

I am involved with several other groups that broaden people's perspectives. One is the International Human Powered Vehicle Association, which stimulates invention by setting up races of low-power, high-technology vehicles (land, water, or air) without the stifling influence of rules. Others are CSICOP (the Committee for the Scientific Investigation of Claims of the Paranormal), and a related group that has been meeting regularly at Caltech, the Southern California Skeptics. These are dedicated to sorting facts from fiction, to investigating and providing, if possible, rational explanations for unusual observations, and to serving as a resource of rationality for members of the media willing to draw on their expertise. These groups explore the mechanisms of gullibility. Magicians are especially effective members because they have excellent insight into techniques of deception and the prevalence of self-deception. In fact, if magic clubs were organized in every high school, millions of students would benefit from first-hand knowledge of how gullible we all are — a humbling lesson in how the mind works.

Beyond supporting organizations that work directly on developing broader thinking, individual scientists and engineers can take other actions as well:

- Ask yourself the probable 20-year consequences — good and bad — of the scientific or technological field you are working in.
- Organize a session at the next national meeting of your professional society to focus on the ethics and broad consequences of your field. Perhaps a joint session with a non-technical society would be useful.
- Write letters to the editors of newspapers and magazines when you think news stories, articles, and editorial discussions are moving in the wrong direction.
- Be willing to devote time to making presentations to school boards and textbook committees. In California a few spirited scientists and engineers have recently been instrumental in reversing the "dumbing down" of textbooks. Most of us have been too lazy to be concerned.
- Volunteer Saturday mornings to teach in special science programs, to help a computer club, or to get young people to museums or out on nature hikes.
- Organize visits to your technology company by school children, garden clubs, religion classes, art groups, and so on — all sorts of people who should become more familiar with technology even if they are not involved with it professionally. Never forget that people outside of technology provide the main resources, votes, and standards that determine technology's economic viability.

Establish colloquia, salons, or informal once-a-month breakfasts where people feel free to discuss and argue about "big" and controversial issues (such as "what is man?," "religion vs. science," "man's responsibility for species extinction," "global survival," "communications in a 21st-century democracy,") I have found that technologists are often eager to discuss such issues but rarely find themselves in circumstances where such discussions are generated.

In the end, technology does not exist by itself. Rather, it fits into a global, ethical framework, where serious, complex questions and concerns arise related to the survival of humankind, nature, and civilization. It is appropriate that those of us involved in the development and use of technology devote attention to consequences and solutions (whether or not the solutions involve technology). We must not succeed in our various short-term goals and find that we thereby lose the grander game. □

Priestley and me

by John D. Roberts

Jack Roberts receives the Priestley Medal from Mary L. Good, president of the American Chemical Society.

I AM VERY PLEASED and greatly honored to receive the Priestley Medal. Let me start off by saying that I am very deeply appreciative of the marvelous students and postdoctoral fellows whose achievements I have cheerfully taken credit for over the years, as well as the representations (and I hope not misrepresentations) that I assume to have been sent in on my behalf, by colleagues and friends, to make possible my being with you tonight. I am reminded of a cartoon I saw in the paper the other day, wherein an Oscar recipient, clutching his statuette, is standing in front of a microphone, saying, "That covers the thank-yous to the people of the first year of my life. Now, for the second year . . ."

For those of you who may be in this spot next year, or in future years, let me warn you that getting the Priestley Medal is definitely not like getting an Oscar from the Motion Picture Academy. There is no last-minute surprise; indeed, it is more like the water cure. The chairman of the American Chemical Society board will call you up in April and ask if you will accept the medal, and then, if you say yes, you will have about 360 days to worry (and I'm inclined to worry) about what you are going to say when the big day arrives. And, in the meantime, you will get a three-inch stack of letters (and very nice letters indeed) congratulating you for having already received the Priestley Medal, long before the fact. All of this at a time when you have no certainty as to whether you will even be alive for 360 more days. I decided I would answer those congratulatory letters after I had the medal in my hand.

Of course, the ACS board does not tell you why you were chosen over other worthy candidates. When I was much younger, I had the perception that the Priestley Medal was awarded almost exclusively to those much-admired and selfless individuals, such as

Roger Adams, Charles A. Thomas, and W. Albert Noyes, Jr., who were not only great chemists in their own right, but also served with distinction as the ACS president, ACS board chairman or the like. However, a review of the list of past awardees indicated that such service is not necessarily the most important factor. So, as an experimentalist, I thought it might be interesting to see if I could find a more common trend, and I did.

Taking the last 20 awardees as a representative sample, the common factor is not field chemistry and not ACS service. It turns out to be maturity. You have to ripen to get into the club. Whatever the other requirements, you just plain have to live long enough. This became clear when I plotted the age of the Priestley recipients against the year that they got the medal. There is a very distinct upward trend. Naturally, there is some scatter in the plot, but, with a correlation coefficient of 0.66, the intercept of the least-squares line is 60 years, and the slope is unity. For the less mathematical of you, this means that the most probable age for the Priestley Medalist in 1966 was 60, and that age has increased by 10 years every 10 years — so that in 1996, the most probable age will be 90 and by 2006, it will be 100! I feel especially honored to be chosen about ten years ahead of the current expected norm.

Having demonstrated the will to achieve the maturity required for the Priestley award, let me say why it is so wonderful to become associated with the name of Joseph Priestley. The fact is, if you do a little research on Priestley, you start to wonder how it was, in the early 1920s, that the somewhat conservative American Chemical Society was willing to take him as the symbol of their highest award. No doubt that Priestley was a remarkable man. He achieved scientific immortality for the discovery of oxygen — an element essential to life and an element of great interest and importance to chemists. But Priestley was not a chemist — his contemporaries thought of him much differently. He was a minister; he held several academic positions for teaching languages, of which he knew at least eight; he was a vigorous spokesman for educational reform; to be sure, he was a natural philosopher (which was what scientists were called in those days), but he was elected to the Royal Society not for his discovery of oxygen but for his research and writings on static electricity; and, finally, he wrote rather extensively on psychology, a subject in which he was profoundly influenced by a man named Hartley, who apparently was one of the first to approach psychology as a science.

None of Priestley's professional activities by themselves should necessarily cause the ACS any great concern, and certainly the discoverer of oxygen could be claimed to be a chemist, regardless of what was written on his union card. What might cause more concern is the undisputable fact that Priestley was a very substantial thorn in the side of the Establishment. He was raised a strict Calvinist, but as a minister he was soon regarded as "unsound on doctrine." In fact, his religious beliefs became radical, and he was the godfather, if not the father, of the modern Unitarian Church. He waged a vigorous battle with Parliament and the Church of England for religious freedom. He fought so that those who were nonconformists to the doctrines of the Church of England could be admitted as students to Oxford and Cambridge and also hold civil and military positions. He fought for educational reform; the then current school curriculum was, in his words, "an object of ridicule." Furthermore, he strongly supported both the French and American Revolutions, as well as movements

A revolutionary Joseph Priestley is depicted as "Doctor Phlogiston" in a 1791 political portrait.

to abolish slavery. He wrote a flood of books and pamphlets outlining his views on these subjects.

His enemies, of whom there were many, characterized him as "a damned rascal . . . a fellow of treasonable mind . . .'Gunpowder Joe,' who sought to overthrow Church and King." He was even shunned by his Establishment Fellows in the Royal Society. The tension finally became so great that in 1791 mobs were incited to loot and burn his church and home. Finally he was glad to follow his sons and emigrate to America in 1794.

Today, all of us — liberal, conservative, or whatever — can applaud the causes for which Priestley fought: for religious freedom, for educational reform, and for personal liberty. But I think it would be wrong to assume that 'Gunpowder Joe,' transplanted to today's world, would be a contented middle-of-the-roader. I am pretty certain he would be an environmentalist; he criticized Paris, citing ". . . the narrowness, the dirt and the stench of almost all the streets." Priestley would surely not be a Marxist; he was too elitist for that. But his writings emphasized deep conviction to the principles of freedom of thought and freedom of inquiry. Today, those principles would certainly bring him into conflict with the creationists, with the religious right, with apartheid, with militarism, and indeed, with the forces of anti-intellectualism, repression, and injustice, wherever they might be found. A modern Priestley counterpart is my colleague, and past Priestley Medalist, Linus Pauling. I am glad that Linus is also associated with Priestley, not only for his contributions to chemistry, but even more for his adherence to the same high moral and social principles. Chemistry — indeed the world — needs more men and women, with not only the ideals of Priestley and Pauling but also with the same willingness to work to establish those ideals in a far-from-perfect world.

Having paid homage to Priestley, I guess I am expected to offer in the remaining minutes some pearls of wisdom or inspiration. I feel wise in only one respect, and in a way which will be probably not very inspiring. The fact is that, in looking back from near the age of academic retirement, I recognize that throughout my life I have been very fortunate, indeed downright lucky. And not just because I have had so many wonderful experiences as a student, postdoctoral fellow, faculty member, and university administrator; nor through being involved with the ACS, chemical industry, textbook publishing, the book series *Organic Syntheses*, the National Science Foundation, the National Academy of Sciences, as well as a lot of travel at home and abroad. I have had more than my share of being in the right place at the right time.

Let me illustrate. Having never been much good at physics or mathematics, I feel I was very fortunate to get started in chemistry during a period when it was a much more descriptive science than it is today; when a knowledge of glassblowing was more important than a knowledge of electronics or quantum mechanics; when slide rules and log tables were our computers; and when the fanciest instrument in the organic laboratory was a refractometer or possibly a polarimeter.

Those things I could understand.

Of course, a lot of other people were also fortunate in starting in chemistry in "the good old days," but I was additionally fortunate in starting my undergraduate work at UCLA in 1936. At that time, UCLA had no PhD program, but it was on the verge of getting one. As a result, UCLA was able to hire bright, young chemistry faculty eager to do research. And those faculty encouraged me to get into research early — in fact, at the end of my sophomore year. This was very important to me because, although I was no great shakes at course work, it turned out that I was pretty good at research and I loved it. Almost for the first time in my life, I did something really well.

With only a few master's degree candidates and a growing undergraduate enrollment, UCLA was also short on teaching assistants. And again I was fortunate, because I was allowed to be the equivalent of a graduate teaching assistant — in six different undergraduate courses. I was not the only one to profit from this particular golden period at UCLA. During that time, the school produced seven future members of the National Academy of Sciences and of those, two became Nobel Prize winners in chemistry, and two became presidents of the ACS. Not bad.

Although I finished UCLA with four rather decent undergraduate research publications, these were not enough to overcome a spotty scholastic record and get me admitted to Wisconsin for graduate study. However, Penn State was willing to take a chance, and I was again fortunate (even if for only a brief period because of the start of World War II) to work with Frank C. Whitmore — a remarkable organic chemist, who became a lifelong inspiration.

At the end of my war research UCLA had gotten its PhD program going, and I was fortunate again to have a really bang-up PhD project with William G. Young, who himself later became a Priestley Medalist. The frosting on the cake of my graduate period was to be able to interact in a very close way with Saul Winstein, a physical organic chemist of remarkable scholarship, imagination, and intellectual tenacity.

Then I was indeed lucky not to be offered a job at DuPont, but instead to go off to Harvard as a postdoctoral fellow, just at the time that R. B. Woodward was getting started there and when Paul Bartlett and Louis Fieser were in their prime. It was a confidence builder to find out that a country boy from the far West could more or less hold his own among the Harvard graduate students and postdoctoral fellows. Bartlett and Woodward helped greatly to shape my perception of what one's objectives should be in research, and the Harvard year was a great experience. It was easy to appreciate how lucky I was to be there.

And yet, I certainly can't claim to have always recognized good fortune immediately when it came my way. Thus, I had hoped after my Harvard year to get a teaching position at Berkeley and was disappointed, even a bit dismayed, when the only opening turned out to be at MIT, where Arthur C. Cope was just beginning to revive and renovate organic chemistry. Getting in on the ground floor at MIT with a dynamic leader like Art Cope and colleagues like John Sheehan and Gardner Swain turned out to be good fortune beyond belief. And I was, and I am still, very grateful to MIT for the opportunity I was given there to get a research program under way. Admittedly, it was a bit ungracious to leave in 1953, but I felt I repaid MIT in spades by persuading Art Cope to sign up George Whitesides (Caltech PhD 1964) for a faculty position almost a year before he got his PhD at Caltech. Still, it was painful to leave Cambridge, just as another of my heroes, Frank Westheimer, was moving from Chicago to Harvard. But the culmination of my academic good fortune was to be offered a professorship at Caltech — a small institution, but one with a lot of clout. When I travel and meet people, they often ask how large the Caltech student body is. I always ask back —"How large do you think it is?" The answer usually ranges from 10,000 to 40,000 and the truth of about 1,800 comes as a shock. Caltech turned out to be the ideal place for me to do science.

Of course, I have had my share of missed opportunities. Somewhere around 1951, Richard Ogg of Stanford tried to convince me that nuclear magnetic resonance (NMR) spectroscopy was going to revolutionize chemistry. Being nearly illiterate in electricity and magnetism, I did not even understand what he was talking about. Four years later I was fortunate, in the course of my DuPont consulting, to have William D. Phillips show me what NMR could do when applied to specific organic structural and rate problems. Only then did I realize how right Richard Ogg had been. At that point I didn't care whether I would ever know how NMR worked, I just knew it would solve problems that I was interested in, and with the help of Linus Pauling the Caltech administration, bless them, came up with the funds to buy the first commercial NMR installation in a university. And so I was able to ride the early crest of the NMR wave which has swept along through chemistry and biochemistry, as well as into medicine, with growing intensity and importance for almost 40 years.

I was also fortunate to be able to ride the early wave of the application of molecular-orbital theory to organic chemistry. The later molecular-orbital waves have gotten so big, so steep, and so hard to ride that I've been happy to stay on shore. Still, around 1950, Hückel molecular-orbital theory (the simplest kind) had been cleverly, and carefully, kept secret from organic chemists by the theorists. "Too tough mathematically for you guys" was the watchword. But, one day, I was lucky to look over the shoulder of my friend, William G. McMillan, one of the high priests of theory, and find to my surprise that he was using simple algebra to solve a molecular-orbital problem I was interested in. I said, "Hey, what's going on here? I can do that too!" Talk about being in the right place at the right time! So wisdom not only comes with good fortune. Sometimes you need good fortune to hit you over the head!

The modern era that I have lived through has had some very bad scenes: things like the Great Depression, the Nazi period, several disastrous wars, and the despoiling of our natural resources. Nonetheless, I feel grateful to have lived in the heyday of the Petroleum Age; to have been around when movies began to talk and when wireless communication went from crystal sets to color video; to be able to jet with abandon from coast to coast; to see the arrangements of atoms in space for molecules as complicated as proteins and viruses; to see closeups of the planets and their moons, from Mercury all the way out to Uranus, with Neptune soon to come; to see the back of the moon; to touch a lunar rock. I'm grateful that I've been around for all of this and more — before the crazies evaporate everyone but the few remaining deep-cave dwellers for no better reason than that some people live by simplistic slogans, such as "better dead than red" or "better dead than red, white and blue."

You may well infer that I have become a nostalgia freak and perhaps even think that I have decided that science cannot go much further after I retire. Not so. I don't think we are anywhere near the limits of the capabilities of science. A lot has been done, but we are still only scratching at the surface. I am enormously excited about the potential of learning more about the nature of the universe — on the one hand, by exploring the cosmos with the aid of devices like the space telescope, gravity-wave detectors, and other goodies to come; and on the other hand, by trying to achieve an understanding of matter all the way down to the properties of those exquisitely minute entities that the physicists call "strings."

It is clear to me that chemistry has a very exciting future in all of this, because it is so close to us and so much a part of what we are that it immediately affects our lives. It is fabulous that chemistry is taking over research in such things as superconductivity, as well as design and synthesis of other super materials for all kinds of purposes. Furthermore, chemistry disguised as "molecular biology" is leading us down the road to understanding life. Ultimately, sometime, in some way, chemistry will even help us understand how we possess and use the marvelous gifts of cognition, of reasoning, of humor, of love, of appreciating in the small constrained way that we can, despite the pain and anguish we may feel at times, the miracle of being alive.

Perhaps I am fortunate in my optimism, but the future looks wonderful to me. But we must control the seemingly inborn, very stubborn defense mechanisms, which make us fear, and too often make us wholly intolerant of people who are at all different from the way we are — whether the differences be in language, geographic location, religion, color, political beliefs, or social status.

Finally, let me say that, besides a love of science and a love of freedom of thought, I share with 'Gunpowder Joe' Priestley another measure of good fortune — a wife, a daughter and three sons. As to wife, Priestley's own appraisal and expression of appreciation can hardly be improved upon. His words were: ". . . a woman of an excellent understanding, much improved by reading, of great fortitude and strength of mind, and of a temper in the highest degree affectionate and generous, feeling strongly for others and little for herself."

My thanks to her and to all of the others, here and elsewhere, who have made it possible for me to be here tonight. □

Research in Progress

Cruisin' for Credit

MOST DRIVERS HOPE to avoid traffic jams. Not juniors Peter Hughes and Brad Solberg. They cruised the freeways this summer looking for trouble. Their goal: to develop a better computer program to predict traffic jams.

Hughes and Solberg's SURF (Summer Undergraduate Research Fellowship) project compared freeway observations with computer simulations. They based their simulations on existing computer models, in which traffic tends to move in waves. These waves are normally invisible to the average driver, but can be seen as the sequential flashing on of brake lights as traffic approaches a slow driver ahead, for example. The students' observations helped document the wave model, and enabled them to contribute some details of their own. For example, drivers have differing reaction times and preferred driving speeds, just as cars have different maximum accelerations and wind resistances.

Hughes and Solberg's model recreates these variations in detail. The model encompasses eight vehicle types, including four kinds of passenger cars, assorted trucks, buses, and 18-wheelers. The model uses 18 variables to describe a car-driver combination. This gives sufficient detail to populate a model freeway with the hotshot lane-changer in the brand-new, cherry-red Porsche Targa, the elderly driver in the '67 Rambler who drives at 40 mph in the center lane, and everyone in between. Additional variables describe the number of lanes, visibility, and related freeway characteristics. Some of the variables, such as AMAX (maximum acceleration) and BRTIME (braking reaction time) were borrowed from previously published models. Others, such as SPAZ (acceleration uncertainty) and BOC (braking overcorrection factor) came from their own observations of freeway flow.

Peter Hughes (left) and Brad Solberg videotaped freeway traffic from strategic hilltops.

Before sending any electronic cars onto their silicon freeway, the two spent many hours on the real thing, watching the behavior of real-life motorists. These observations initially helped determine what variables governed a driver's actions. Later, when the model was being tested, the antics of a simulated driver were compared to these same observations to determine the model's accuracy.

The pair set up several observation posts overlooking the freeways. The posts overlooked points where traffic jams could be expected to occur, such as freeway interchanges and long uphill stretches, as well as areas where traffic flows freely. They videotaped traffic for hours on end. "We learned a lot just by watching the traffic," Hughes said. "We could see waves [of higher density traffic] combining to form traffic jams. The San Diego Freeway through the Sepulveda Pass was especially good to watch, because it's a long hill with few exits."

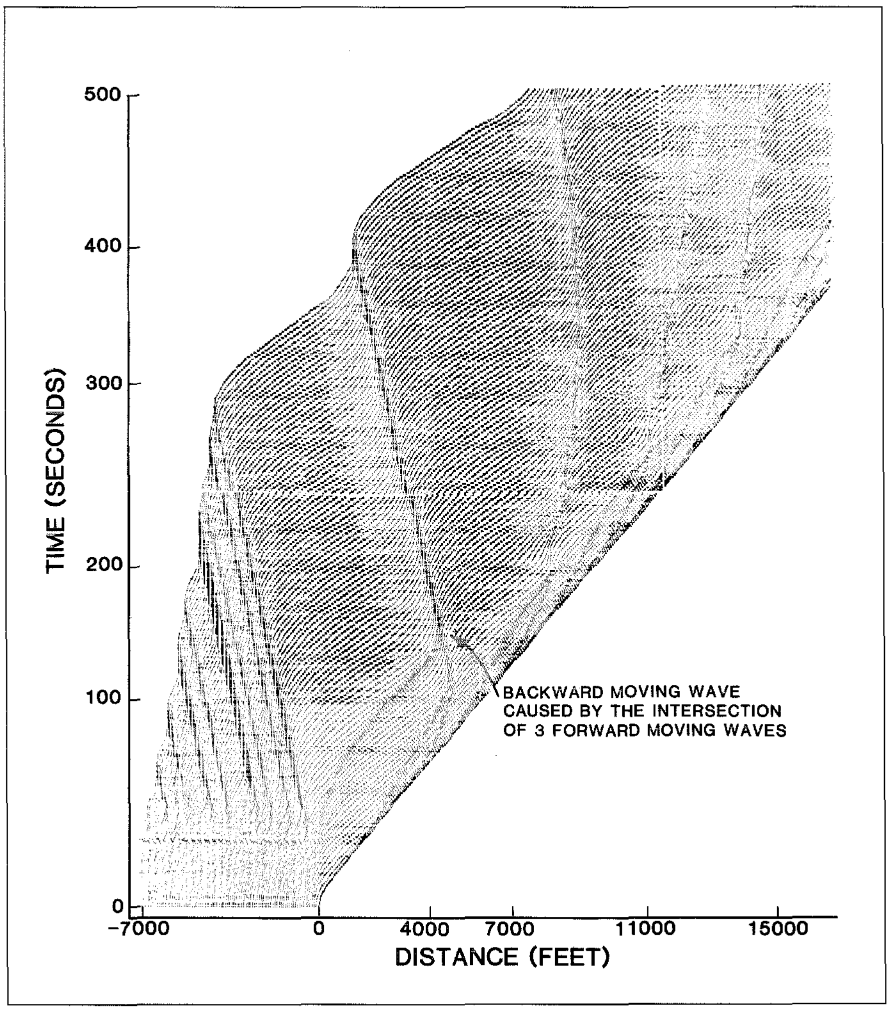
After several days videotaping traffic, the two students were almost ready to take to the roads themselves. But before any meaningful speed data could be collected, they had to be sure their speedometers were accurate.

They calibrated their speedometers by driving a measured stretch of road near campus at various speeds and comparing their times to calculated values. "I have a Datsun B-210, and Brad has a '72 AMC Matador. It turned out that my speedometer was off by a constant 10 mph, while Brad's was off by a linear factor," Hughes said. "We wound up using Brad's car mostly. At least it settled the argument about who has the best car."

Then, armed with a digital stopwatch and a carefully calibrated speedometer, they were ready to roll. They patrolled the Ventura, San Diego, and San Gabriel Freeways. They recorded their speed and mileage at ten-second intervals (five seconds during traffic jams). The speed/time data would be converted to speed/distance, acceleration/time, and acceleration/distance plots on the computer.

They drove the same stretches of freeway over and over again at different times of day. The accumulated plots from off-peak hours enabled them to determine the effects of hills, curves, interchanges, and other fixed features on traffic flow. Traffic jams were not hard to find, however, as these same stretches of freeway clog regularly every rush hour. A portion of the Ventura Freeway, in fact, has the dubious distinction of being the busiest freeway in the nation, according to the United States Department of Transportation.

Between time and speed recordings, they observed their fellow motorists' driving habits. Hughes summarized their observations: "There are a couple basic types [of drivers]. One is the guy who picks a lane and just sits in it and will drive at whatever speed he feels like. It's pretty variable how well people control their speed. Sometimes people will be driving along and will go down a hill and won't watch their speed. Other people are very careful. Then there are the types who drive just as fast as they can, and weave from lane to lane when other people get in their way. Then there is the driver who'll drive along, usually in the fast lane, just as fast as possible, and when he comes up behind someone he'll tailgate and honk his horn and flash his lights till the guy moves out of his way. We started seeing a lot less of that sort of behavior in the second half of the summer after the freeway shootings started. . . . That changed everybody's driving strategy. Fewer people were driving in the left lane, people were letting people in a little more often, and we were seeing people looking around a lot more."



Time-distance plot for all the cars in a 100-car simulation. The simulation begins with all the cars stopped bumper to bumper. Each car is represented by a line. The plot shows how small waves of greater-than-average density combine to form very-high-density waves — traffic jams. The plot also shows the low-density areas following the jams.

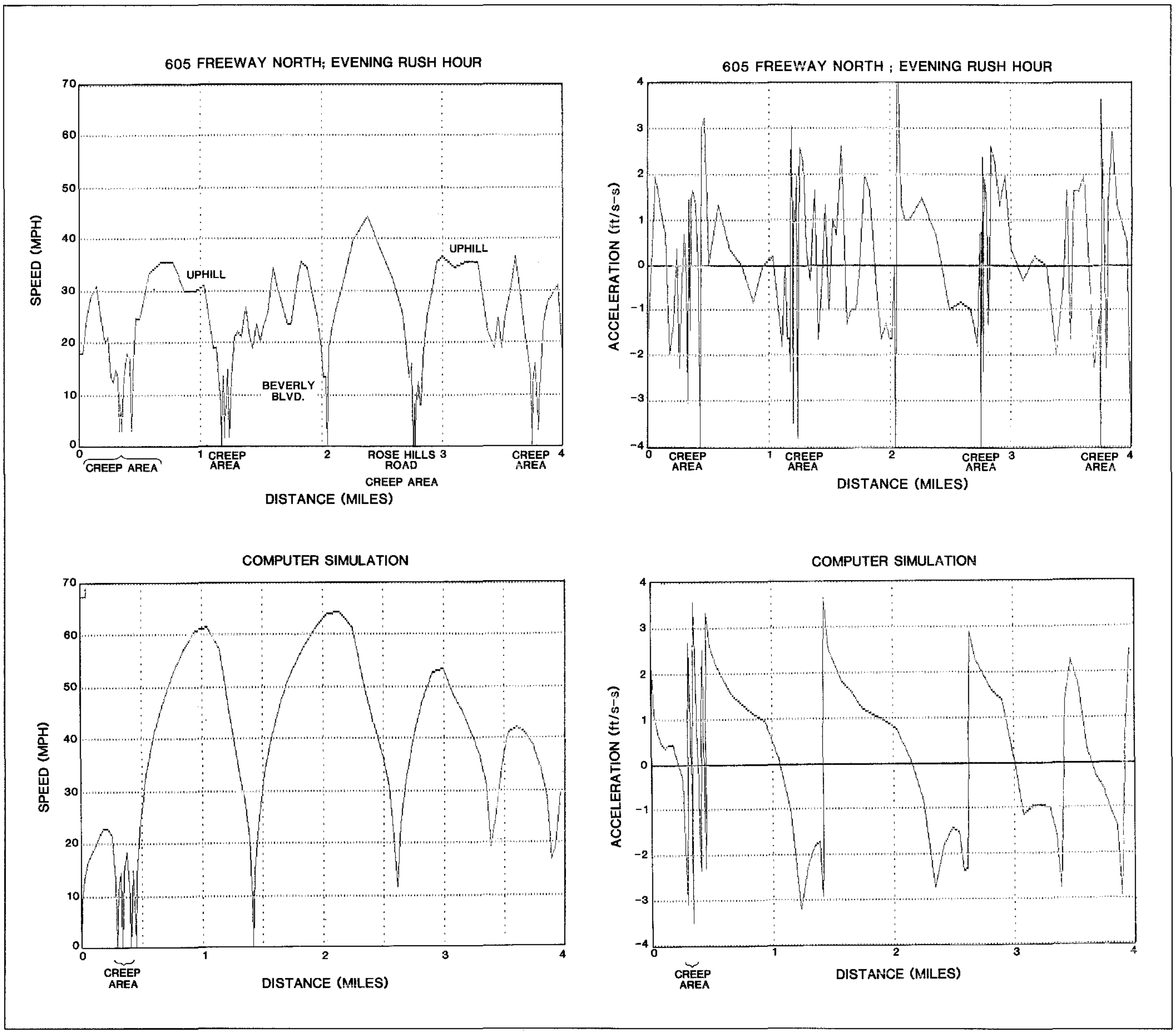
The pair developed a set of variables to describe the driving strategies they observed. The variables quantify subjective responses such as driver frustration. Now the real work could begin: integrating the new variables into existing programs to produce realistic behavior.

They divided the model into two sections, a "car-following" routine and a "lane-changing" routine. "Previous models had also combined these problems," Solberg noted. "But ours added more individuality to both maneuvers."

The car-following routine simulates a driver's efforts to maintain a preferred speed. Hughes explains, "People will brake or accelerate to where they will avoid hitting the car in front of them but yet will approach their preferred speed. Most other car-following models we saw had a breakdown into two separate situations: when you're not constrained by another car [in front of you] and when you are constrained. We fit them together, saying that a person will see more and more effect as they approach the car ahead of them. People react a lot more to large speed differences [relative to the car ahead] than they do to small ones. It's a more-than-linear relationship, so we used a squared relationship." They finally derived an equation which included the square of the velocity difference between the two cars, the distance between the cars, the preferred following distance, and the "braking overcorrection factor."

"The other important change we made from what researchers had done in the past was to put in this braking overcorrection factor. It makes the equations asymmetric. When people are braking, they'll brake harder than they need to, whereas when they're accelerating they will accelerate just as much as they need to," Hughes said.

The lane-changing routine presented a special problem. "Since lane changing is an all-or-nothing response," Hughes said, "we had to find a way to make the probability of this response proportional to the stimulus." They created a series of four steps to determine when a car changes lanes. In the first step, the computer determines if traffic is flowing freely. If it is, there is no need to change lanes. If traffic flow is constrained, the computer calculates the desire to



Speed and acceleration plots for a real car compared to the simulation. Both sets of plots show traffic slowing to a creep at intervals of approximately 0.7 miles. The acceleration plots show the braking overcorrection factor as a small hump following each large deceleration. The real life plot shows the effects of uphills and merging traffic.

change lanes as a function of the driver's "frustration factor" and elapsed time. After about 20 seconds, the driver is fed up and is ready to change lanes. Now the computer looks at the adjoining lanes, and if there is already a car there, or if changing lanes would cause a car in the new lane to brake unsafely, no lane change occurs. If the way is clear, the computer compares accelerations in the new lane and the current lane. If the current lane has the higher acceleration, no lane change occurs. If the new lane has a sufficiently higher acceleration, then the car changes lanes.

The model includes random variations, even for similar drivers in similar cars. The computer chooses the exact value of any variable based on a bell-shaped distribution around the mean value assigned to that class of car.

As the pair worked to refine their model, they compared computer generated plots to their field plots. "We set up a computer model and determined whether the waves formed were actually similar in shape to the waves we saw on the real freeway. Then we'd tweak the coefficients . . . We had a lot of people plowing into each other in our computer models when we were debugging them. In fact, that was our major way of figuring out that something had gone wrong. We had a collision flag in our program that went, 'SMASH CRUMPLE SCREAM MOAN' on the screen and the program would terminate. We figured that in normal freeway operation, even in a traffic jam, accidents probably won't happen if people are attentive," Hughes said. He added that they did not get involved in any accidents during their study, but that a few weeks later he was rear-ended by an inattentive driver on a surface street near campus. No injuries and little damage resulted.

The fruit of their labor is a computer simulation in which cars nearing slow traffic brake and change lanes realistically. The computer tracks all the cars on the freeway section, updating each car's position and velocity every 0.2 seconds. The data can be printed out as a trip chart for any given car; or as a snapshot of the entire freeway section at a given time; or at any point on the freeway for the duration of the test.

The simulation produces individual vehicle plots resembling field plots. A real car's velocity or acceleration, plotted against time or distance, gives a series of jagged, but reasonably regular, undulations. The peaks represent free-flowing motion between jams, while the valleys represent the jams. The braking overcorrection factor gives rise to the little hump seen at the end of each large peak on the acceleration plot. The peaks have a wavelength of approximately 0.7 miles.

When all the cars on the freeway are plotted on the same graph, a traveling wave pattern emerges. The plot shows a series of small waves of greater-than-average density. These ripples can travel forward, that is, along the direction of traffic flow, or backward. When two or more ripples meet, they merge to create a large, backward-moving wave of much higher density. A low-density zone follows, duplicating the area beyond the slowdown where cars burst free into empty lanes. This wave replicates those traffic jams, which, once a motorist has fought through them, have no visible cause. The model created these jams every 0.7 mile or so, duplicating Hughes and Solberg's freeway observations. The ripples also pile up at natural choke points, such as uphill grades, merging traffic areas, and stalled cars.

The computer plots tend to be simpler and more regular than field plots. Hughes hopes to make the model more realistic this year by expanding a driver's lane-changing options to include forcing his way into the adjoining lane; moving to cut off a car entering his lane; and losing his nerve partway into a lane change, aborting it. When these modifications are added to an expanded system, the simulation will enable transportation planners to simulate traffic conditions on their own freeways.□ — *DS*

The PASADENA Effect

Michael Pravica, Daniel Weitekamp, and Russell Bowers (l to r) prepare parahydrogen.

THE PASADENA EFFECT (Parahydrogen and Synthesis Allow Dramatically Enhanced Nuclear Alignment) produces greatly enhanced NMR signals that will allow scientists to determine the structure of reaction intermediates previously invisible to them. Predicted by Assistant Professor of Chemistry Daniel P. Weitekamp in June 1986, it has since been verified experimentally by C. Russell Bowers, a graduate student of his.

The discovery has many uses, as Weitekamp explains. "Looking at the structure and kinetics of catalytic sites, both in solution and at solid surfaces, will be the hot things. Many catalytic reactions occur on solid surfaces. When you want to design a catalyst, you have to figure out how it works first. Then you know how to design it better. Also, we can transfer the effect to nuclei that normally have very weak signals, such as ^{13}C and ^{103}Rh."

An NMR (Nuclear Magnetic Resonance) signal is generated when a nucleus changes its spin state. All other things being equal, the probability of making a transition from one allowed spin state to another is directly related to the relative populations of the two states. Nuclei, like Daniel Boone, want elbow room. The less crowded the destination, the more nuclei will make the transition and the greater the resulting signal. In the PASADENA effect, a sample of hydrogen gas is forced to choose a particular nuclear spin state. When that sample reacts with an asymmetric carbon-carbon double bond, the product molecule preserves that spin state long enough to be visible to NMR. The nuclei's headlong flight from the overpopulated spin state generates an NMR signal far out of proportion to the molecule's concentration.

According to Weitekamp, "The PASADENA effect signals are up to several thousand times bigger than the signals you could have gotten just by taking the product molecule and putting it in a magnetic field. If you had this molecule in a magnetic field, you'd have some population in each of four energy states, and the fractional differences between the amount in each of these four states would be something on the order of 10^{-5}. If you had 100,000 molecules in the highest state you'd have 100,002 molecules in the lowest state. With the PASA-

DENA effect we have no molecules in the highest and lowest states, and all the population just in the two middle energy levels until we turn on the radio waves. That causes a very intense transition. That makes for very large signal enhancements."

The four states Weitekamp describes are shown on the energy level diagram below. The hydrogen's energy (nuclear spin) states before the reaction determine the product's energy states. Quantum mechanical considerations divide the hydrogen molecule population into four energy states. Here's why.

Hydrogen, like all nuclei with an odd mass number or an odd number of protons, has a nuclear spin. By the laws of quantum mechanics, hydrogen's spin is limited to one of two possibilities: +½ (also known as the "up" or α state), and −½ (the "down" or β state). Since hydrogen gas (H_2) is a diatomic molecule, the spin states of both protons must be considered. The lowest energy state, where the rotational quantum number equals zero, is called parahydrogen; it is described in wavefunction terms as $\alpha\beta - \beta\alpha$. (Remember, α is up and β is down. Which atom is which doesn't really matter, since the molecule is symmetric and floating freely in space.) There are three other states accessible to room-temperature hydrogen gas. All have one quantum of rotational energy and are, in order of increasing energy, αα, $\alpha\beta + \beta\alpha$, and ββ. These three states together are called orthohydrogen. Room-temperature hydrogen interconverts among all four states, and the concentration of each is approximately equal.

If the gas is cooled, the proportion of parahydrogen increases. At absolute zero, the hydrogen will be 100 percent para. "It's been known for 50 years that you could isolate this pure nuclear spin state," Weitekamp observed. "But nobody's ever done anything with it. This nuclear spin state is seemingly very dull from the viewpoint of magnetic resonance because it has no magnetic moment. One spin points up; the other points down; so it has no observable magnetization."

It's the magnetization that makes hydrogen nuclei (protons) visible by NMR. The spinning, positively charged protons act like tiny magnets. When they find themselves in a strong, externally-induced magnetic field, they tend to align themselves with the field. Up spins are parallel to the field, down spins antiparallel. The parallel alignment is the lower, and therefore slightly more populated, energy state.

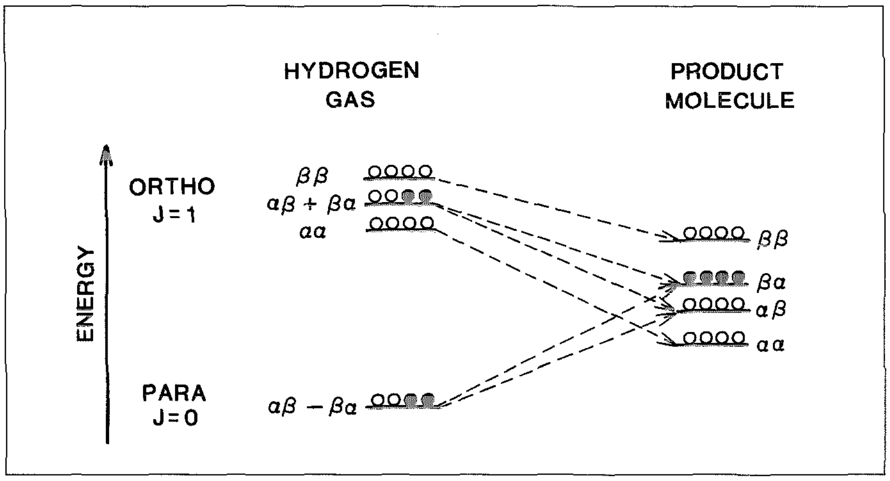
Protons can switch alignments (resonate) to a higher energy state by absorbing a quantum of the correct energy. A proton in the higher state could emit the same quantum and flip back to the low energy state. The net effect, absorption or emission, depends on which state is less densely populated. The required energy depends on the strength of the magnetic field. For the strongest fields practical in the laboratory, these quanta fall in the radio frequency (rf) range. The NMR technique places a sample in a strong magnetic field and bombards it with rf radiation. When a proton changes alignment, it generates a signal in a detector.

The NMR spectrum is a plot of absorption peaks versus field strength. Protons in different chemical environments resonate at slightly different field strengths, because other atoms in the vicinity can "shield" the proton from the magnetic field's full effect. The field interacts with nearby electrons, creating small secondary magnetic fields oriented opposite to the primary field. The shielded proton feels a lesser field and so resonates at a higher applied field. An interaction between nuclei, the J-coupling, can split a peak into a multiplet, a group of several closely spaced peaks.

But if parahydrogen has no net spin, it won't be affected by the magnetic field, so what's all the fuss about? "We use it as a chemical reagent and break the symmetry that prevented it from having any interesting spectroscopy," Weitekamp said. Molecular hydrogen reacts with carbon-carbon double bonds, forming a product with a carbon-carbon single bond and a hydrogen atom on each carbon. If the carbons were bound to different substituents to start with, the two hydrogen atoms would wind up in different magnetic environments. Suddenly, the hydrogens would not be equivalent any more. Now it matters which atom was up and which was down, because the two up-down combinations will be at slightly different energy states. The spins no longer cancel, and the protons suddenly become susceptible to the magnetic field. Half of the population winds up in each of the two available orientations, as shown on the following page, ready to make those intense transitions.

Once the theory had been worked out, the effect was simple to demonstrate.

Bowers prepared para- enriched hydrogen gas by cooling it. (At room temperature, all four spin states have almost equivalent populations.) As the temperature drops, the proportion of the lower-energy para- state increases. At liquid nitrogen temperatures (around 70K), half the molecules are in this one spin state. Unfortunately, the interconversion process is very slow. It takes several days for the ortho- and para- states to equilibrate



Normal hydrogen is equally distributed among four energy states (left). Hydrogen adding across an asymmetric double bond transfers its energy states to the product as shown by the dashed arrows. The black circles show how half the population in each of the $\alpha\beta - \beta\alpha$ *and* $\alpha\beta + \beta\alpha$ *states combine to form two new states, depending on which hydrogen is* α.

without catalytic help.

Bowers passed ordinary hydrogen through a coiled tube immersed in a liquid nitrogen bath. The tube contained a nickel catalyst to accelerate the interconversion process. In the meantime, a probe containing a premixed solution of substrate and catalyst was inserted into the room-temperature heart of the NMR's liquid helium-cooled superconducting magnet. When everything was ready, Bowers pushed a button to bubble a single pulse of parahydrogen-enriched gas through the probe.

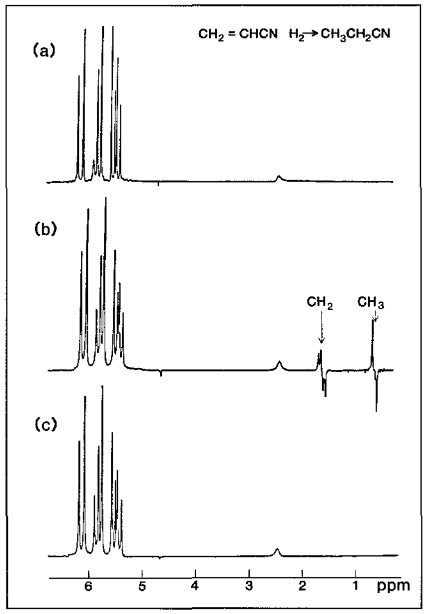
The experiment proceeded in a carefully timed sequence. The hydrogen pulse lasted for 1.1 seconds. A delay of 0.8 seconds followed, allowing bubbles, which would distort the NMR signal, time to escape. Next, a 6.0 μsec pulse from the rf transmitter stimulated spin-state transitions, which were recorded for the next 1.25 seconds. The instrument collected 8,000 digital data sets for Fourier frequency analysis in that small interval. This tiny length of time was sufficient to generate a product molecule peak as big as one that would take a day to acquire under ordinary conditions.

Weitekamp's group has observed the effect in several substrates having carbon-carbon double bonds. In the spectrum shown here, acrylonitrile (CH_2=CHCN) has been converted to propionitrile (CH_3CH_2CN). The hydrogens must be in magnetically inequivalent positions in the product molecule for the effect to occur. The cyano group provides the shielding difference needed to break the magnetic symmetry. Wilkinson's catalyst — tris(triphenylphosphine)rhodium(I) chloride ($Rh(PPh_3)_3Cl$) — was used to bind dissolved hydrogen molecules and add them across the double bond.

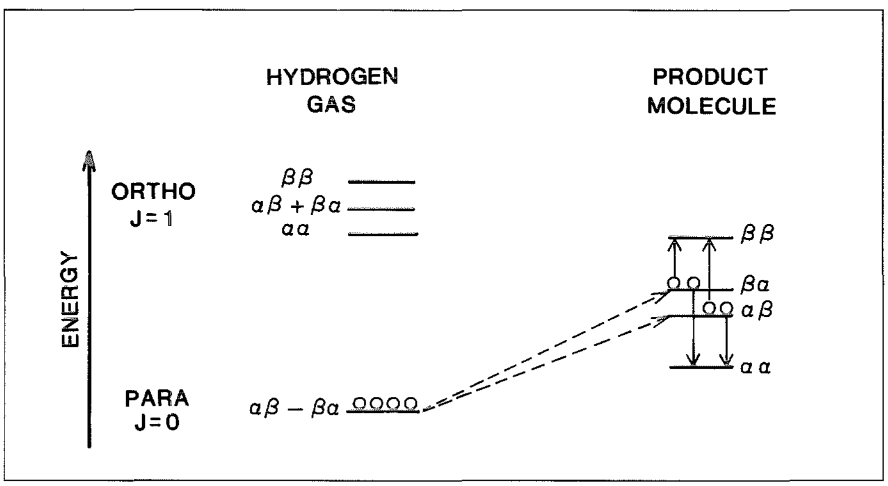
The experiments confirmed Weitekamp's predictions. The tiny quantity of reaction product, too small to be seen by ordinary NMR, identified itself with a readily detectable signal. The signal disappeared after a few minutes, as the effect wore off and the molecule slipped back into a normal population distribution.

Another set of experiments used the catalyst but no substrate. The two hydrogen atoms occupy asymmetric positions on the catalyst, so Weitekamp hoped to obtain an NMR signal from the reaction intermediate. As predicted, the reaction intermediate gave a strong signal.

The NMR signals did not merely appear; they gave new information about the molecules' structure. Unlike conventional NMR, the PASADENA effect allows transitions to lower energy states (emissions) as well as absorptions. Emission peaks appear inverted, below the baseline. Note that only half of the peaks in each multiplet are inverted. Which half are inverted depends upon the sign of the "coupling constant," J, which governs peak separation within the multiplet. The coupling constant measures how strongly nearby nuclei interact with each other. In the product spectrum, the upfield half of the CH_3 and CH_2 multiplets were inverted, showing J had a positive sign; this agreed with theory and with less direct experimental evidence. In the spectrum of the catalytic intermediate, however, the downfield half of the multiplet was upside down, indicating J had a negative sign. Quantum theory had not predicted J's sign in this case, so the discovery came as quite a surprise.



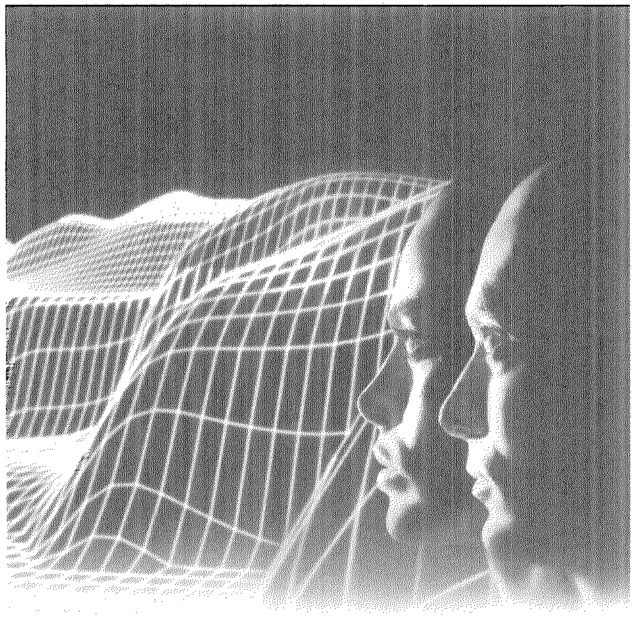
NMR spectra of catalytic conversion of acrylonitrile to propionitrile. Peaks clustered around 6 ppm are due to acrylonitrile. (a) Before adding parahydrogen. (b) The PASADENA effect makes trace quantities of propionitrile visible. The middle peak of the CH_3 triplet has been spilt into two equal peaks of opposite sign that cancel each other. (c) 200 seconds after adding parahydrogen. The PASADENA effect has disappeared.



The dashed arrows show how pure parahydrogen adding across an asymmetric double bond can only reach two of the product's four available energy states. The product can make transitions to the other energy states as shown by the solid arrows. Up arrows are absorptions; down arrows, emissions.

Having both positive and negative lines within a multiplet can lead to problems. If the line widths become greater than their splitting distance, they overlap and cancel, reducing the overall signal. Michael Pravica, a senior working under the SURF (Summer Undergraduate Research Fellowship) program this summer, has demonstrated a variation wherein all lines in a multiplet add together constructively. The researchers have christened this variant the ALTADENA effect. It only remains to figure out what the acronym stands for.

Weitekamp summarized his work thus: "It's an interesting project because it combines quantum mechanics, where everybody learns and forgets about ortho- and parahydrogen, with real chemistry — catalysis."□ — *DS*



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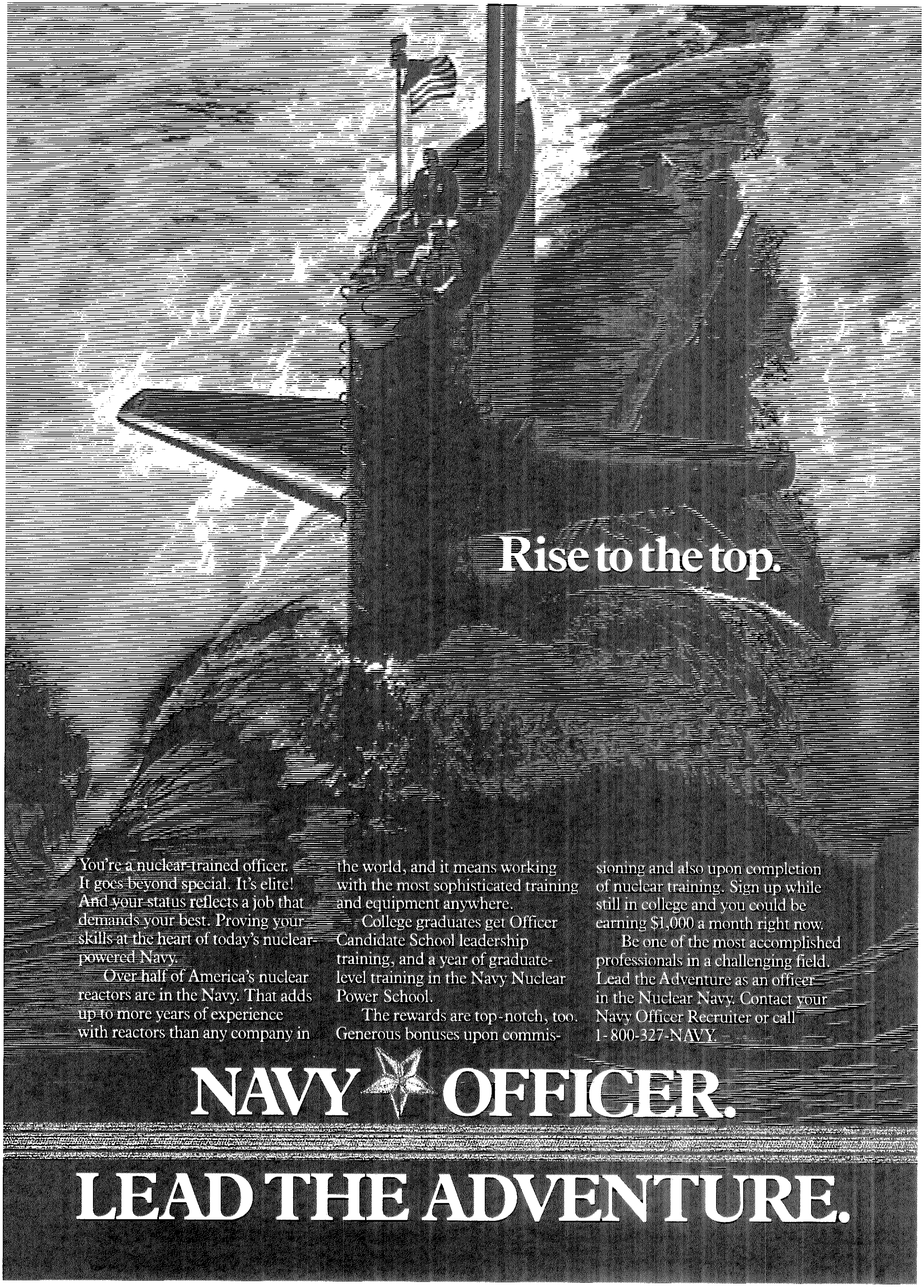
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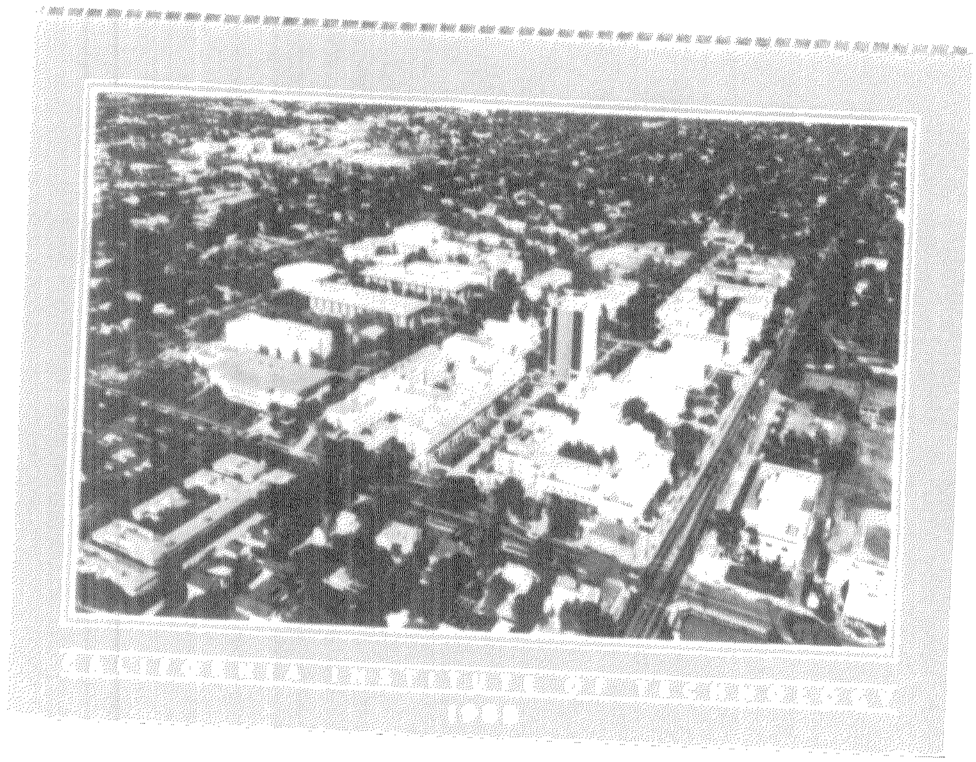

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Lasker Award

LEROY E. HOOD has received the 1987 Albert Lasker Basic Medical Research Award, an honor that in biomedicine ranks in prestige just below the Nobel Prize. Hood, the Ethel Wilson Bowles and Robert Bowles Professor of Biology and chairman of the Division of Biology, shares the honor with two others (from MIT and Harvard). He received the award for his "imaginative studies of the somatic recombination of genes of the immune system, which makes possible an infinite diversity of antibodies." Two other Caltech faculty members have won the Lasker Award — Seymour Benzer in 1971 and Roger Sperry in 1979.

Obituaries

ALAN T. MOFFET, professor of radio astronomy, died August 20 at age 51. Moffet had been associated with Caltech's Owens Valley Radio Observatory from its beginning. He helped elucidate the structure of radio sources, and contributed to the design of radiotelescopes and other instruments.

Moffet received his BA from Wesleyan University in 1957, and his PhD from Caltech in 1961. As a graduate student, he helped build the Owens Valley facility. He joined the faculty in 1966 as an assistant professor, became an associate professor in 1968, and a full professor in 1971. He served as the Observatory's director from 1975 to 1979. He was executive officer for astronomy from 1984 to 1987.

Rodman W. Paul, the Edward S. Harkness Professor of History, Emeritus, died May 15. He was 74. Paul was an authority on the history of the American West and prizewinning author of several books on western mining. In his 40 years at Caltech he built up the history department virtually from scratch and was a strong advocate of improving humanities education at Caltech and of attracting top scholars in the humanities to the Institute. He was also instrumental in developing the Huntington Library's western history collection.

Paul received all his degrees from Harvard (AB 1936, AM 1937, PhD 1943). He came to Caltech in 1947 as associate professor of history, became professor in 1951, and was named to the Harkness chair in 1972. He retired in 1981.

Anthonie Van Harreveld, professor of physiology, emeritus, died August 14 at age 83. Van Harreveld's research was focused on the nervous system, its structure, and its chemical interactions. He had been at Caltech since 1934.

Van Harreveld was a native of Haarlem, the Netherlands. He attended Amsterdam University from 1925 to 1931, earning his BA, MA, PhD, and MD there. He was an assistant there from 1926 to 1932, and chief assistant at Utrecht University, the Netherlands, from 1932 to 1934.

Van Harreveld emigrated to Caltech in 1934 as a research assistant. He served as an instructor from 1935 to 1940. He became an assistant professor in 1940, an associate professor in 1942, and a full professor in 1947. He retired as professor emeritus in 1974.

Astronomy Gift

ASTRONOMY AT CALTECH has received a $1 million gift from Mr. and Mrs. Samuel Oschin. The 48-inch Schmidt telescope at Palomar Observatory will be renamed the Oschin Telescope in their honor.

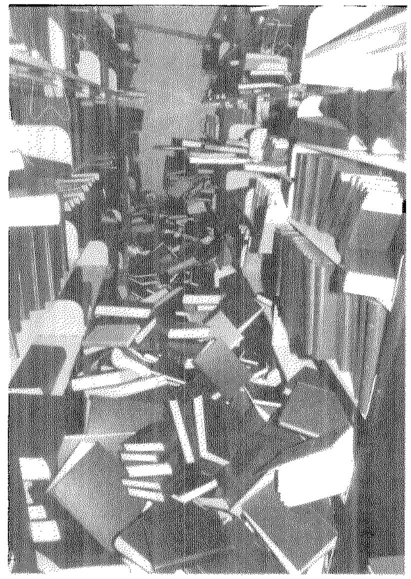
The gift will be used to fund observations with the 200-inch Hale telescope and a 60-inch telescope as well as the Oschin Telescope. All three telescopes are at Palomar Observatory. The Oschin Telescope is currently part of a five-year project to photograph the entire northern sky. The resulting star atlas will serve as the basic astronomical guide for decades to come.

Mr. and Mrs. Oschin are long-term Caltech supporters. Mr. Oschin is an amateur astronomer, and is currently building a 12-inch telescope at his Palm Springs home.

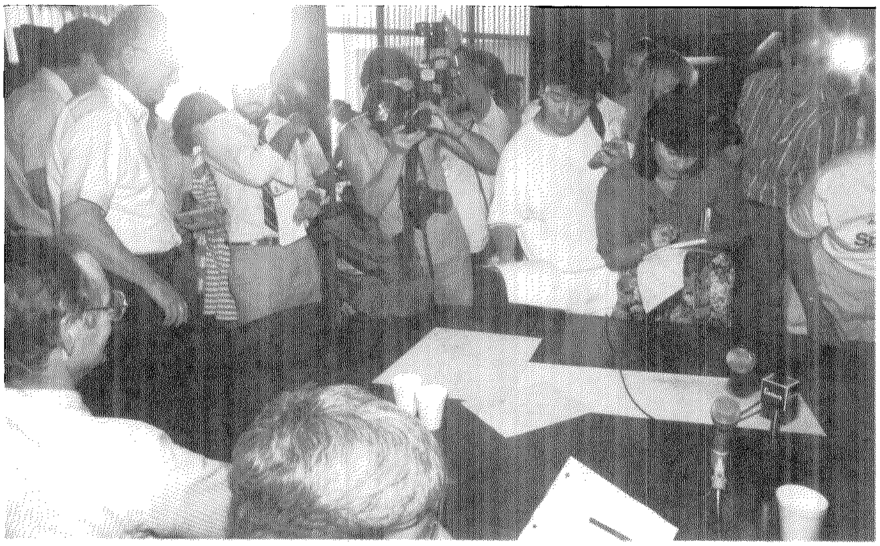
Teaching Awards

THE ASSOCIATED STUDENTS at Caltech (ASCIT) has recognized six faculty members for their teaching excellence. They are Donald Cohen, professor of applied mathematics; Dennis Dougherty, associate professor of chemistry; Valentina Lindholm, lecturer in Russian; Thomas Prince, associate professor of physics; Jean-Paul Revel, the Albert Billings Ruddock Professor of Biology; and Thomas Tombrello, professor of physics. The awards are based on student evaluations of clarity, enthusiasm, command of subject, rapport with class, and interest in the students as individuals.

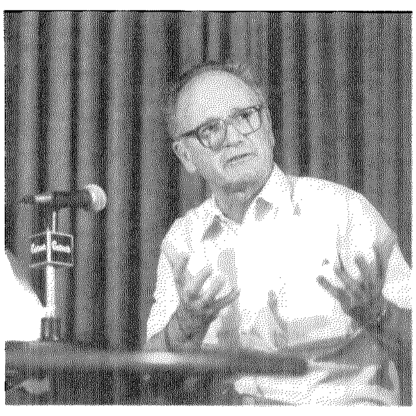
6.1 Earthquake Shakes Up Campus



The early morning quake on October 1, whose epicenter was located just seven miles from campus, tipped over a shelf on the fifth floor of Millikan Library (left) and dumped piles of books off the shelves on the ninth floor.



Damage occurred all over campus, including this crack in a Calder arch, where it joins the Crellin side of the Arnold and Mabel Beckman Laboratory of Chemical Synthesis. This turned out to be minor, but other facilities, such as the 10-foot wind tunnel, didn't fare so well.



By the mid-morning press conference (above), the national news media were besieging Caltech experts for information. The panel included Paul Jennings, division chairman and professor of civil engineering and applied mechanics, and Kate Hutton, staff seismologist (the heads at lower left). Clarence Allen, professor of geology and geophysics (left), describes the Whittier fault during the press conference.

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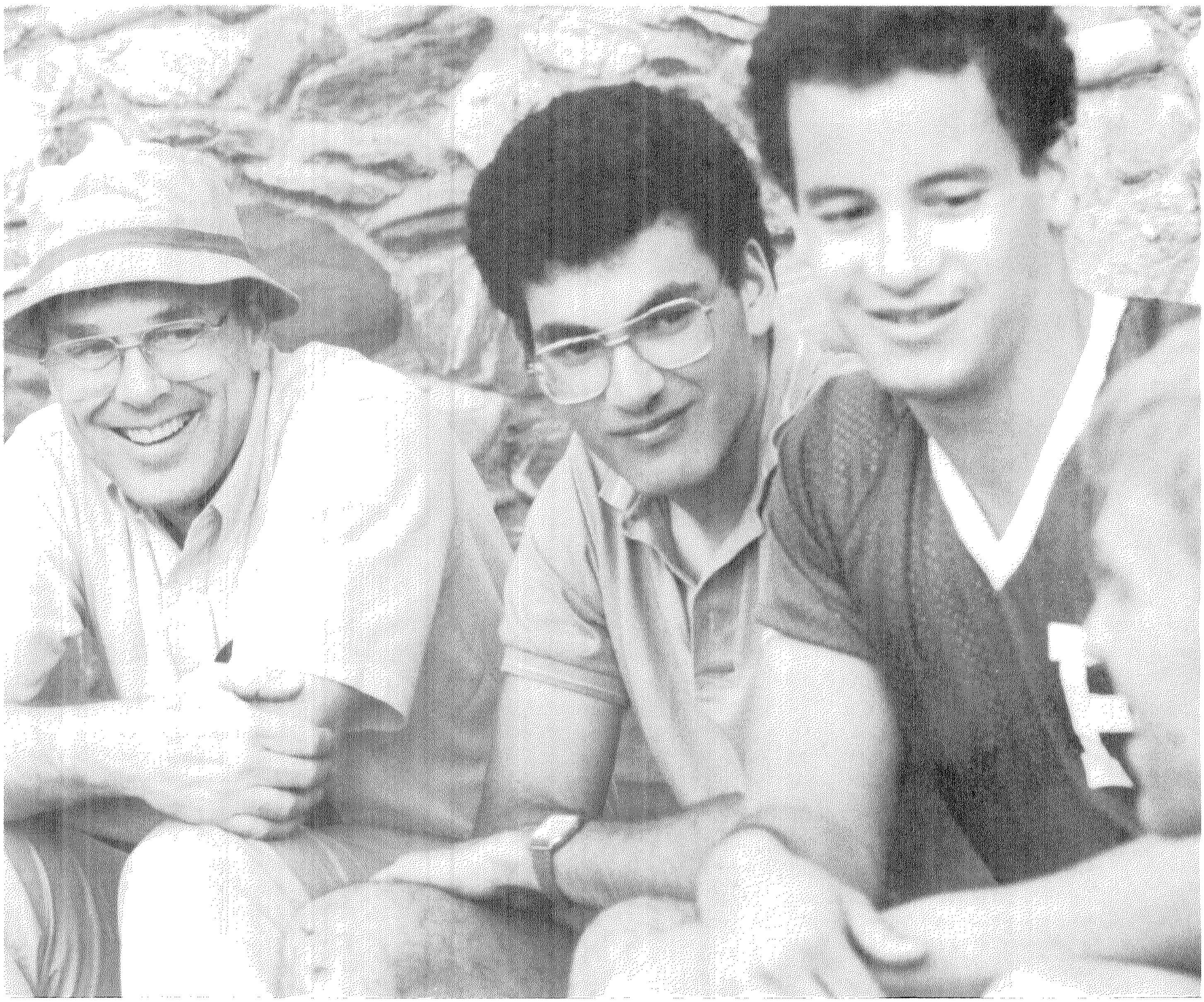

Caltech President Tom Everhart enjoys talking to students at his first Freshman Camp on Catalina Island.