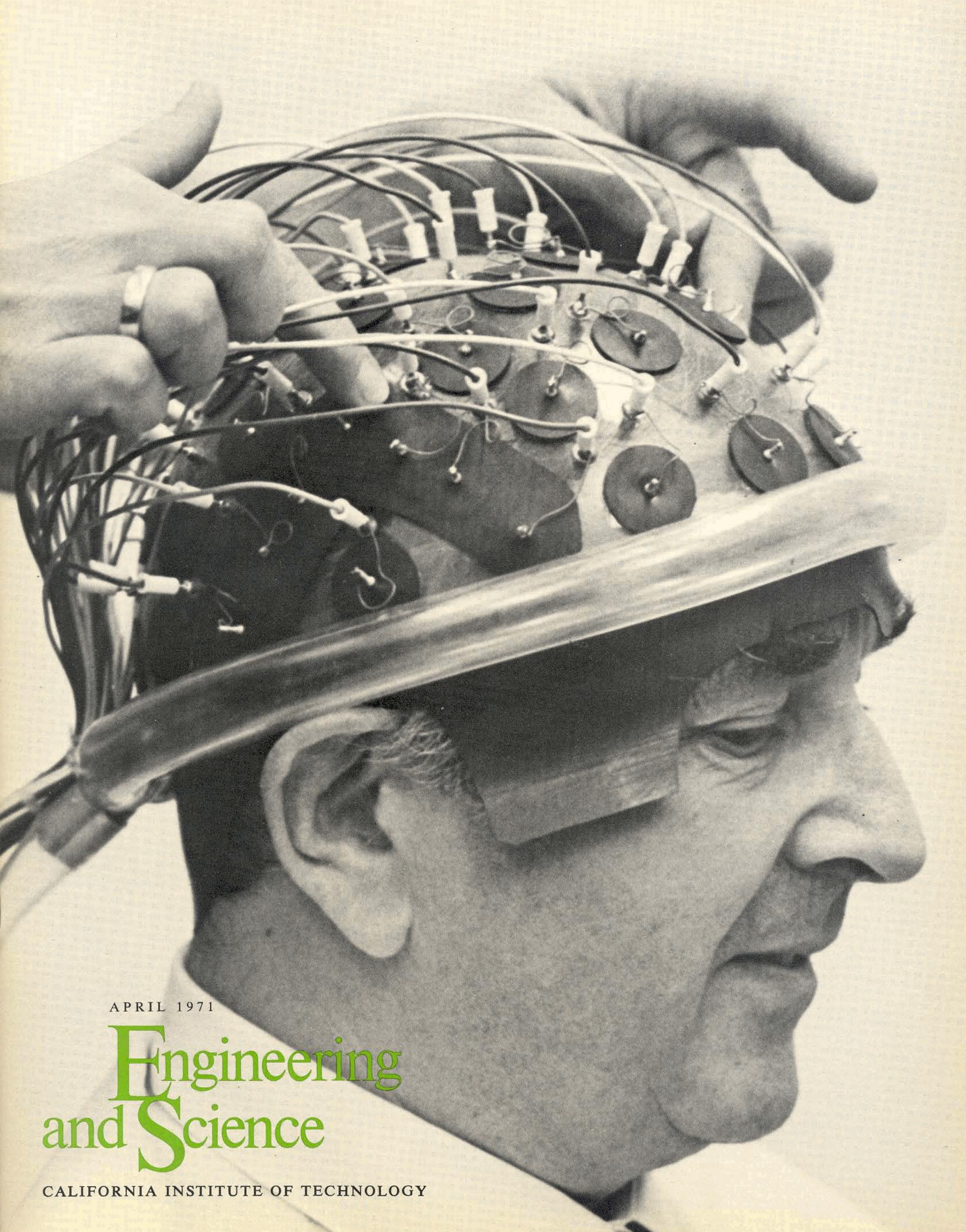
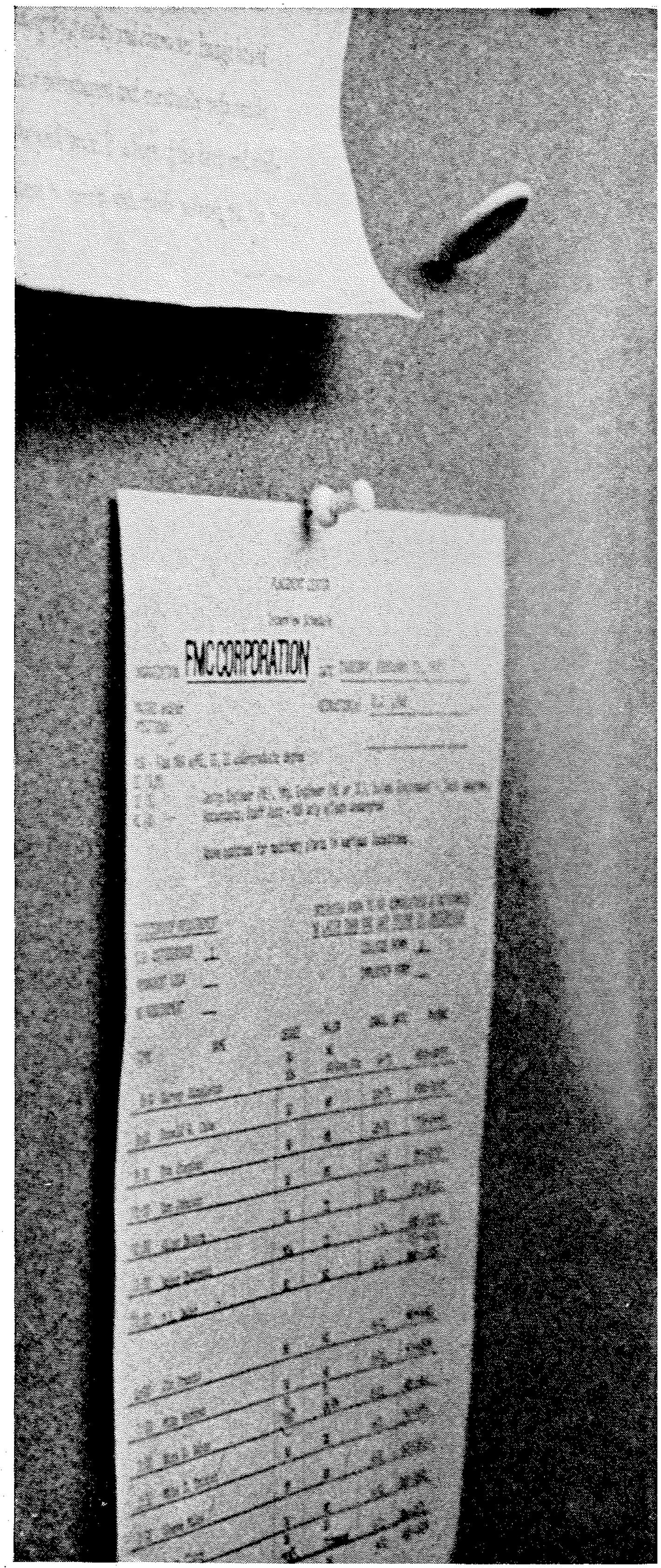


APRIL 1971

Engineering and Science

CALIFORNIA INSTITUTE OF TECHNOLOGY

Don't let our name confuse you.



WHERE IS CHEMISTRY GOING?

Caltech's chemistry division has just concluded a weekly series of seminars on Chemistry and Society. The series was conceived, organized, and supervised by chemistry graduate student Bill Beranek (E&S, February 1971) "to stimulate introspection among graduate students and the faculty in chemistry." A more general goal was to increase communication between the scientific community and the rest of the world.

The nine seminars covered four broad areas: the effect of chemistry on society, the structure of chemistry, the responsibility of the chemist, and the future of chemistry as a discipline. The final meeting of the series—a panel discussion of the future of chemistry—appears here in condensed form.

Moderator of the panel discussion was C. J. Pings, Caltech professor of chemical engineering and chemical physics, executive officer for chemical engineering, vice provost of the Institute, and dean of graduate studies.

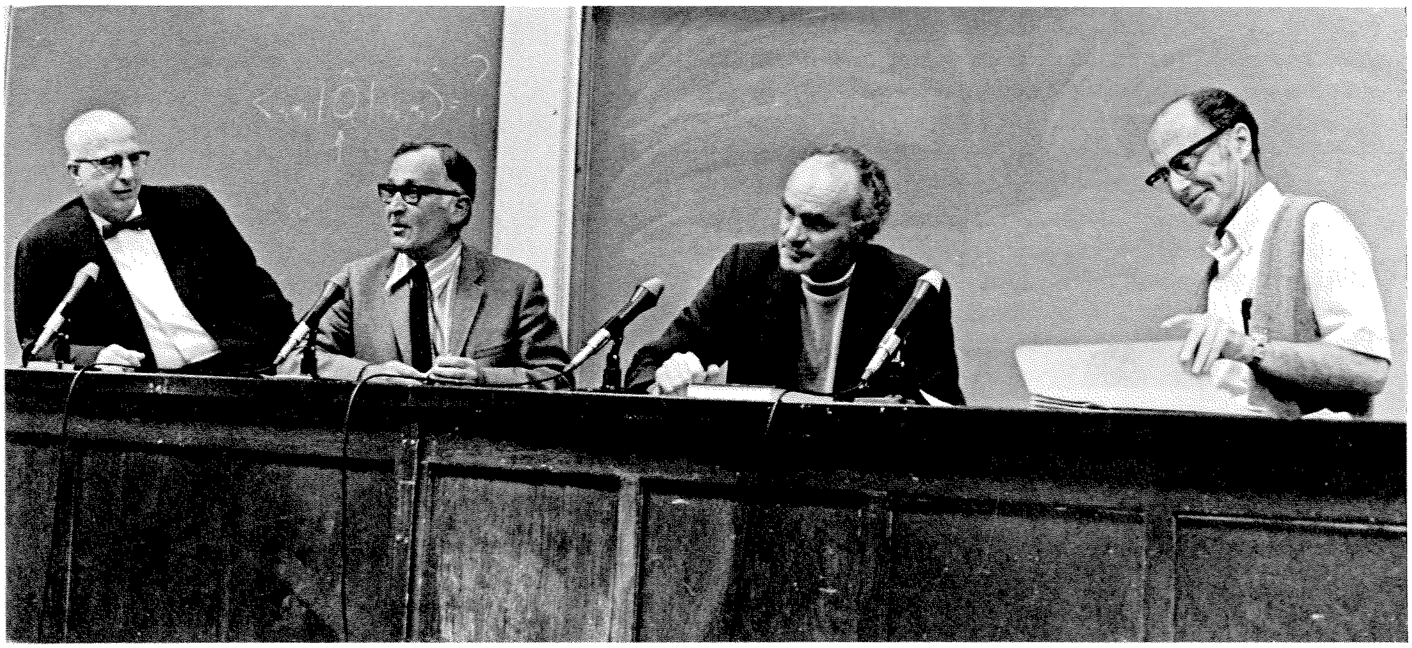
Pings: We are here today to discuss the future of chemistry—if any. We have a panel of eminent speakers —two local experts, and another flown in at great expense from the Midwest. All are well known in their field. All are members of the National Academy of Sciences. They will speak in the following order: George Hammond, Norman Davidson, and Harry Drickamer. Hammond is chairman of the division of chemistry and chemical engineering at the Institute and Arthur Amos Noyes Professor of Chemistry. Davidson is professor of chemistry and executive officer for chemistry. Drickamer is professor of chemical engineering and physical chemistry at the University of Illinois. He is in residence at Caltech this week as the fourth Lacey Lecturer.

Hammond: I'll try to speak briefly (though this is not one of my habits) about what I really think is likely to happen in chemistry in the fairly near future.

My first prediction is that in the forseeable future it's quite possible that the traditional disciplines of science will have been mixed up, redefined, and done differently so that chemistry won't exist explicitly, nor will physics. However, I don't think this is going to happen in a big hurry, and so over the short range of the next couple of decades, chemistry is likely to still stay chemistry.

Other predictions have to do with areas of activity within chemistry or chemical science. First, in research: I think the style of research done in various kinds of chemistry probably will change quite a lot within the next 10 years. I think some switch in orientation will occur; there'll be more emphasis on doing new things, answering new questions, than there will be in solving very much better some of the questions that have been classic for the past 20 years. I even think that it's likely that there will be an increased movement toward obliteration and slow redefinition of the subdisciplines within chemistry.

Second area, chemical industry: I think there will be quite a lot of change in the chemical industry. By and large, the missions of industrial chemistry have not changed a whole lot during a period when there's been great development. People keep saying, "What we need is a new nylon," and so they invent nylon over and over again. And I think that this kind of philosophy will slowly either sink into the traditional chemical industry or else we will probably see arising slowly new chemically based industries. If so, then the traditional chemical industries, like Monsanto and DuPont will, in fact, become commodity industries, and the real action—new industrial chemistry—will be the newcomers. I don't know how this will happen, but somehow I believe it will occur. And probably this new industry will place less emphasis on turnover of masses of material, a characteristic of common thinking in the industry in the recent past. Questions such as "How much of a thing can you produce?" "How many pounds of it can you sell?" will be asked less often, simply because this would be consistent with other societal trends. The society is beginning to worry about masses of stuff that we pass through our hands and process.

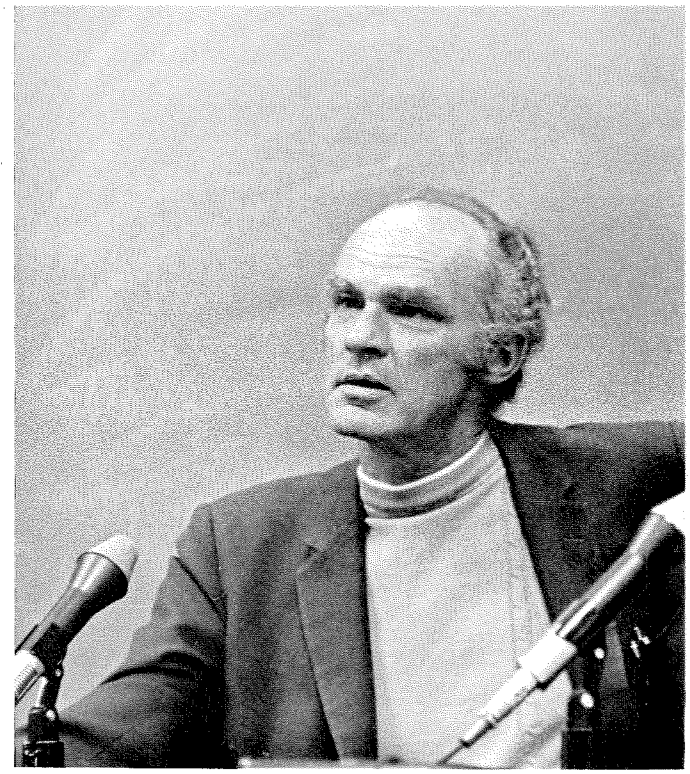


The panel: Drickamer *Pings* *Hammond* *Davidson*

Government research is a separate area on which I'd like to comment directly because I see some interesting prospects. I think that, in one form or another, chemical scientific research is likely to become relatively more important in government's own laboratories, simply because the big things—in volume of dollars and also in the numbers of people involved—in the near past, have been in militarily related research, in aerospace industry, and to some extent in development of new electronics industry to support the military and aerospace. These things are going to be decreased. I think that it's almost inevitable that the government itself is going to stay in scientific research and that, as one kind of thing is turned down, another is likely to be turned up. I don't think it's all going to go into health-related science, because that's already fairly large. So I believe that among the likely candidates will be some kind of chemically related science. It's likely that this will start out sounding as though it's all environmental and turn into other things as people branch out from that.

The fourth area is education. I think there's going to be tremendous change in chemical education during the next 10 years, because chemical education is a part of a very large system—general education—which is undergoing enormous changes. There will be less emphasis on the goodness of great numbers of students, in chemistry and elsewhere, because as a nation we face the fact that we've probably overproduced intellectual snobs. Thirty years ago that type had a unique and valuable role, but now we have more of them than are needed at the moment, and this is something that the nation as a whole will struggle with. There will be many experiments with new styles in education. For example, we may even discover that the lecture, which is, of course, a heritage from the time before there were books, is not necessarily the greatest way on earth to communicate. Chemical education will surely be a part of the changes.

I have one last thing to say which is *not* a prediction, but simply a hope. I wish I could predict that the style of chemical science would become a lot more realistic in self-appraisal. What we need is a good deal less sacredness in our view of ourselves and how we want people to view us. We need a good deal less feeling of total responsibility for everything that happens in the world.



There will be many experiments with new styles in education . . .
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GEORGE HAMMOND

Davidson: I started to write out this 10-minute speech (I never do it for a 40-minute lecture), and it sounded terrible. One paragraph was platitudinous, and the next one struck me as likely to get me lynched. So instead, I've written out a series of more or less disconnected statements. I'm still likely to emit platitudes or say things that are going to get me lynched, but you're not going to criticize me for not having an organized, systematic presentation. It's not *supposed* to be organized and systematic.

My first topic is the economic future of chemistry. The economic future of chemistry is very bleak right now. Essentially, the chemical industry isn't hiring anybody, and the signals I pick up are that the probable rate of hiring in the chemical industries over the next decade or something like that is going to be less than half—perhaps a half to a quarter—of what it has been in the great years. Government support: The latest signals are that it's going to be up some from the recent bleak years, and just how that affects the over-all picture I don't know. Teaching: Teaching is bleak.

The main point of this table from the Cartter report (page 7) is that the demand for new faculty with PhD's is going to remain essentially constant through 1980, and then because of population trends or something, the demand becomes negative. Now, according to George Hammond, half of the teachers are going to be female, and the demand for male teachers is accordingly cut in half. I suppose the

Estimates by Allan M. Cartter of New Faculty Required to Maintain Quality, and New Doctorates Available: Actual and Projected 1965-1985

Chemistry	New Faculty with PhD Needed	New PhD's
1965	505	1,439
1970	492	2,030
1975	578	2,290
1980	475	2,888
1985	–37	Not Available

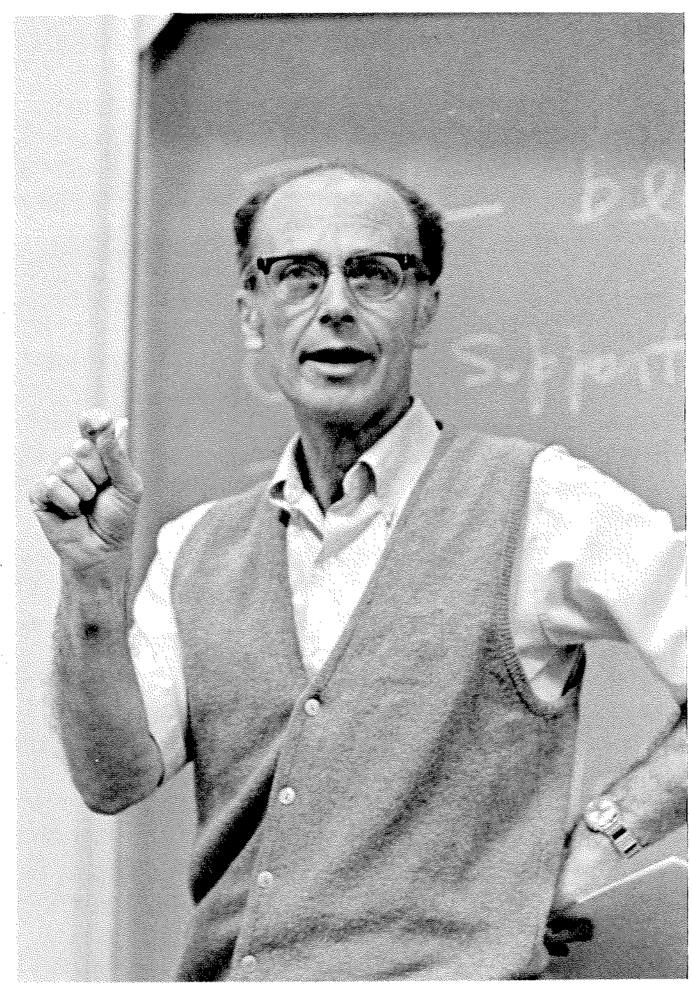
prediction of the constancy in the demand for faculty for the next 10 years can be regarded as modestly encouraging. I am uncertain about the future of biomedical research; there's serious talk of an additional $100 million a year for cancer and for other things. If that happens, it's likely to have an impact and make the opportunities in chemical biology somewhat better than the opportunities in straight chemistry, but I don't know how much. So if there's going to be any substantial expansion in opportunities for chemistry, it has to be new outlets.

My next statements are about the intellectual future of chemistry. The prospects are medium, neither terribly bright nor terribly dull. Chemistry is, in my view, a mature science. Molecular biologists have a tendency to think it is basically dead. Gunther Stent in his "Golden Age of Chemistry" lecture here a few years ago said that organic chemistry is dead, but my friend Jack Roberts says that there's more new stuff happening now than happened 10 years ago. I think he was thinking of things like the whole development or understanding of the mechanism of electro-cyclic reactions—his understanding, not mine, you understand—the synthesis of novel molecular structures usually strained or otherwise unstable, the development of the chemistry of the interaction of π-electron systems with transition metals, the developments in nuclear magnetic resonance, chromatography, X-ray structure analysis, and lots of other wonderful things. Well, these are all great advances, but they don't strike me as conceptual revolutions. Supposing I'd retired from the field in 1940 when I got a PhD; how hard would it be for me now to assimilate these ideas? I think most of them were things we talked and thought about in a primitive way then.

I'd like to say explicitly that in my opinion the purpose of modern science is to be useful. At one time science was a great intellectual liberating force; it liberated us from superstition in the guise of religion, it enlarged our vision of the nature of the physical world and of man, and then—in the cases of quantum mechanics and relativity—it enlarged our understanding of how we interpret the world. But I think that's practically over.

Chemistry is too myopic, too parochial, and too stereotyped. We all have too much of a tendency to do the same thing we learned to do in grad school.

NORMAN DAVIDSON

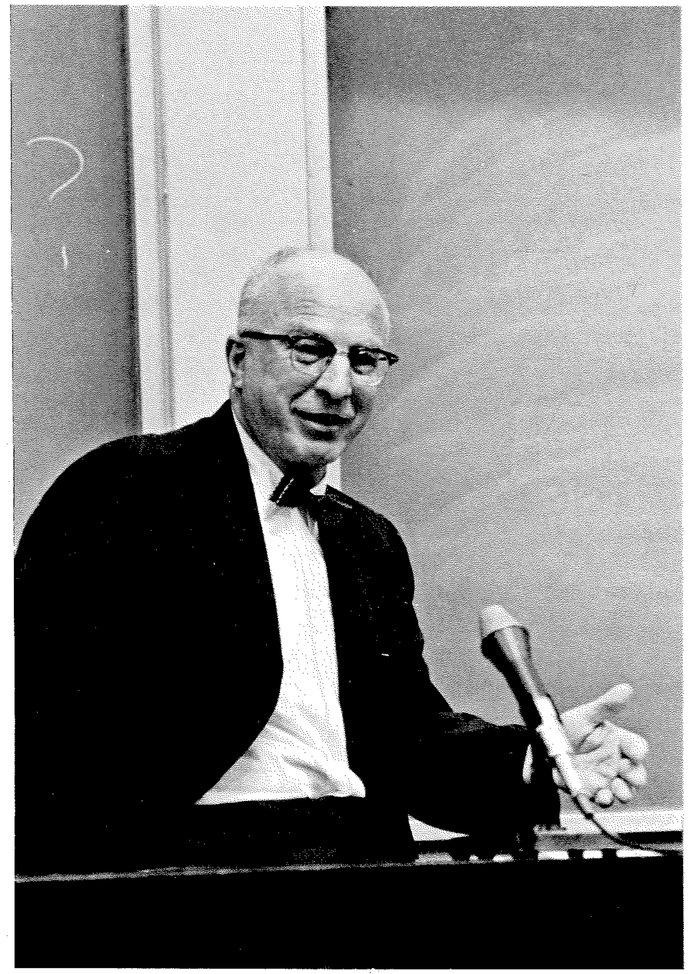


The only part of modern science that can lay a similiar claim now is behavioral biology or psychology. We don't understand man as a thinking, feeling being. The rest of natural science will lead to no conceptual revolutions. And a social justification, as distinct from the motivation of the individual scientist, will have to be related to its usefulness.

In this context basic research is clearly important, because it is basic. It gives us a base from which to attack and solve a number of problems. There is going to be both intellectual and financial pressure to select those areas of basic research that are likely to be relevant. The practical problems we face concern complex systems. My own feeling is that we're going to decide that the useful kind of basic research is to study models just one level of complexity down. Research on very simple systems is going to have a hard time justifying itself because the usefulness of such research for predicting the behavior of complex systems is limited.

I have a special note here to make the prediction that analytical chemistry is going to have a bright future. Even though analytical chemistry as such tends to be in disrepute among the more intellectual members of our profession, there is a good case for the proposition that a major fraction of progress in chemistry is due to progress in analysis. When I first started working in gas-phase chemical kinetics around 1946, it was really a speculative morass. People put things in tubes and they heated them up and watched the pressure change. Then they elaborated mechanisms, but the mechanisms involved reactions which were not in fact occurring. And gas-phase chemical kinetics didn't take off until it was possible to analyze reaction products by gas chromatography and mass spectrophotometry. Similar things have happened in chemical biology. The ability to sequence nucleic acids and proteins has increased the power of our research a great deal.

Under the title "The Trouble with Chemistry" I'd like to express some of my beliefs—and you can call them prejudices—about what's wrong with chemistry at the present: It's too myopic, too parochial, and too stereotyped. We all have too much of a tendency to do the same thing we learned to do in grad school. When I visit other departments of chemistry, I find them all talking about trying to build chemistry departments just like Caltech, Stanford, or Berkeley. Why shouldn't a department strive to excel in medicinal chemistry or in the chemistry of the solid state, or in polymers, and try to do it in such a way that students are educated in



So, I think if we are going to try to encourage young people to do this innovative work, it behooves us who are somewhat established to move out and be exploratory—not in deference to the young people but for our own self-respect.

HARRY DRICKAMER

understanding chemical bonding, non-bonding interactions, chemical dynamics? I think that if we did that we'd have a broader and more diversified and more interesting profession. But other schools hire guys who get their PhD's at Stanford, Caltech, and Berkeley, and unless they are willing to be adventuresome, I don't see much diversification ahead. I think chemistry has a reasonably bright future, but the more innovative and adventuresome we are, the brighter.

Drickamer: I'm somewhat appalled. I felt that surely by now one of these fellows would have gotten lynched and broken up the meeting, so I wasn't really prepared.
Davidson: We reserve lynching for guests.
Drickamer: I'm going to start from the position that a fair number of people feel that there are a fair number of problems within the present situation in chemistry. After all, when things are really booming, one doesn't hold meetings on "Whither Chemistry." At that point you're so busy turning out new ideas and new results that you don't have time for such meetings.

As I see the situation, back in the late 1940's there was considerable dissatisfaction among chemical engineers as well as physical and organic chemists vis-à-vis the relative excitement of nuclear physics in the late thirties and solid state physics in the late forties. Physical chemists felt that their approach to problems was both unsophisticated and sterile in the sense that thermodynamics had been fairly well milked and macroscopic measurements of kinetics weren't getting any further. There was the feeling among both physical and organic chemists that the semi-routine sort of synthesis—which we referred to as sticking another ethyl group in the beta position—didn't have the kind of sophistication and fertility that physics had. I think we did something about it: In physical chemistry we introduced the ideas of quantum mechanics and group theory from the theoretical standpoint and the instrumentation of physics in spectroscopy. In chemical engineering, we introduced primarily sophisticated applied mathematics, and the experimental techniques of fluid mechanics to study transport and moving systems rather than just making thermodynamic measurements. In organic chemistry, we introduced both the instrumentation of physical chemistry and physics and also the concepts of chemical dynamics. And the outcome of all this was very fruitful; we changed both the form and the substance of our approach. There was a burst of significant output in all these areas.

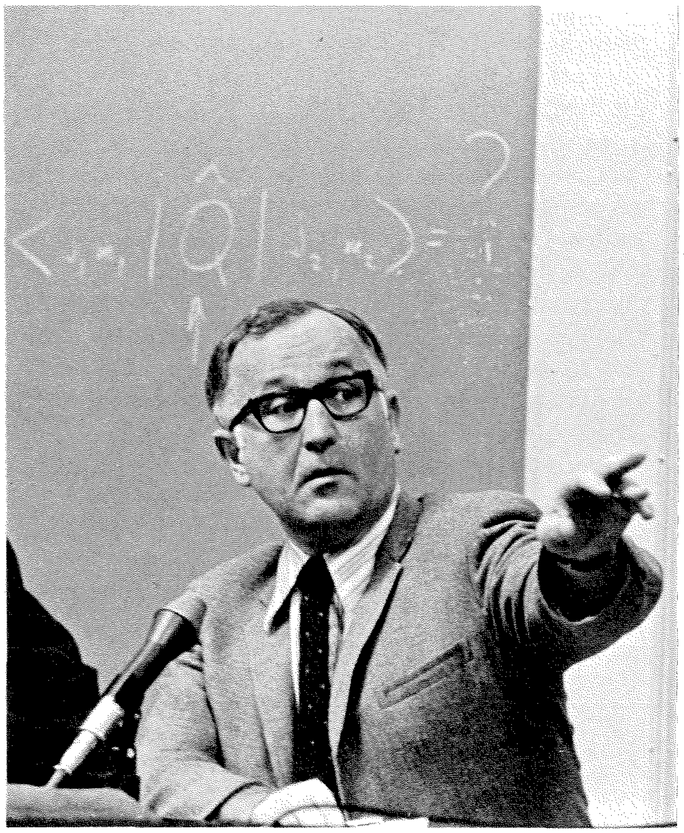
But in all human endeavors there's a basic conservatism in which one tends to preserve the form even when some of the substance is gone, and this applies to solid state physics as well as to legal systems, to social systems, and to everything else. If one has had a successful form and it has produced some real substance, we are reluctant to give up the form. We have tended to feel that it's more important to be elegant and sophisticated than it is to be fruitful; i.e., we have been using these elegant methods to study relatively simple systems: in engineering, relatively simple models for flowing systems; in physical chemistry, relatively simple molecules; in solid state physics, the alkali halides and silicon and germanium and things of that sort. Of course, we learned a great deal about these simple systems. But we tend to refine our measurements and refine our calculations without any real hope that a new generalization can ever arise from these studies.

I think what we need to do is make some kind of break into new areas where we may use sophisticated tools, but our approach may be relatively unsophisticated. People have to be willing to do a little more exploratory work, to open up to new fields. Even though you may use sophisticated techniques, the treatment may have to be relatively unsophisticated because it's a really new idea. I think of Mott & Jones, a book on the structure of solids, printed in 1936. I was talking to Mott two or three years ago, and he said, "Of course, you understand that all that's wrong," meaning it was unsophisticated. Still, this was probably the most seminal book ever written on an area of this kind, because it contained a lot of ideas that could be tested and refined. I think we will have to be interested and excited about partial solutions in large problems rather than complete solutions for very small problems. And I think we'll have to worry about interacting with other fields even though it's not possible to do it in a very sophisticated way.

I think that relevance is a very dangerous term. I come from an engineering education, and the engineering education of 30 years ago was relevant; we studied know-how which was obsolete before we got out of school. It had to be unless the field was dead. But I think there is no harm in studying specific systems of real use, where you can do something interesting and exciting. I can recall a time about 20 years ago when a man in a certain branch of chemistry told me that the thing that made him proud to be in that branch of chemistry was that there was no possible way of applying it. That kind of attitude was nonsense then, and it's nonsense today.

There are problems in breaking into a new field, there are problems in being exploratory. This isn't an easy thing to do, and to ask young people to stick their necks out is asking a lot. There are practical difficulties like getting support and interesting graduate students, and there are more important psychological difficulties. Man is a social animal, and it's a kind of comforting thing to go to a meeting and find a half a dozen other people doing very nearly the same thing you do. You can talk, and it's exciting. There's competition, but there's also companionship. And when you're doing things that aren't quite like what other people are doing, they say, "Well, gee, that's fine stuff. By the way, did you hear about what I'm doing?" So I think if we are going to try to encourage young people to do this innovative work, it behooves us who are somewhat established to move out and be exploratory—not in deference to the young people but for our own self-respect. Perhaps people our age ought to start whatever revolution we're going to start.

Pings: I promised the panelists that they could have a crack at each other, so I'll see if they have any pent-up feelings that they wish to vent right now. George, do you have anything to say in reply to these other two presentations?
Hammond: Yes. I've been sitting here in the middle and realizing that I really am in the middle. Because when I talk about where chemistry is going, I talk about outlets into other fields, and the two fields I always pick are biological chemistry, which is well established, and engineering science, which is becoming established. And on my left I have a man who clearly has flowed through the breach in the wall and is over there in biological chemistry. And on my right, there's Harry, who a long time ago discovered engineering science. And I'm the poor cat who's stuck back in the middle, which is not the sort of image I like to have of myself. I do not think that the middle is totally dead. The fact of the matter is that we guys in the middle probably do one hell of a good job—as good a job as anybody really cares about or is going to learn a lot from—in calculating the ionization energy of benzene. And that is probably as dead as Norman and Harry make it sound. But I don't think either of these guys can do such a good job with the boiling point of benzene. And that's neither engineering nor biochemistry, it's chemistry.
Davidson: I was going to ask what the boiling point of benzene is.
Hammond: 84°?
Davidson: 80? 78?
Hammond: Well, *I* haven't boiled it recently, and maybe it's changed in the meantime.
Drickamer: As an organic chemist, I think there was probably something else in your benzene.
Davidson: I have another question, but I think I'd rather participate in this discussion. I suspect that there are some problems, like the statistical mechanics of liquids, that are just not going to be solved theoretically. I think we're going to end up taking the attitude that we can measure the boiling point of benzene with a flask and a thermometer, and calculating it is not going to be popular,
Hammond: There was a time when anyone would have said that about the ionization potential.
Davidson: OK, but I said that right now about the boiling point of benzene. I may be wrong.
Pings: Yes, but you certainly hope that if you did do it with a flask and a thermometer, that then when you stuck the ethyl group in the beta position, you wouldn't have to repeat it for the new substance.
Hammond: In fact, Wilse Robinson [Caltech professor of physical chemistry] may have the boiling point of benzene while you're still around to admire it. I mean benzene isn't argon, but hell . . .
Davidson: Well, unless after Wilse gets done with benzene, he really can do ethyl benzene . . .
Robinson [from the audience]: Why are you picking on me, Norman?
Davidson: Hammond picked on you; I didn't.
Hammond: I didn't pick on him; I pinned my faith on him.
Davidson: I'm seriously trying to raise the question of whether it is really valuable to calculate the properties of certain simple systems when we know in a qualitative sense what's involved in the theory, and it's dubious whether the quantitative success of the theory will ever be sufficient to enable us to figure out things that we couldn't just as easily measure.
Hammond: In my opinion, yes. I think the point is not to know the boiling point of benzene—because even if I forget it, I can look it up—but because the second or third generation development coming out of this will be enormously fruitful inspiration for understanding the properties and behavior of amorphous systems in general.
Davidson: That kind of theory I'm in favor of.
Pings: Good. We agree on something.
Davidson: Listen, I think we agree on a lot. I think we are really saying very similar things.
Pings: Yeah. As a matter of fact I'm beginning to despair right now because we're going to lag. Harry, what do you have to say?
Drickamer: Well, I think it's very easy to sit up here and



We agree on something—
that the point is
not to know the boiling
point of benzene.

C. J. PINGS

lecture people on where we ought to go, but I've been trying to remember why it is that people stick so much to the form. I guess it's simply because they know they can accomplish something. They know they can do a reasonable job with a student in a reasonable time. The real question is how practical is this exploratory work as a means of getting people degrees. That's a very difficult problem.

And that leads me to another point. I think that one of the problems we face in universities is some decoupling between the natural desire of the faculty to do research and the available jobs for graduate sudents. The number of students in graduate school really depends on only two factors: the amount of money you've got to support them and the number of people that the faculty want working with them. It's in no sense even remotely correlated with the possibilities of them getting jobs in the future. We haven't had to worry about that, but I just cannot see a vastly expanding job market taking care of the number of students we're going to turn out. One possible solution is to introduce a much larger number of technicians into the university. A typical faculty man might have, instead of six graduate students, only three graduate students and a technician. He would still get a certain amount of research done for about the same amount of money, and I figure he's not likely to get any more money. In the best of all possible worlds, faculty would have their graduate students to work with, but in this second-best of all possible worlds, perhaps two faculty could share a technician and still accomplish their research and retain their teaching function.

I think there are disadvantages to this system, but there are disadvantages to any system in the real world. This is a smaller disadvantage than the vast expansion of the offering of PhD's in the last 20 years. My impression of the number of schools giving PhD's in chemistry is that it's doubled in the last 20 years; I know it's tripled in chemical engineering; and it's about doubled in physics. And really there's been no demand for this on the basis of people pushing schools to give more PhD's.

What has happened is that we have faculty educated to believe that research is a way of life, and they want bodies to work with. There are far too many places offering the PhD, and a man can shop around at almost any intellectual level and find a place where he can get it. This may be a bookkeeping detail compared with the big picture we've been talking about, but it would be a big detail in the life of students if they were encouraged to go on only if they have a particular vocation for it—not because they would be available to do research for those of us who are already established.

Watching the Brain at Work

Caltech's Derek Fender is trying to find out what goes on in the brain when it's thinking, and what patterns nerve impulses follow when they are activated by light.

What goes on in the brain when it's thinking? What patterns, if any, do the nerve impulses follow when they are activated by a simple stimulus such as a flash of light?

In short, how does the brain work?

The problems in answering questions like these seem at first to be nearly insurmountable. For example, the inherent fragility and complexity of the brain itself, as well as the electronic speed of its activities, defy investigation. Even if direct observation were possible, the observer wouldn't be able to see anything, since the brain's activity occurs through countless electrochemical circuits at electrical potentials on the order of millionths of a volt.

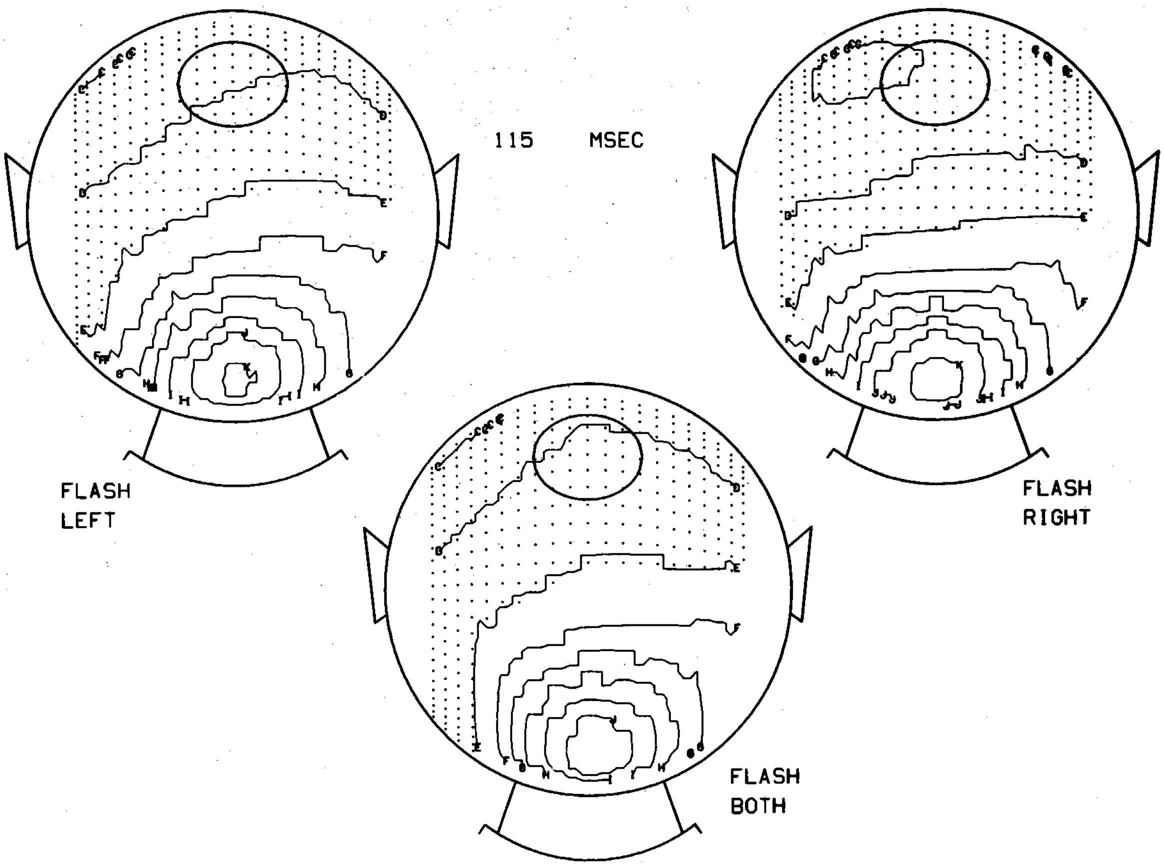
Nevertheless, Derek Fender, professor of biology and applied science, and his graduate assistant Robert Kavanagh, have found some preliminary answers to the question of how the brain works. Through an apparatus they have designed and assembled in the Booth Computing Center, together with some computer software painstakingly developed over the past 24 months, they have reached the threshold of being able to visually follow the interactions among the parts of the brain as it performs some low-level perceptual and cognitive processes.

The technique with which Fender "sees" what happens in the brain involves using a helmet bristling with electrodes that are linked up to an IBM 360-75 computer. Looking like an exotic hair dryer, the helmet is custom-made for each subject, air-conditioned, and vacuum-fitted to the head so that each electrode makes a good contact with the scalp. The brain waves are picked up, recorded on digital tape, and transmitted to the computer. The computer in turn is programmed to translate the digital signals into a visual pattern on a cathode ray tube—somewhat like a television tube. The result is a picture of the brain waves—a contour map of the peaks and troughs of electrical activity as "seen" through the top of the subject's head.

Each picture on the tube is photographed and ends up as a frame in a movie, which is then studied to see how the brain waves emanate from the various regions of the brain. Fender and Kavanagh have made two such movies, each a little over a minute long, representing the brain wave activity in a quarter of a second—but slowed down 250 times.

Fender and Kavanagh have studied the brain wave patterns of 27 people. One of the things their investigation has already shown them is that perception of a simultaneous light flash and clicking noise will stimulate activity in three distinct locations of the brain. One area analyzes visual images. The second analyzes sound signals. Fender thinks the third area is the one that tries to decide whether the flash and the noise come from the same place.

People have been studying brain waves for 40 years, but there have been numerous obstacles to overcome. Investigators in the past have usually affixed only a few



These three frames from a computer-generated movie illustrate the brain wave pattern that follows a flash of light to each eye separately (left and right) or to both eyes simultaneously (center). The potential field on the surface of the head at 115 milliseconds after a light flash is displayed as contour lines. Dotted areas show negative potential, and the small ellipse on the midline denotes the vertex of the head.

electrodes to the skull, and then have tried to deduce what was going on in the brain from what those few electrodes told them. In fact, though, at least nine electrodes, strategically placed, are needed just to locate a single brain wave source—a point where a cluster of brain cells has fired in collaboration.

Pinpointing the locations of two sources, whose waves may be intermingled by the time they radiate outward to the surface of the scalp, requires a minimum of 30 electrodes in any practical scheme. At present, Fender's system employs a helmet with 49 electrodes.

So, one of the chief features about Fender's helmet is that it gathers a sufficient amount of data. But probably the crucial aspect of the system, and the characteristic which distinguishes it from all other techniques for evaluating electroencephalographic (EEG) data, is the logic reflected in the computer software itself.

When a brain wave signal reaches the surface, its strength as measured by any given electrode will vary depending on how close the source was to the electrode, and what direction the source fired in. Sometimes the strength of a signal makes it appear as though the source were located directly beneath an electrode, when in fact it could be the combined signal resulting from two sources firing from points farther away—but pointed at that electrode.

The only way to tell such cases apart is to do the careful electrical engineering calculations that, in effect, plot the signals picked up from several electrodes on a graph, and then use the resulting curve to deduce the location of the brain wave source. That's where the software comes in.

The software contains the logic—reflected in the form of algorithms—for thousands of such curves, each curve representing the pattern of electrical values that would be picked up by the electrodes if a specific brain wave source should happen to fire.

In a way, the software produces a kind of catalog of electrical curves, each one representing a series of differential equations—and all of them solved by Kavanagh. That's why it took him two years to write it.

In sharp contrast with this technique is what Fender calls the "classical" EEG method: First of all it is based on signals picked up from a limited number of electrodes. Then, looking at a strip-chart recording of the signals, the EEG specialist deduces the location of a given signal source. The potential for serious errors using this "eyeball" method is obvious. (It's no accident that a brain surgeon normally removes a segment of skull bone many times larger than the brain area he plans to work on. He needs a considerable margin of safety.)

And even if the EEG specialist could somehow plot the curves for each pattern of signals picked up by his electrodes, he still could not recognize the subtle—but crucial—differences among patterns with the accuracy or rapidity of a computer.

Fortunately for this experiment, most of man's thinking is done at very shallow depths of the brain—in the cortex at the surface of the hemispheres—rather than in the brain's deeper structures. This makes it easier to locate active neural populations from measurements taken on the scalp.

Another major obstacle has resulted from the low voltage of the brain waves themselves, which range in amplitude from 5 to 100 microvolts (millionths of a volt). Because of the brain's low voltage, the interference problem is severe—not only because of stray voltages constantly surrounding the subject (such as fluorescent lights, line voltages, and radio transmissions), but also because of frequencies produced by other organs and muscles of the body. The heart muscles, for example, produce a twentieth of a volt. Even the muscles of the arteries of the head produce signals. All of this adds up to an enormous data "cleaning" problem.

To deal with this problem, Fender and Kavanagh conduct their tests in a specially built cubicle with copper mesh walls that take care of the stray signals coming from outside. To solve the problem of the interference from the muscles and organs in the subject's own body, special data recording and processing techniques are used, plus Kavanagh's software which instructs the computer to recognize—and ignore—those unwanted signals.

One of the biggest difficulties in brain research is the sheer volume of data that has to be handled, even in very basic functions like recognizing light flashes and buzzers. But Fender's technique, which makes it possible to record and analyze 1.25 million pieces of information in a quarter-second of brain activity caused by a light flash, appears to have overcome this hurdle.

Even so, the computer time required to translate this data into a series of pictures on the cathode ray tube amounts to 44 minutes—and that is a very long time to spend on any task with a third-generation machine like the IBM 360-75, which can do most tasks in a few seconds.

Fender's work, which is supported by the U.S. Public Health Service, will help bridge the enormous gap in understanding between the neurophysiological work which records and explains the activity of a single neuron or of small groups of neurons, and the work on the complex neural control of behavior in humans.

Individual neurons are very "nonlinear" devices. If even a small number, say 20 or so, are connected together in a network, then however much we might know about the individual cells, it is still very difficult to predict how the network will behave. And yet, the human brain operates on populations of cells that number in the millions. It is the statistical function of the population that carries on, so if some of the cells die, the statistics of the populations are not substantially altered. That's why we can be nearly as efficient at age 70 as we are when we are young.

If the function of the human brain is to be understood by building up from the work done on single cells, the investigations clearly have a long way to go. Present knowledge would not allow us to predict even what a 20-cell network would do.

Work on single cells is further complicated because it requires surgical procedures that would not be tolerated on humans and must be done on animals. This means that measurements made on the brains of cats and monkeys must be used to predict how the human brain works, and this represents an added complication.

Brain research needs people working at many levels, including those who work empirically from the single cell up, those who work on the human being and use theoretical and mathematical techniques to work down, and those who work on populations of cells in between.

Fender expects the work of all these groups to join up one day and form a coherent story of how the human brain really works.

Watching a Brain-Watcher Work

What he really wants to find out is how his own brain works.

Derek Fender, professor of biology and applied science, concentrates most of his scientific energies these days on learning how brain waves are propagated. There are plenty of possible applications for his research—learning how to spot brain tumors, diagnose brain injuries, identify sources of epileptic seizures, or how to build a foolproof lie detector, for example.

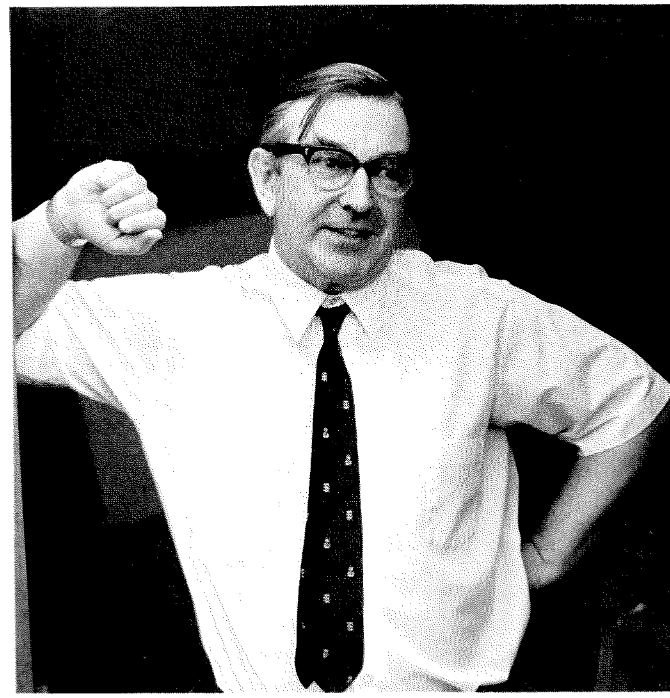
But though he's glad his experiments may benefit others, such practical applications really aren't Fender's main interest. His real reason for studying the brains of monkeys and other animals, including humans, is essentially egotistical. The fact is, he doesn't much care how monkeys' brains work. He isn't even primarily interested in how man's brain in general works. What he really wants to find out is how his own brain works. And he's honest enough to admit it.

He can't point to any single experience that prompted his motivation; rather, he thinks it has grown over his professional lifetime. But accidental or not, the historical process whereby Fender arrived at his brain wave experiments is just as intriguing as the experiments themselves.

On July 15, 1939, Derek Fender was awarded a bachelor of science degree in physical sciences from Reading University in England. Twenty-four hours later, as part of the first draft call in British history, he was in a truck headed for the Royal Berkshire Regiment—the first stop on a six-month tour of duty in His Majesty's Army. Five weeks after that Great Britain entered World War II, and the young physicist's six-month tour stretched into eight years.

A few months after the war broke out, the massive push began at Whitehall—the British equivalent of the Pentagon—to develop radar, and Fender, like practically every other qualified physicist or engineer in the country, was put to work on the project. His main area of concentration was the development of control systems for both the antenna and the antiaircraft guns, and it was here that he first became aware of the human engineering aspects of a man-machine system.

For example, getting the antiaircraft gun to fire at a point where the aircraft would be in, say, 30 seconds (the time it would take the projectile to get there) involved a neat exercise in three-dimensional mathematics —even without taking into account the human element. But in fact it was not enough to know aircraft range, bearing, altitude, and speed, for a major determining factor turned out to be the enemy pilot himself.



Derek Fender, professor of biology and applied science, began his career studying automated systems; then he turned to man-machine systems. Now he does research into the nature of the cognitive processes in the brain of man.

Some flew zigzag patterns, others swooped and dove, others flew yet different patterns. Fender and his crew eventually discovered there were certain flight characteristics that helped to classify a given aircraft according to one flight pattern or another, and these in turn could be used to narrow down the number of probable future target positions.

Another human engineering problem cropped up when it was found that the British gunners couldn't adjust the positions of their guns fast enough to keep up with the "predictor"—an early version of an analog computer, which translated enemy aircraft flight behavior picked up on radar into commands to the gunner. This human time lag in the system meant that the guns could not track with the enemy planes. Fender and his team attacked the problem informally at first, but their effort soon grew into a major undertaking known as the Human Operator Project.

This was Fender's first in-depth experience with human engineering. He and his team dealt with the problem by considering the human operator as a "black box," with certain input and output characteristics just like any other component of a system. This necessarily entailed a lot of research in human perception and response. As a result of the project, enough was eventually learned about operator performance characteristics so that appropriate allowances could be made in the commands issued by the computer.

His wartime experience with the human operator problem in antiaircraft systems made Fender uniquely qualified in the newly discovered field of human engineering, and in 1947 he became a lecturer in the subject at the Royal Military College of Science at Shrivenham—an institution roughly equivalent to military postgraduate schools in the U.S. The British Army, being very much aware by now of the importance of human engineering in an age of mechanized warfare, was the first branch of the services to teach the subject formally, and during the years immediately following the war Fender taught classes of military officers from all over the Commonwealth.

While a lecturer at Shrivenham, Fender took his second BSc (Special)—which is much the same as the American master's degree. But in the highly structured English academic system he still found his horizons severely limited by the lack of a PhD, despite his qualifications in his field. So, in 1953 the Fender family, which now included a wife and two children, moved to Reading, and with the help of R. W. Ditchburn, head of the physics department, Fender got a junior faculty job at Reading University. He managed at the same time to enroll in the PhD program in physics there. Fender reckons that in addition to his doctoral research at the time, he had 12 to 15 hours of classes to teach—plus the donkey work of labs and correcting papers—and it made a full load by any standard.

Despite this horrendous schedule, he had made sanguine plans: In the course of the three-year program, he intended to do one book in the first year, another book in the second year, and his thesis in the third year. As things turned out, everything was written in the final year.

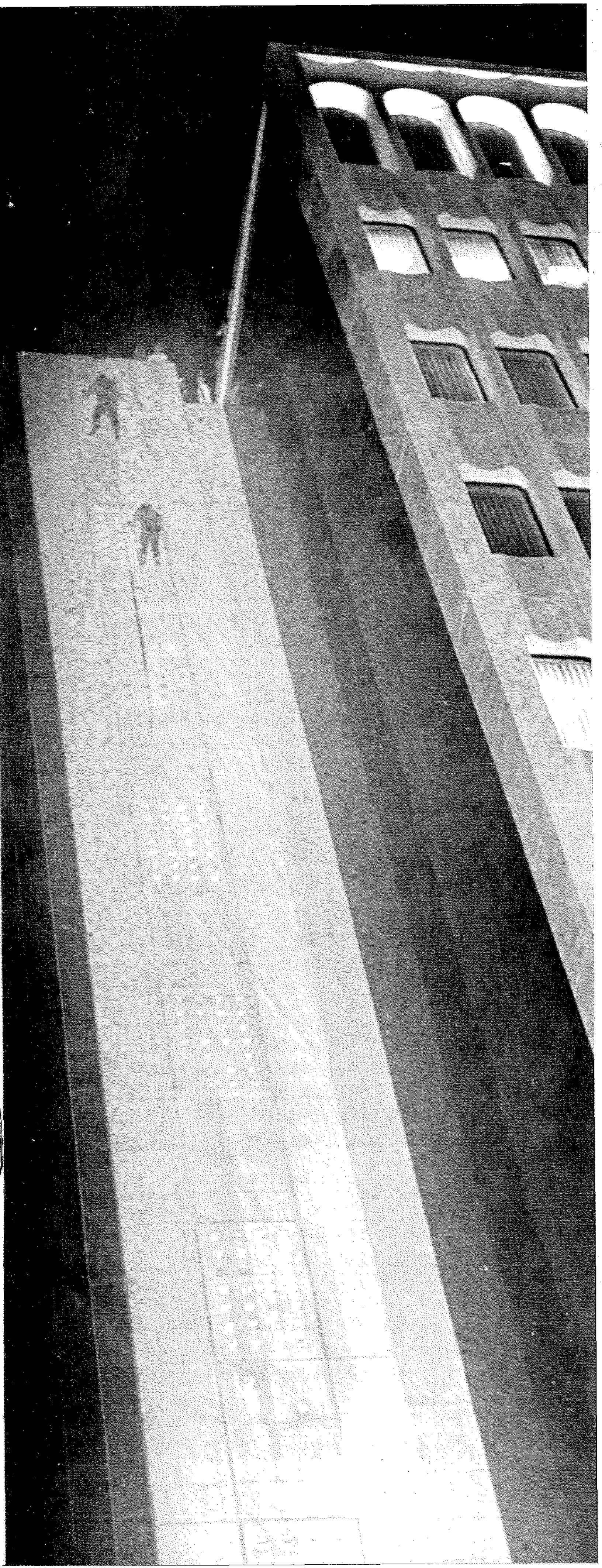
After that life smoothed out considerably. Fender was immediately promoted from "demonstrator" (something like a teaching assistant) to "lecturer grade I"—the equivalent of a full professor in an American university.

Fender came to Caltech first as a senior research fellow in 1961, and the year after that he was appointed professor of biology and applied science. Some of his research since has involved him in a joint project at the Institute of Visual Sciences in San Francisco, where he collaborated with Dietrich Lehmann on problems in clinical electroencephalography. The problems he encountered on this project in the analysis of conventional EEG records motivated him to refine the design of his own 49-electrode helmet.

During this project, Fender and his colleague discovered that the best subjects for their brain wave study were waitresses. College students were too inquisitive; their heads were full of what Fender calls "flat-fast" brain waves, characteristic of problem-solving activity. Other sorts of people (Fender refuses to identify them) simply went to sleep, and all the electroencephalograph could pick up was the long, smooth brain wave characteristic of an idling or sleeping brain.

But waitresses were just right. They were bright, so they didn't fall asleep. They were industrious enough to concentrate on the light-flash stimulus they were being paid to watch. And they weren't too nosy about what was going on or too preoccupied with some other problem, so they didn't show much flat-fast brain wave activity.

Fender doesn't foresee any fundamental change in the over-all direction of his research, though he does note a tendency on his own part to be less involved with the details of every research project that he advises or manages. But anybody who thinks Derek Fender is getting away from research should watch him do his experiments. One of those futuristic-looking electrode helmets used to gather brain wave data fits nobody else's head but his, and it's the most frequently used helmet of all.



Climb Every Mountain–or anything else that's handy

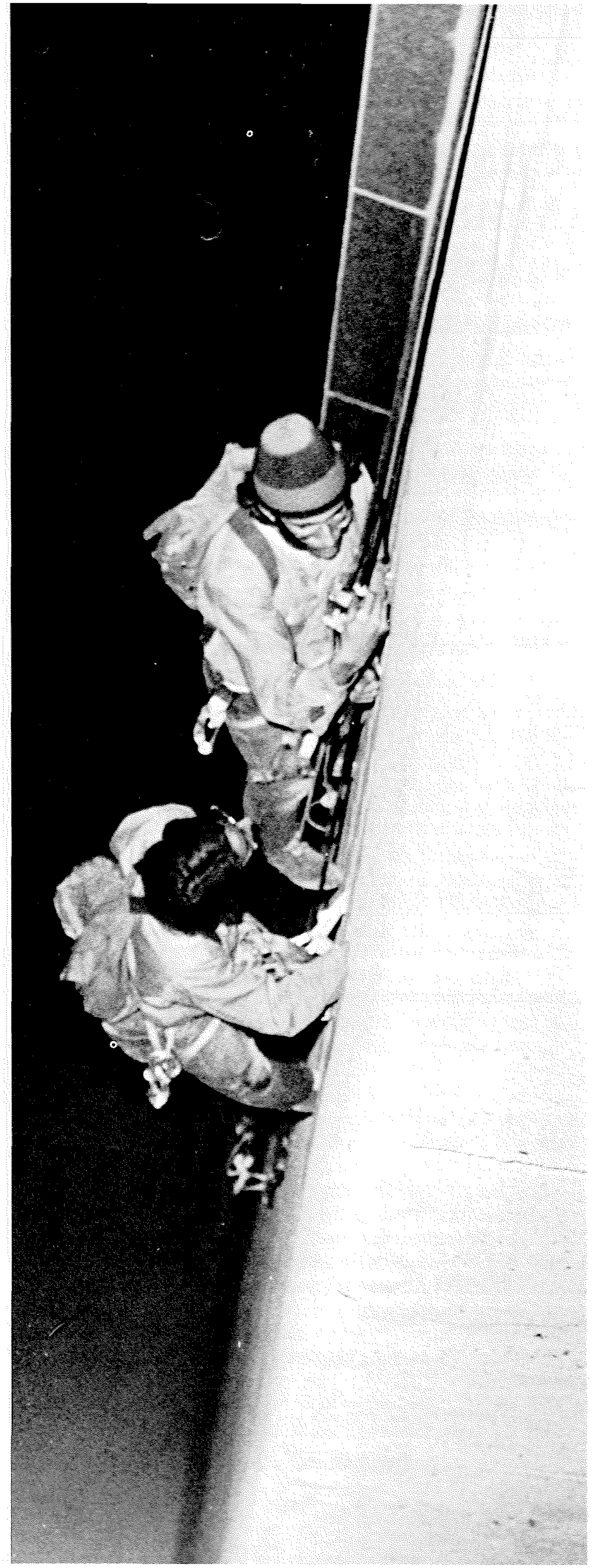
Caltech's climbers keep proving that mountains are where you find them.

On the night of February 5, 1971, Dwight Carey, a Caltech junior, and Bob Durst, a freshman, added an important chapter to the long, shadowy history of building climbing at Caltech. They became the first to scale the nine-story Millikan Library.

There is a widely held—and erroneous—impression that students do this kind of thing as some kind of daredevil prank. Actually, campus building climbers are generally accomplished mountaineers and rockclimbers who are just marking time until they can get out to the real thing again.

There must have been building climbers on campus as far back as the day the scaffolding for Throop Hall went up in 1910. But it was only a couple of years ago that the Alpine Club—the long-time haven of Caltech climbers—published the first *Climber's Guide to Caltech.* The guidebook doesn't claim to be complete, and it doesn't record anything that happened before the 1940's, but it is full of information on the 1950's—a time of prodigious climbing activity at Caltech.

Making history in one of the more unusual ways, Dwight Carey and Bob Durst near the end of the first ascent of the nine-story Millikan Library. Most of the distance was covered by inserting a grappling hook device in the ventilation grille holes.



One of the great climbers of the fifties was Dave Rearick, a mathematics graduate student. He is immortalized in the *Climber's Guide* through his exploits on the "Rearick mantles"—the 1½-inch-wide ledges, seven feet above ground, which form a decorative motif on the walkway arches along the older campus buildings. Rearick is still the only man who has ever been able to pull himself up by them and make it to a standing position. He was also the first man to assail the south and east faces of Robinson and to climb Spalding. His conquest of Spalding is still considered noteworthy because he ascended the building's layback in almost one fell up-swoop. He rested only once, halfway up, on the belay rope, which was handled by Howard Sturgis, '58, an active climbing enthusiast even though he had a severe problem with acrophobia.

Rearick scaled the east face of Gates once—but only once. When he reached the roof, he found that the trap door was locked. There was no suitable anchor for a rappel so he had to down-climb the route, a dangerous operation which involved lying on a sloping ledge just below the top and groping with one foot for a small horn on the top of the ornamental stonework. On another memorable night he took along a friend, Bill Woodruff, MS '60, for a climb up Arms by the south door pillars. Woodruff got stranded on the balcony and had to be rescued from inside by some expert lock pickers. Rearick kept in shape for all this activity by going over to the gym regularly, several times a week—to practice barefoot friction ascents.

Dave Rearick is now on the faculty at the University of Colorado, where the buildings are all the same, with sandstone faces—presenting few aesthetic features to a cilmber. "The only thing the students think they're good for," he says, "is to build up their fingers."

After a hiatus in which climbing was mostly limited to such steeplejackery as the Fleming House Mickey Mouse Club putting seasonal folderol on the Throop cupola, climbing came into another golden age in the late sixties. In those days Dave Rossum, Bob Jackson, Neil Erickson, and Keith Edwards were the nucleus of the Alpine Club.

A slight miscalculation marred the success of the first Millikan climb, forcing Carey and Durst to pull themselves up the last few feet by rope. Next time, they swore, they'd do it right.

They published the *Climber's Guide*, which they dedicated to Charles Wilts, professor of electrical engineering who is a well-known climber. Also, since he got his BS at Caltech (1940), stayed on for an MS and a PhD, and then became a member of the faculty, Wilts is probably more knowledgeable about every toe hold, finger space, and ledge on campus than any other man at the Institute.

Lacking transportation to rock country and anxious to exercise their mountaineering muscles, these climbers of the sixties concentrated on becoming the first to conquer the newer buildings of the north campus. Beginning in February of 1967, they mastered the south chimneys of Booth Computing Center and the south-southeast chimney of Steele. Then they drifted south a little and followed Rearick's trail up Arms and the east face of Gates.

Climbs followed each other in rapid succession for the next three months—barefoot friction ascents of the gym, free and rope ascents of the west face of Kerckhoff, a direct climb to the RA's balcony on the west side of Dabney House, the north chimneys of Booth, the Winnett chimney, a second-story traverse of Booth, and the north face of the Gates Library. The season was polished off in May with a neat little climb to the second-story door of Steele.

These were the great days when there were giant footprints up the east face of Millikan, and Alpine Club meeting notices were plastered 20 feet down from the top —all of which gave the climbers excellent rappel practice.

Last year Rossum, Edwards, and company figured out a way for Caltech students to climb for PE credit. They deftly assured athletic director Warren Emery that Charles Wilts would be glad to teach such a course. Simultaneously they informed Wilts that the PE department was in favor of such a course and would furnish ropes and climbing hardware. So last spring Wilts started teaching more than a dozen students the elements of climbing—for credit.

From last year's beginners' course, a number of the students went on to an advanced class this year. If interest keeps up, this is the way the progression will continue.

Wilts starts his classes with skull practice on a blackboard. He outlines the physics of climbing and gets students to appreciate its hazards—the failure of equipment and/or falling. Weather permitting, the class makes

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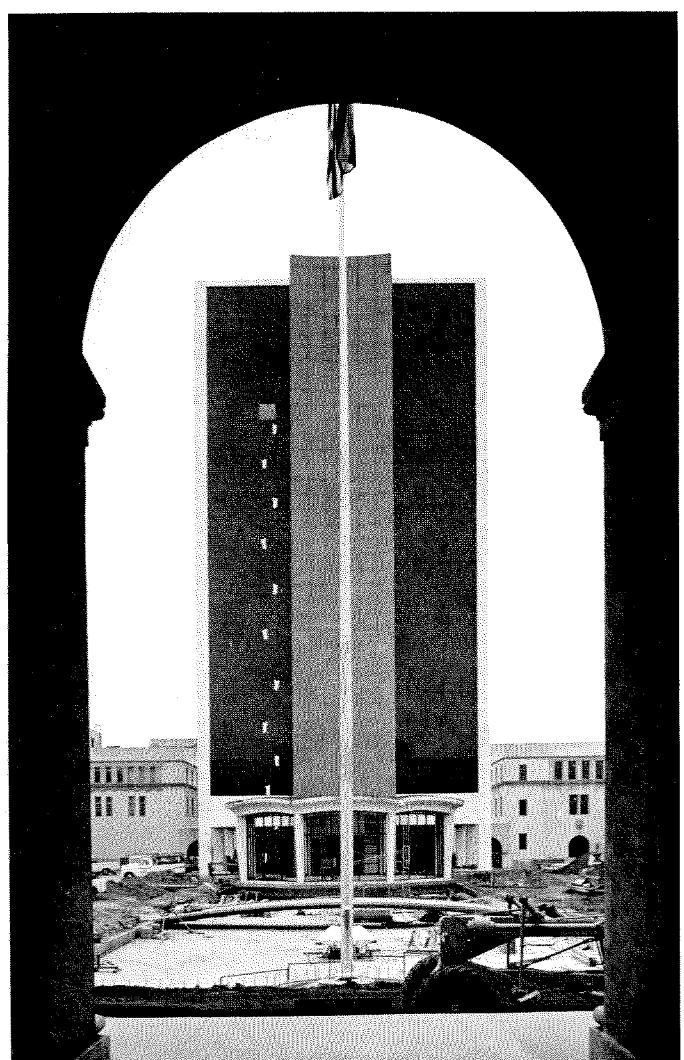


field trips one afternoon a week, the first time heading for Stony Point, an area of sandstone cliffs near San Fernando, where the climbs can begin from level ground. There, the first thing the beginners do is to get the feel of another person's weight at the end of a rope. Students pair off into teams and take turns at this.

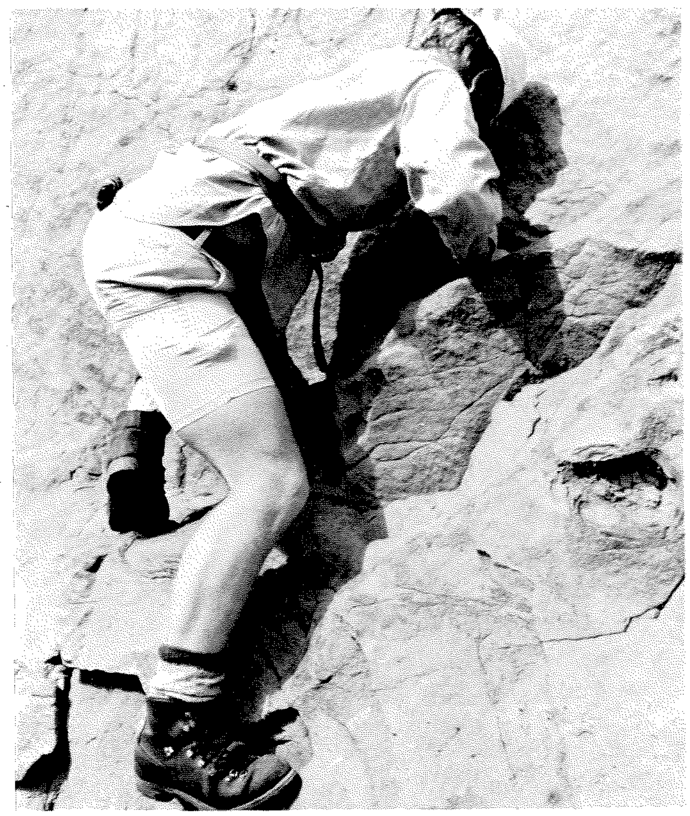
Climbing rocks by all possible routes is the second step. At Stony Point there are two rocks about 20 feet high which present different types of routes. Such short climbing is known as "bouldering." And although the climbs are short, they offer some of the tough and basic aspects—how to find and assess small niches, ledges, and rough spots, and what different kinds of weight distribution feel like.

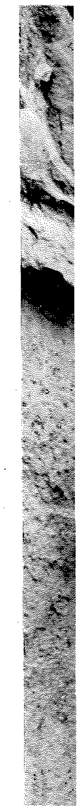
From instruction at Stony Point, the class moves to granite rock at Mt. Pacifico, which is reached by going up Angeles Crest Highway and striking inland on a rutty dirt road. The Pacifico climbs vary from 10 to 100 feet. The final examination for the course takes place on a weekend at Tahquitz Peak, near Hemet. There, the climbs are long (several hundred feet) and some are hair-raisers. Wilts is an authority on Tahquitz climbs, being the author of *A Climber's Guide to Tahquitz*, which is now in its fourth edition.

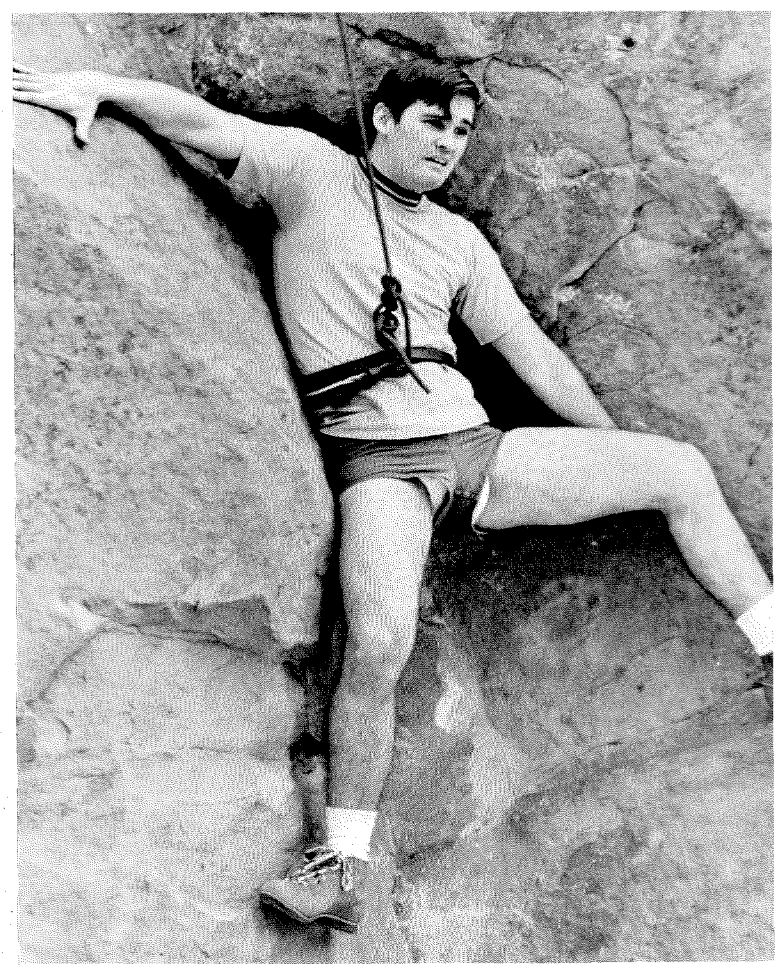
Caltech's climbers worry a little about the reckless oddball who may try to scale campus buildings as a workout for some sort of ego-salving. The use-your-brains-and-live boys have carefully researched hazards like chemical fans on top of laboratories, and ledges made slippery by years of chemical solvent deposits from vents.

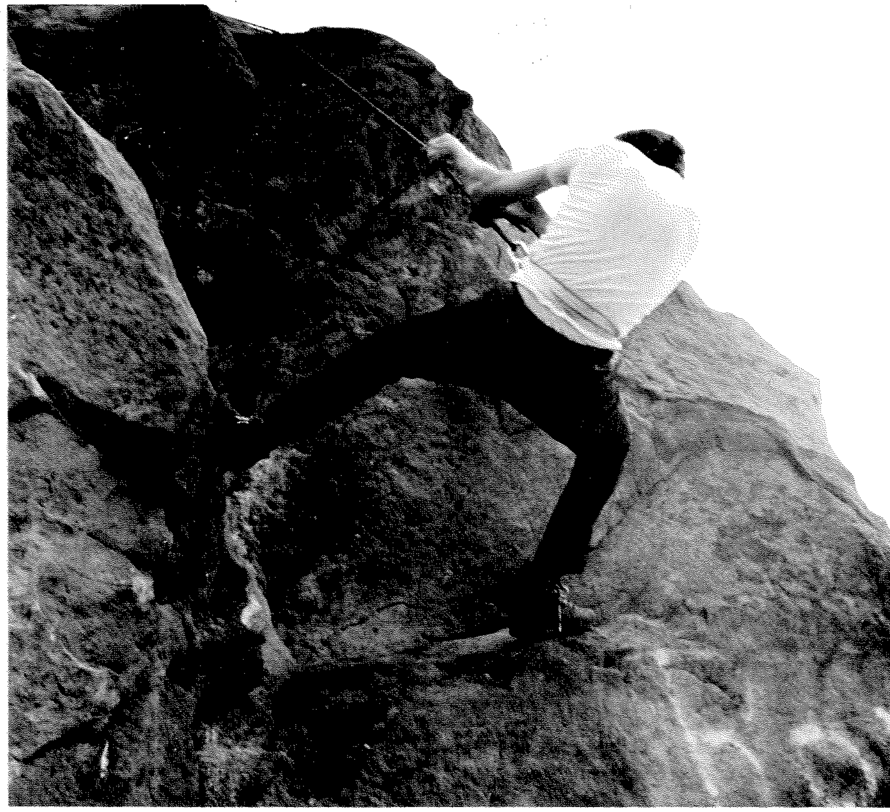
All in all, the climbers have so far acted in a pretty responsible way, and no one has been hurt—which makes it possible for the administration to maintain its delicate, unspoken agreement not to interfere with the activity. And that's the way *everyone* wants to keep it.

—*Janet Lansburgh*



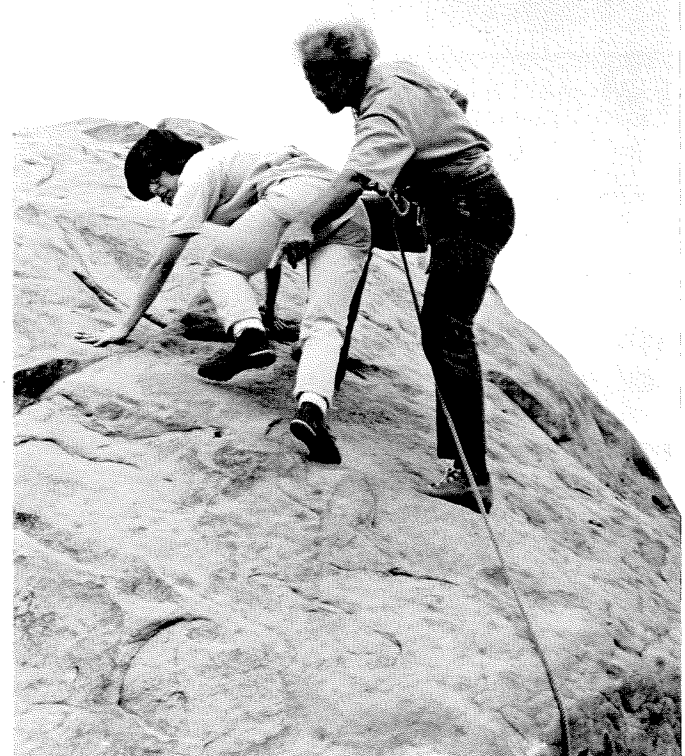






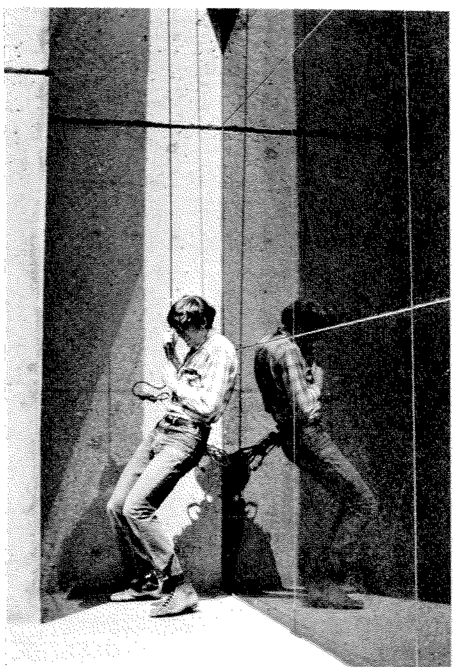
Beginners' Rock

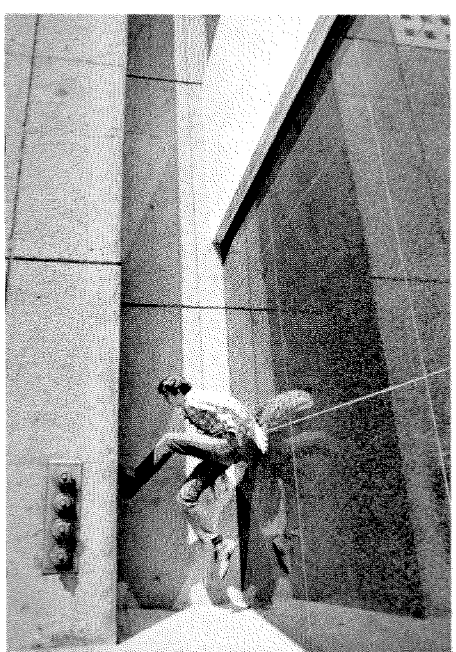
The first time Charles Wilts (right) takes his students to the beginners' rock at Stony Point in Chatsworth, they see what appears to be a benign backyard boulder. Within minutes they know it for a mocking monster that hides each finger ledge and toehold, and frustrates all comers. Even Wilts, who knows every square inch, treats it with respect because it is a great climbing teacher.

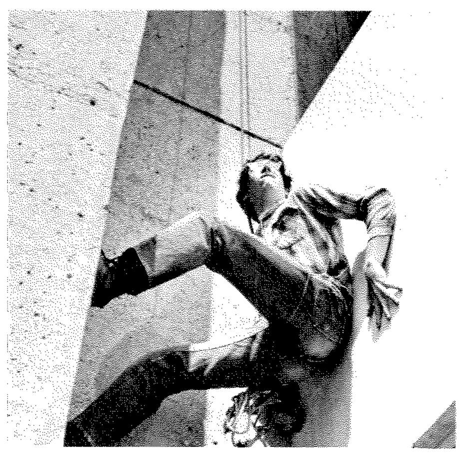


We only asked him to pose for a picture–

Tom Weaver, a freshman mathematics student, agreed to climb a little way up a Millikan Library "chimney" in broad daylight in the interests of illustrating this story. But since Weaver is a dedicated climber, when he reached the point of no return, there really was no doubt about his decision to go all the way. To follow his upward course, start here and read down *each column.*

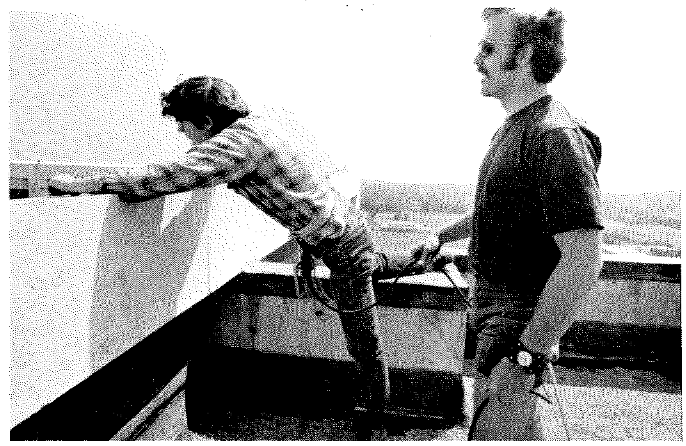


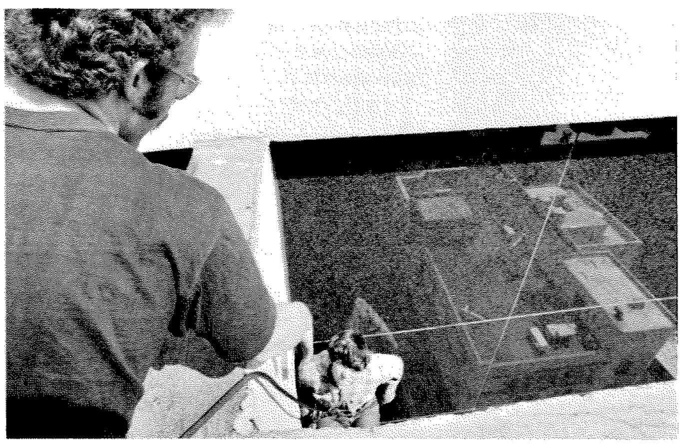


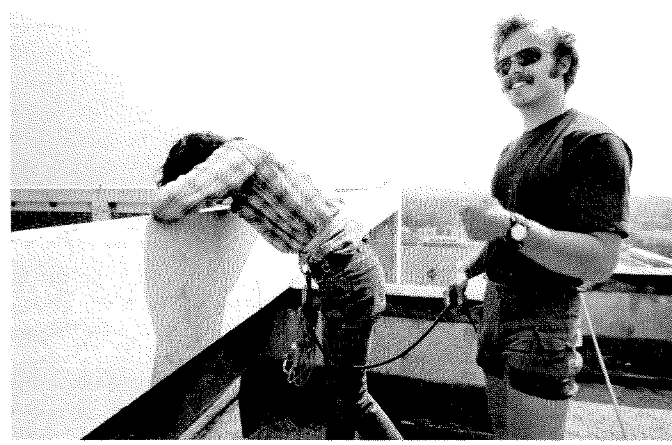


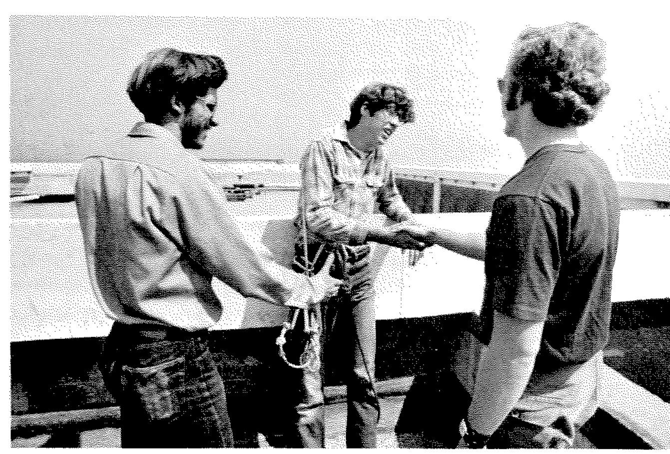
We didn't say: "Shoot the works."

Dwight Carey and Lary Andrews, a sophomore, were on the belay on the Millikan roof. Weaver had inched his way up using his back and legs, so he had plenty of arm strength to make it handily over the top—and demonstrate that climbers tend to look most unhappy when they are pleased.









MARS–STILL A MYSTERY

The Core of the Problem

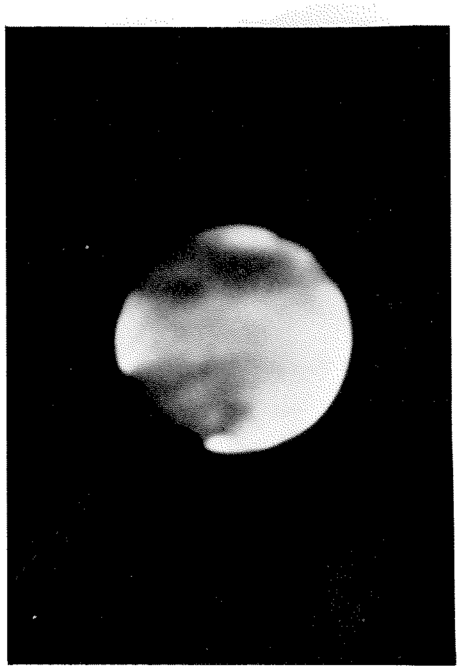
Earth's nearest neighbor in the solar system, Mars, has always seemed the planet most like our own. But, with a diameter roughly only half that of the earth, Mars seemed too small for a molten core like the earth's to have formed in it while the planet was solidifying from the primordial gas—along with all the other planets—about 4.6 billion years ago. New evidence, however, indicates that Mars is an even more earthlike body than has previously been supposed.

"Mars undoubtedly has a large core—perhaps 1,000 miles in diameter," says Don Anderson, professor of geophysics and director of the seismological laboratory, "and it is probably at least partly molten."

Anderson postulates his model of the interior of Mars from two sets of information: 1) a model of the earth's interior based on a considerable body of seismic, ultrasonic, shock-wave, static compression, and petrological data; and 2) some very exact measurements made by the several Mariner spacecraft that have flown behind the planet. The data include an accurate measurement of the diameter of Mars—4,208 miles, or about 75 miles greater than scientists had thought before the Mariner flights. This measurement made it possible to derive a precise figure for the mean density of Mars; it was also possible to make precise measurements of Mars' gravitational pull on the spacecraft. Previous studies of the orbits of the two small moons of the planet supplied its moment of inertia, which is related to the flattening at the poles.

All these data lead Anderson and graduate student Thomas Jordan to conclude that Mars is a differentiated body like the earth, with a mantle, a core that is probably molten, and possibly a crust.

The core of Mars is much less dense than the earth's, but its mantle is denser. The reason for this is that the temperatures in the interior of the earth are much higher than those in Mars, and the gravitational field of the earth has pulled most of the iron out of its mantle and deposited it in its core. Much of the iron still remains in the Martian mantle.



To migrate through a planet's interior, metal must be in molten form, but indications are that all of the core of Mars hasn't yet separated from its mantle. This implies that Mars hasn't gotten hot enough to melt all the iron, sulfur, and nickel, which are the core-forming materials. But it is likely that the Martian core is richer in sulfur than the earth's, since sulfur compounds melt at lower temperatures than pure metals and therefore migrate at lower temperatures.

Since Mars has so many other earth-like features, one might expect it also to have a magnetic field, but it does not. A magnetic field requires two factors, of which Mars has only one—a molten core. The second, a large enough moon to maintain currents or motions in the molten core, is lacking on Mars. Mars does have two small satellites about five to ten miles in diameter, but this is in contrast with the earth's moon, which has a diameter of 2,160 miles.

In the earth the core rotates at a different speed from the mantle—a phenomenon called differential rotation. The gravitational tidal forces produced by the moon on the rotation of the earth cause its axis of rotation to change, producing the differential rotation that drives the motions in the core. These tidal forces vary because of the elliptical and inclined orbit of the moon and the elliptical shape of the earth, and they are effective because of the large mass of the earth's moon.

Mars is proportionately poorer in iron than the earth because of the redistribution of iron toward the sun in the formation days of the solar system. The planets nearer the sun got more iron. Mercury, the innermost planet, is the richest of all the planets in iron.

Of all the material in the inner solar system—including the meteorites, the moon, and the four small, inner planets—the earth is most representative of the primordial chemical composition of the solar system and most similar to the composition of the heavier elements in the sun. If it were possible to make one planet out of Mercury, Venus, Mars, and our Moon, that planet would be the same size as Earth and have the same amount of iron in it.

At a time when the planets were formed and the sun was still condensing, the sun brightened considerably for a time, and the solar wind blew hundreds of times stronger than it does today. This wind blew the light gases—hydrogen and helium—away from the four small inner planets toward the outer planets. The inner planets were too near the sun to retain the light gases in any abundance; but the outer planets were able to retain them, perhaps as great shells of light material around small earthlike—and even Mars-like—bodies made of rock and iron.

A Layer of Ice?

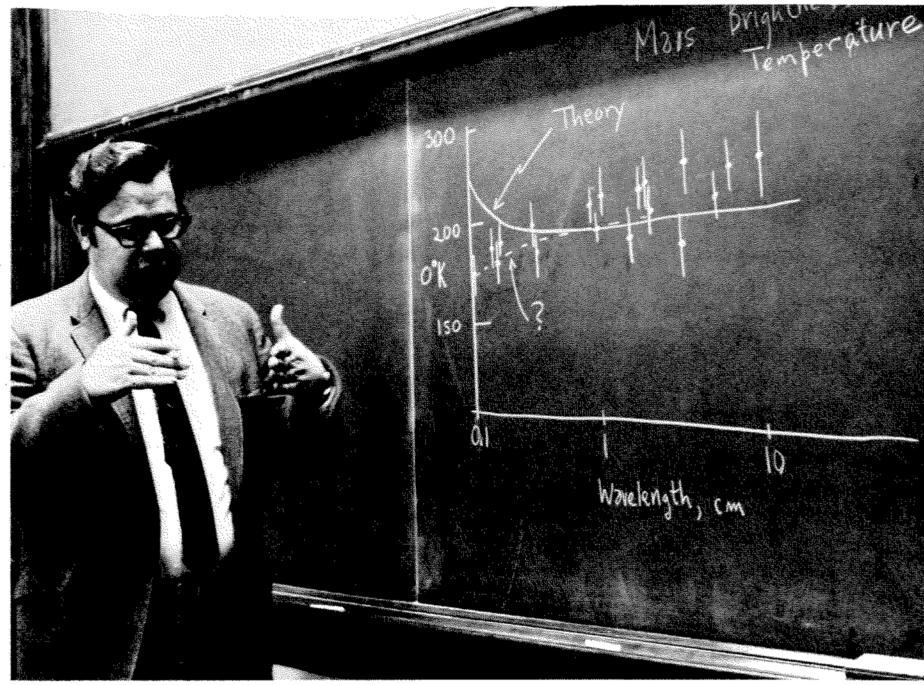
A puzzling sidelight on the question of life on Mars came up recently when Duane Muhleman, professor of planetary science and staff member of the Owens Valley Radio Observatory, found that temperature measurements of the Martian surface taken over the past six years indicate that the soil just below the surface is cooler than the soil a foot or more below that. This is just the opposite of the way soils behave on Earth and the Moon, where the warmer soils are on top during the daytime.

There's no immediate explanation for this phenomenon, but one of the hypotheses being advanced is that Mars has a layer of ice beneath its surface—and that could have tremendous implications for the possibility of life on Mars. If there is ice, there is water, and the presence of water is essential for life as we understand it.

Muhleman himself doesn't support the ice-layer hypothesis, preferring to attribute the anomaly to inaccurate measurements, which have been taken by radio astronomers around the world, using giant, dish-like antennae to pick up the radio waves emitted by the planet. But these measurements are difficult to do, partly because they must often be made at low angles to the earth through the densest layers of the atmosphere, and this tends to distort signals.

Another problem is that it's only feasible to study Mars when it's in line with Earth—about every two years—and the rest of the time it's too far away. This means that the planet is studied only when it's high noon there, the hottest time of the Martian day, and the measurements taken then are used to infer what the soil temperatures are during the coldest part of the Martian day.

Finally, Muhleman mistrusts the measurements simply because they're rarely checked. Most radio astronomers work at different wavelengths, since they don't want to study a frequency range already being investigated by somebody else. So, unless a researcher wants to go through the difficult and time-consuming process of checking somebody else's figures, those figures have to be taken at face value.



Duane Muhleman, professor of planetary science, diagrams an explanation of his mistrust of the ice-layer theory about the surface of Mars (dashed line). Elementary theory (represented by the curved solid line) predicts increasing brightness temperatures with decreasing wavelengths. But actual measurements by many different radio astronomers indicate decreases in brightness temperatures with decreasing wavelengths.

Muhleman, Glen Berge, a senior research fellow in radio astronomy, and geology graduate student Jeff Cuzzi will be studying Mars using Caltech's big radio dishes in the Owens Valley next August, when Mars will make its closest approach to Earth in 15 years. But these studies are not likely to answer the ice-layer question. The best views of Mars during this period will be from the Southern Hemisphere, and—disappointingly—no radio astronomers on that side of the world will be studying Mars at that time in the important wavelength region.

One way to resolve the issue would be to fly radio sensors on the next two Mariner spacecraft, which are scheduled to orbit Mars next fall. With the instruments in Mars orbit, temperature measurements could be taken during the complete Martian day. Unfortunately, no such experiments are planned.

What all this means for the possibility of ice on Mars remains difficult to assess with any certainty. This question is another that apparently must wait for an answer until we can land an instrument package on the Martian surface—probably in 1975 when the Viking missions are scheduled.

Life on Mars? Possible, but Still Improbable

It's still improbable that life exists on Mars, but it's not as improbable as it seemed after the early Mariner-Mars fly-bys disclosed the planet's thin atmosphere and bleak, moon-like surface. At any rate, organic compounds that are believed to have been precursors to life on Earth are probably also being produced by sunlight on the Martian surface—or just beneath it.

That's about as far as Norman Horowitz, professor of biology and executive officer for the division of biology, is willing to go at this point toward answering one of the tantalizing questions of our time.

Horowitz and two collaborators, Jerry Hubbard and James Hardy of Caltech's Jet Propulsion Laboratory, have been working for the past year on a series of NASA-sponsored experiments in which formaldehyde, acetaldehyde, glycolic acid, and other organic compounds were produced in a simulated Martian environment. The researchers used gases known to be present in the Martian atmosphere, plus ultraviolet radiation in wavelengths known to reach the Martian surface. Horowitz thinks it likely that these compounds would react with ammonia—if Mars has ever had any—to produce amino acids.

The Martian conditions were simulated by using fine soil or pulverized glass in a gas mixture of 97 percent carbon dioxide, with added carbon monoxide and water vapor. This represented the atmosphere indicated by the data returned from Mariners 6 and 7. Both the soil and the glass were sterilized at high temperatures before being used in the tests.

The ultraviolet radiation, which approximated the radiation that strikes the Martian surface as indicated by the two Mariner 1969 spacecraft, was produced by a high-pressure xenon lamp. Some experiments used a low-pressure mercury lamp. A large number of tests, varying in duration from three hours to seven days, were conducted.

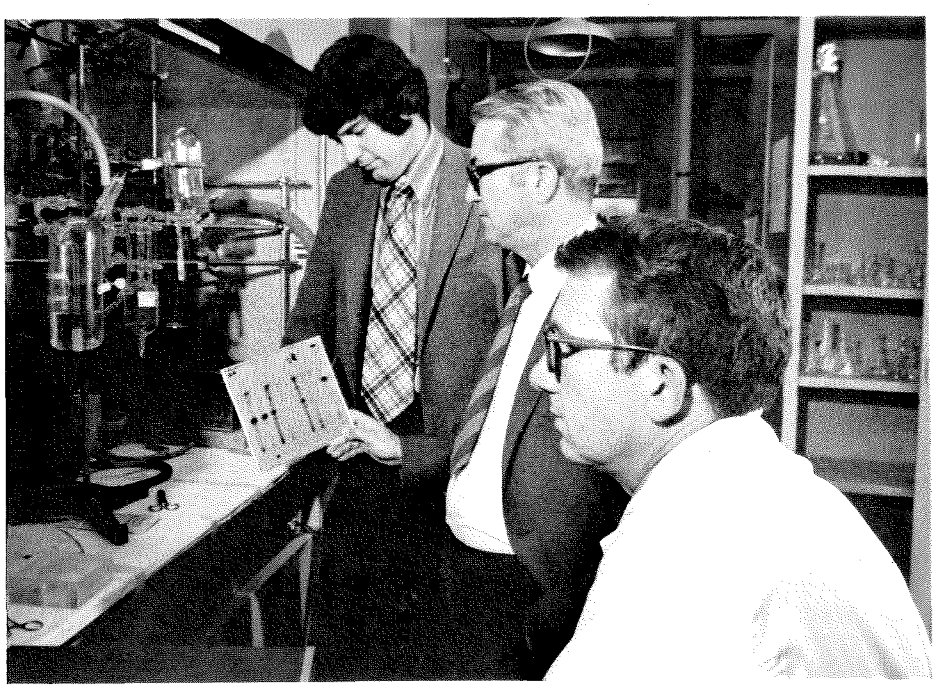
One of the key features of the experiment was its use of material to simulate the Martian surface. The organic compounds were produced by ultraviolet rays of wavelengths longer than 2,000 angstroms acting on the gases adsorbed on the surface material. Previous experiments had shown that ultraviolet rays in these wavelengths could not produce such compounds, but the earlier investigations had worked only with irradiation of the free gases.

Horowitz's experiment shows that radiation over a broad range between 2,000 and 3,000 angstroms can form the organic compounds. Wavelengths shorter than about 2,000 angstroms are thought to be absorbed by the heavy carbon dioxide content of the Martian atmosphere.

Ultraviolet radiation can also be a destroyer of life and lesser organic compounds, and on Mars the organic compounds may be constantly synthesized in the soil and constantly destroyed by the same radiation, resulting in a steady-state concentration of organic matter. Some compounds may sift down into the deeper soil and be protected from such destruction, and thus accumulate.

How much of these compounds the Martian soil may be producing depends in part on how much carbon monoxide and water there is in the planet's atmosphere. But even though it's only a small amount, there still could be considerable quantities of organic matter formed over geologic time. The experiment showed that irradiation over a longer period of time caused a larger conversion of carbon monoxide to carbon dioxide and organic products, up to a certain point. Reducing the amount of water vapor or the surface material reduced the organic accumulation.

The findings of the Horowitz team, announced in the March issue of the *Proceedings of the National Academy of Sciences,* considerably increase interest in the outcome of the data that will be returned from the two Mariner spacecraft to be launched in May and to go into orbits of Mars in November. Thereafter, for three months, television cameras and sensors will concentrate on an almost complete mapping of the Martian surface and an analysis of its cloud, dust, polar cap, and color characteristics. And in 1975 the United States will attempt to land two packages of life-detection instruments on Mars—which indeed may bring us much closer to a definitive answer to that tantalizing question.



Will life arise wherever conditions exist for the synthesis and evolution of organic compounds? Caltech professor of biology Norman Horowitz (center) and two colleagues at JPL—James P. Hardy and Jerry S. Hubbard—have found autoradiograph evidence of three: formaldehyde, acetaldehyde, and glycolic acid. Believed to be precursors of biological molecules on the earth, these compounds were found in tests simulating atmospheric and sunlight ultraviolet conditions on Mars. Horowitz, who has been studying the planet for a decade, thinks this is the most favorable indication for possible Martian biological evolution that has turned up in the last five years.

The Red Shift Yardstick

Are quasars as far away as their great red shifts imply? Is the red shift a valid yardstick for measuring their distance? James E. Gunn has produced evidence that seems to answer yes to both of these questions. On the other hand, Halton Arp has found two unusual galaxies that do not conform to the red shift yardstick. Both astronomers are staff members of Hale Observatories.

The red shift is a carefully calibrated gauge for measuring the distances of galaxies. It is obtained by sorting the light from galaxies into its respective wavelengths and recording them as lines on a photographic plate. The farther away the object, the farther these lines are shifted toward the red—or longer wavelength—end of the spectrum.

All quasars have comparatively large red shifts, and some are much larger than those of any other object in the universe. The question of how an object that appears to be so bright can be so far away has led some scientists to conclude that quasars must be comparatively nearby. If this is true, it would indicate that their great red shifts may not be valid distance indicators.

One way of solving the problem would be to find a quasar associated with a group of galaxies and then to compare the quasar's red shift with that of the galaxies. If they were similar, it would seem to show that the brighter quasar was really very distant and that the red shifts of quasars are good yardsticks for both quasars and galaxies. This is just what James Gunn has done.

Using the 200-inch Hale telescope at Palomar Observatory, Gunn photographed and obtained the spectrum of a bright quasar in an unnamed cluster of galaxies. He compared the red shift of the quasar and of its associated galaxies and found that they are apparently the same, placing both about three billion light years distant from earth. This finding is direct evidence that quasars are as distant as they seem to be; and it supports the validity of the controversial red shift yardstick for measuring the great distances of the universe.

In the March 15 issue of *The Astrophysical Journal*, Gunn reports his success in making this comparison with one actual quasar and with one quasar-like object—a large galaxy with a quasar-like nucleus.

The quasar Gunn used has the name PKS 2251 plus 11. The PKS means that it was discovered at the Parkes Radio Observatory in Australia and is listed in the Parkes catalog. The numbers give its position in the sky in terms of right ascension and declination.

Astronomer James Gunn has discovered evidence that the red shift is a valid distance indicator for quasars, but...

PKS 2251 plus 11 has all the characteristics of a quasar: It is bright—about ten times more luminous than the brightest galaxies in its cluster; it has a small star-like image; it is blue; and it has a large red shift. It also radiates energy in the radio frequencies as many quasars do.

Near PKS 2251 plus 11 Gunn found an unusual, fuzzy, cloud-like object that may have been ejected from the quasar. Named Ton 256—because it is the 256th object listed in the catalog of Mexico's Tonantzintla National Observatory—this object is now known to be an elliptical galaxy with a brilliant quasar-like object as its nucleus. Comparison of the distances of Ton 256 and of the cluster of galaxies in which it is found again showed the validity of the red shift as a measurement of distance.

Halton Arp's investigation of two very bright—but also very unusual—galaxies seems, however, to controvert the theory. One of the galaxies is considerably larger than the other, and they are apparently connected by a plainly visible arching bridge—presumably of stars—and a second fainter bridge with a much more pronounced arch. According to their red shifts, these galaxies should be a third of a billion light years apart. However, Arp's direct photographs indicate that they are only 30,000 light years from each other.

The red shift of the larger of these galaxies indicates it is some 325 million light years distant from earth. The much greater red shift of the smaller of the two implies a distance of 650 million light years—twice as far away as its companion.

Using a three-hour exposure Arp photographed these companion galaxies with the 200-inch Hale telescope. His findings, reported in the February issue of *Astrophysical Letters*, indicate that neither object is a spiral like our Milky Way Galaxy. The larger one, NGC 7063 (NGC stands for New General Catalog), is a Seyfert galaxy, the kind of galaxy that has a very bright nucleus. The smaller galaxy apparently was ejected from the larger one some 10 million years ago, leaving a trail of luminous material behind it. The larger galaxy is disturbed —with a small, compact nucleus that shows evidence of hot, excited gas. The smaller is brighter per unit area than its companion and is perhaps a compact body of stars.

These are very unusual galaxies, and Arp interprets their red shift as a combination of two effects—of recession and some other effect as yet unknown.

...colleague Halton Arp's discovery of these two unusual galaxies just might controvert the theory.

The Month at Caltech

Commencement Speaker

James C. Fletcher, newly appointed administrator of the National Aeronautics and Space Administration, will be the speaker at Caltech's 77th commencement on June 11. He is a Caltech alumnus (PhD '48) and was the recipient of one of the Institute's first Alumni Distinguished Service Awards.

A native of New Jersey, Fletcher holds a bachelor's degree from Columbia University. Even before he took his Caltech degree, he did research on sonar and underwater devices with the U.S. Navy Bureau of Ordnance. In 1941 he was a special research associate at Harvard University; and in 1942 he went to Princeton as a teaching fellow, instructor, and research physicist.

At the end of World War II he began graduate work at Caltech, and after receiving his doctorate became director of the theory and analysis laboratory of the Hughes Aircraft Company, working there on the Falcon air-to-air missile and the F-102 all-weather interceptor. In 1954 Fletcher joined the Ramo-Wooldridge Corporation and soon became director of its Space Technology Laboratories, which had technical responsibility for all the nation's intercontinental ballistic missiles, the intermediate range Thor missile, and our first space probe—Pioneer IV.

Fletcher was one of the organizers in 1958 of the Space Electronics Corporation, which in 1960 became a part of Aerojet-General Corporation. In 1964 he resigned as Aerojet's systems vice president and president of its subsidiary Space-General Corporation to become president of the University of Utah.

Among Fletcher's contributions to the nation are service on more than 50 national committees and chairmanship of 10 of these. He has been a member of President Johnson's Science Advisory Committee, the Task Force on Higher Education, and the President's Committee on the National Medal of Science.

Guggenheim Fellowships

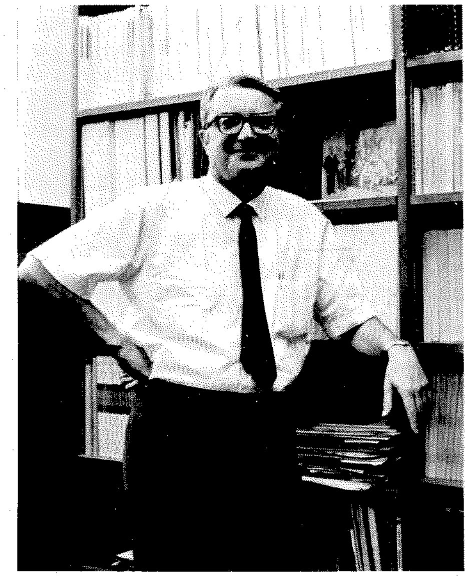
Three Caltech faculty members and three alumni are among the 354 winners of John Simon Guggenheim Memorial fellowships for 1971. The fellowships are awarded on the basis of demonstrated accomplishment in the past and strong promise for the future.

Steven Frautschi, professor of theoretical physics, will be doing theoretical studies in high energy particle physics at CERN, a Swiss research center for the study of nuclear and particle physics, located in Geneva. Frautschi, who has been at Caltech since 1962, will be on leave from September 1971 to September 1972.

Murray Gell-Mann, Robert Andrews Millikan Professor of Theoretical Physics, will also be at CERN for the same period. Gell-Mann came to Caltech in 1955 and was winner of the Nobel Prize in physics in 1969. He will be making theoretical studies in elementary particle physics.

G. Wilse Robinson, professor of physical chemistry and a member of the faculty since 1959, is making studies in photobiology. He left Caltech this month to spend five weeks in England beginning his project; after a summer back in Pasadena, he will leave in September for three to five months in New Zealand.

Caltech alumni recipients of Guggenheim fellowships this year include Robert L. Kovach, PhD '62, professor of geophysics at Stanford, who will be making studies of man's intervention in geologic processes; David E. Metzler, BS '48, professor of biochemistry at Iowa State University, who will work on the chemical reactions of living cells; and Steven E. Schwarz, BS '59, MS '61, and PhD '64, associate professor of electrical engineering and computer sciences at UC Berkeley, who will do research in quantum electronics.



New Executive Officer

Norman H. Horowitz, professor of biology, has been appointed executive officer for biology—a newly created position. He will assist the chairman, Robert L. Sinsheimer, in the administration of the division.

Horowitz, a Caltech alumnus (PhD '39), has been a member of the faculty of the Institute since 1946. He is noted for his work in biochemical genetics, and is at present studying the water metabolism of the common mold Neurospora. For five years (1966-70) he was head of the bioscience section of the Jet Propulsion Laboratory. He is now a part-time consultant to the JPL Viking biology team, which is designing a spacecraft that will land on the planet Mars in 1975 and search for evidences of life there. The team is also investigating the fundamental chemistry of pre-biological synthesis of organic matter on the planets.

With Horowitz, there are now eight executive officers among the six divisions at the Institute. The others are Norman Davidson, for chemistry; David Elliot, for humanities and social sciences; Jesse Greenstein, for astronomy; W. A. J. Luxemburg, for mathematics; Jon Mathews, for physics; C. J. Pings, for chemical engineering; and Ernest Sechler, for the graduate aeronautical laboratories.

Honors and Awards

Felix H. Boehm, professor of physics, will be doing advanced research in Switzerland next fall under a National Science Foundation Senior Faculty Fellowship. He will work at CERN, a leading Swiss research center in nuclear and particle physics, studying the nuclear properties in mesic atoms (the short-lived systems connecting a meson and an atomic nucleus).

At CERN, he will collaborate with Egbert Kankeleit, who was a senior research fellow in physics at Caltech in 1964. At that time, Kankeleit and Boehm worked together in pioneering research into the "weakly interacting" nuclear force in nature.

Boehm, who is a graduate of the Federal Institute of Technology in Zurich, has been at Caltech since 1953.

Carver A. Mead, professor of electrical engineering, has won the T. D. Callinan Award of the American Electrochemical Society's Dielectrics and Insulation Division. The award recognizes Mead's work in developing the theory of flow of electric current in dielectric materials (materials that do not readily conduct electricity) and for his work on dielectric thin films in microelectronics.

Mead received his BS at Caltech in 1956, his MS in 1957, and his PhD in 1959. He has been a member of the faculty since 1958.

Jack E. McKee, professor of environmental engineering, was recently presented with an Outstanding Engineering Merit Award by the Institute for the Advancement of Engineering. McKee, who has been a member of the Caltech faculty since 1949, has received numerous honors for his pioneering research on water quality and waste treatment.

Thomas A. Tombrello Jr., associate professor of physics, and Edward C. Stone Jr., assistant professor of physics, are among 77 young physical scientists who have just been awarded Alfred P. Sloan Foundation research fellowships.

Sloan research fellows are selected for outstanding research potential on the basis of nominations by senior colleagues who are familiar with their work. They receive research support averaging $8,750 a year for two years and are free to shift the direction of their research at any time if a more promising line of inquiry becomes apparent.

Tombrello is currently working with the low temperature physics group in the Kellogg Radiation Laboratory in a cooperative venture to test the feasibility of building a superconducting linear accelerator for accelerating heavy ion beams. He is also working on the application of nuclear physics to solid state and astrophysical problems.

Stone's research is in the general field of cosmic rays. He is involved with several satellite and balloon-borne experiments for NASA that are designed to provide information on the origin of cosmic rays, their propagation in space, and their interaction with the earth's magnetic field.

Freshman Re-orientation

Approximately 150 freshmen, upperclassmen, and professors turned up in Dabney Garden on April 10 to spend most of a sunny Saturday in a Freshman Orientation Workshop discussing problems in placement procedures, courses, and social life at Caltech. Sponsored by the Deans' Office and the Caltech Y, the workshop gave students and faculty a chance to compare problems, as this group is doing with Robert Sinsheimer, chairman of the biology division. Another outcome was a long-range plan for collecting data about classes, instructors, and Institute life in general.

Soon tests will begin on a bright idea for roofing stadiums with stainless steel balloons. And nickel's helping make it happen.

It sounds like something out of Jules Verne. Actually, it's fresh out of our advanced design studies.

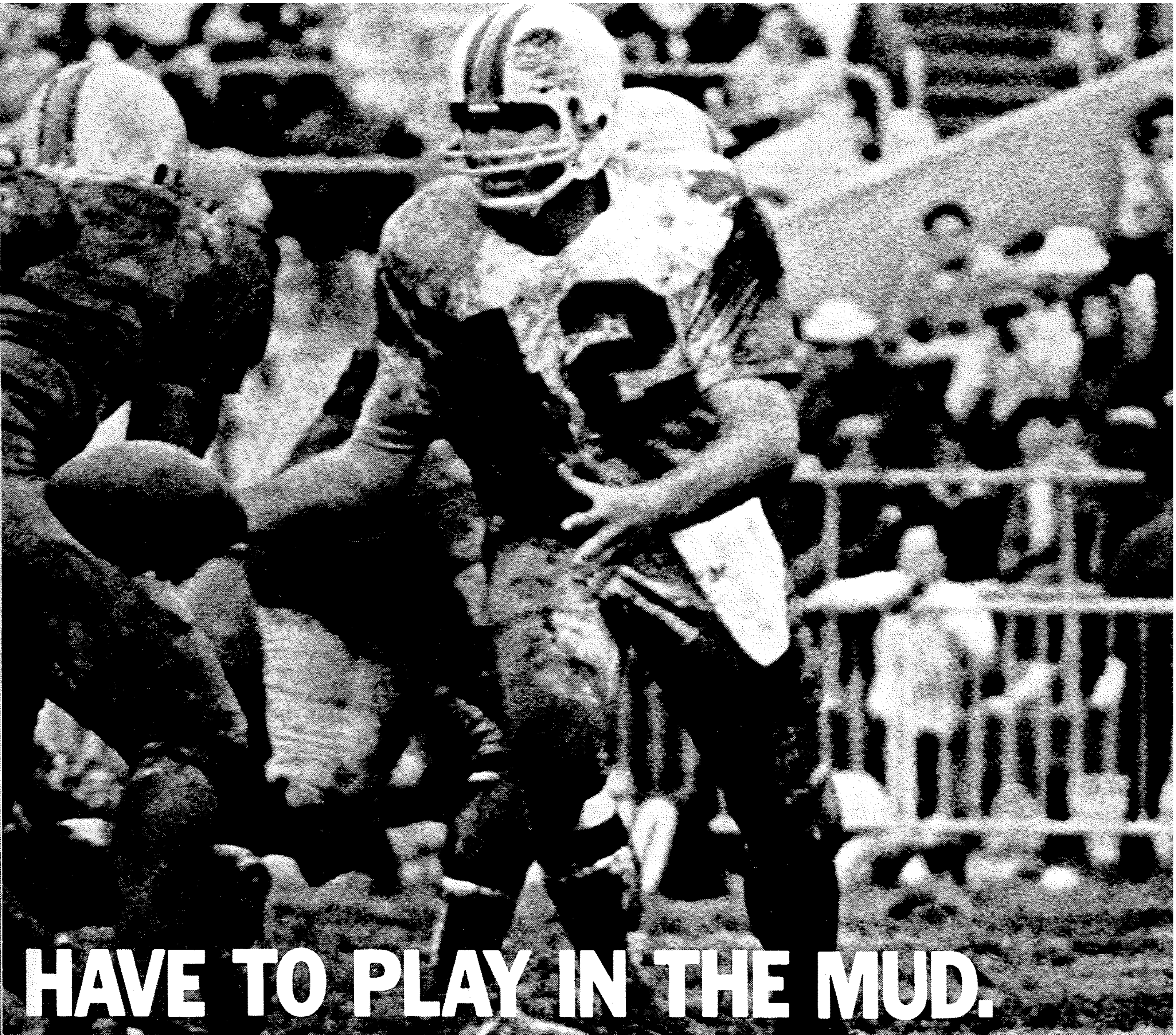
A gigantic, *inflatable* metal lid that can be stretched across a football stadium without any pillars or posts of any kind.

The idea is so mind-boggling that most people have a hard time visualizing it.

Think of a pie that's hollow inside, with the bottom and the top made of a metal skin only 1/16th of an inch thick. When the air is pumped into the pie, the whole thing gets so rigid it can be jacked up into place over the field and never even flutter during a windstorm.

The weather stays outside, the players don't slide around on their backsides, and the spectators don't drown. Somehow, the whole thing seems a little more civilized than a public mud bath.

And the cost could be as little as 1/3 of a conventional trussed roof.



The metal is nickel stainless steel. The nickel is there to make the skin easier to work, and to give it the necessary toughness and strength. Plus corrosion resistance.

It's a fascinating idea, this revolutionary roof of ours, and scale models are about to be thoroughly tested.

But the point of the story is this. Just as our metal is a helper, one that makes other metals stronger, or easier to work with, or longer lasting, so International Nickel is a helper.

We assist dozens of different industries all over the world in the use of metals. We offer technical information. And the benefit of our experience. Often, Inco metallurgists are able to anticipate alloys that will be needed in the future, and to set about creating them. Sometimes, we come up with whole new concepts—like a stainless steel balloon for a stadium roof.

This kind of genuine helpfulness, we figure, will encourage our customers to keep coming back to us.

And that helps all around.

The International Nickel Company, Inc., New York, N.Y. The International Nickel Company of Canada, Limited, Toronto. International Nickel Limited, London, England.

Model test roof of nickel stainless steel.

INTERNATIONAL NICKEL HELPS

In the minds of many, modern technology has created a monster.

The computer.

We've all heard the stories about people making, say, a $30 purchase. And then being billed for $3,000 by the computer.

Nonsense.

The danger is not that the computer makes mistakes, but that human errors remain uncorrected while the machine rolls on, compounding them.

Computers are literal minded. They must be correctly instructed to help us in the solution of problems. They do exactly what they are told. Not what they ought to have been told.

The computer is man's assistant. Not his replacement.

The unaided human mind needs help to cope successfully with the complexity of our society.

Intellectual aids, such as computers, will not only increase the skill of our minds, but leave more time for human creativity by freeing man of burdensome routine tasks.

Do we really believe that our achievements in space could have been accomplished without computer assistance?

Do we really believe that we can function efficiently in our complex modern environment without computer assistance?

The answer, of course, is obvious.

In truth, the invention of the computer can be compared with the invention of the printing press.

Engineers engaged in the development of computer systems are convinced that over the next decade it is possible to develop networks of interconnected computer systems capable of offering a wide variety of services to the public.

By necessity, one-way mass communications — radio, television—deal with a common denominator of entertainment. This situation can be changed by developing computer-based systems that offer each individual an almost unlimited range of entertainment and information. Each individual will select what he wants, and to how great a depth he wants to delve into the areas in which he is interested.

At his choice of time.

Apply this principle to education.

What it amounts to is individualized instruction. To meet simultaneously the needs of many students.

From a practical standpoint, limits to excellence in education are almost purely economic.

The computer provides a solution by performing high quality instruction for large numbers of students, economically.

Our goal is to make it possible for a teacher to provide individual guidance to many students, instead of few.

Yet, computer-assisted instruction is not a concept which has been enthusiastically embraced by all. There are many who feel that the computer will replace teachers.

Not so.

This interpretation implies mechanizing, rather than personalizing, education.

Everywhere in our lives is the effect and promise of the computer.

Its ability to predict demand makes it possible to apply the economies of mass production to a wide variety of customized products.

It will allow for the use of a computer terminal device for greater efficiency in home shopping and much wider diversity in home entertainment.

It can be a safeguard against the boom and bust cycle of our economy.

In short, the computer means accuracy, efficiency, progress.

The computer affords us the way to store knowledge in a directly usable form—in a way that permits people to apply it without having to master it in detail.

And without the concomitant human delays.

The computer is indicative of our present-day technology—a technology which has advanced to such an extent that man now is capable, literally, of changing his world.

We must insure that this technological potential is applied for the benefit of all mankind.

If you're an engineer, scientist or systems programmer, and want to be part of RCA's vision of the future, we invite inquiries.

If you are interested in a comprehensive index of over 1100 technical papers published by RCA scientists and engineers last year, let us know.

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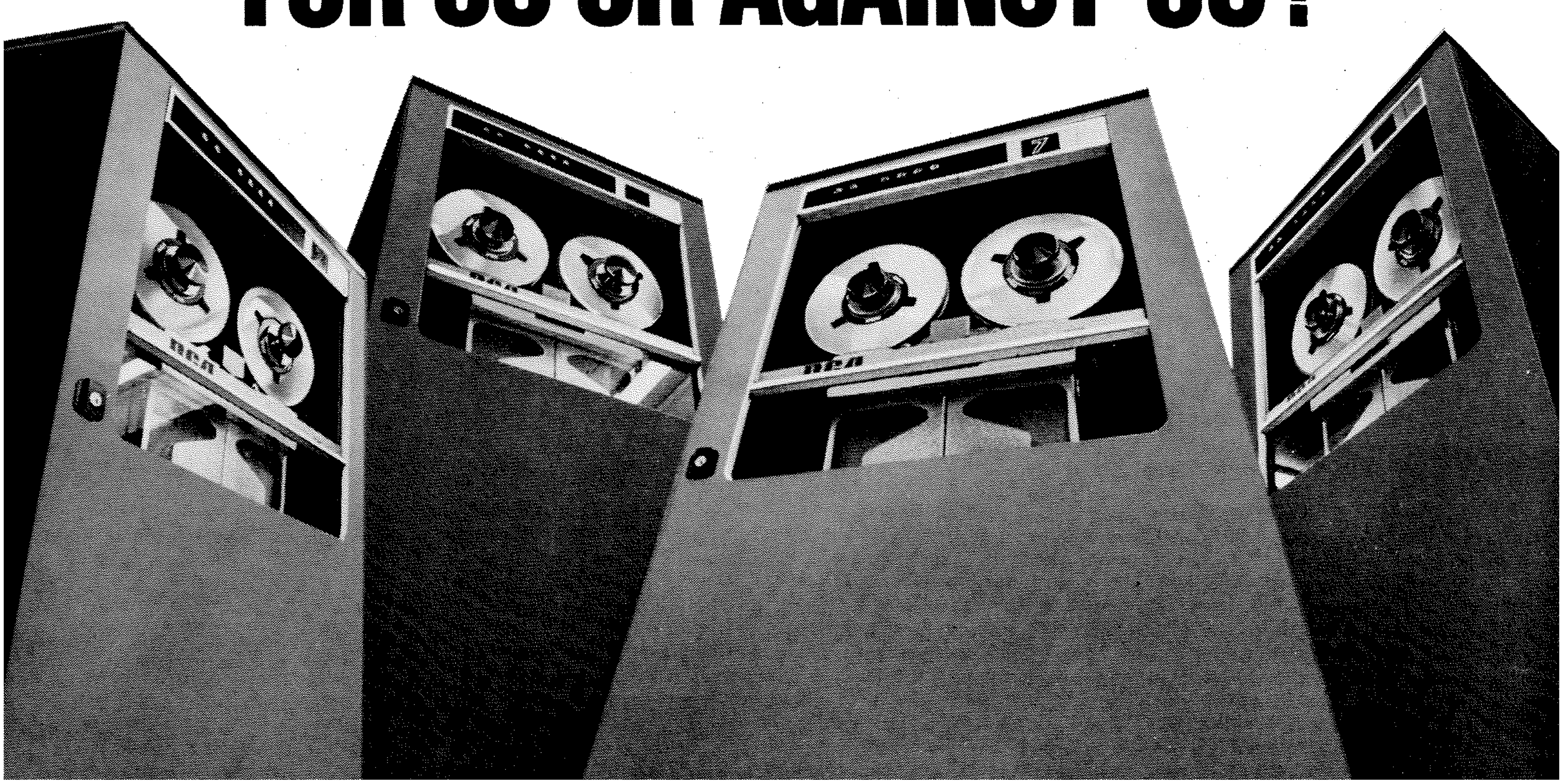

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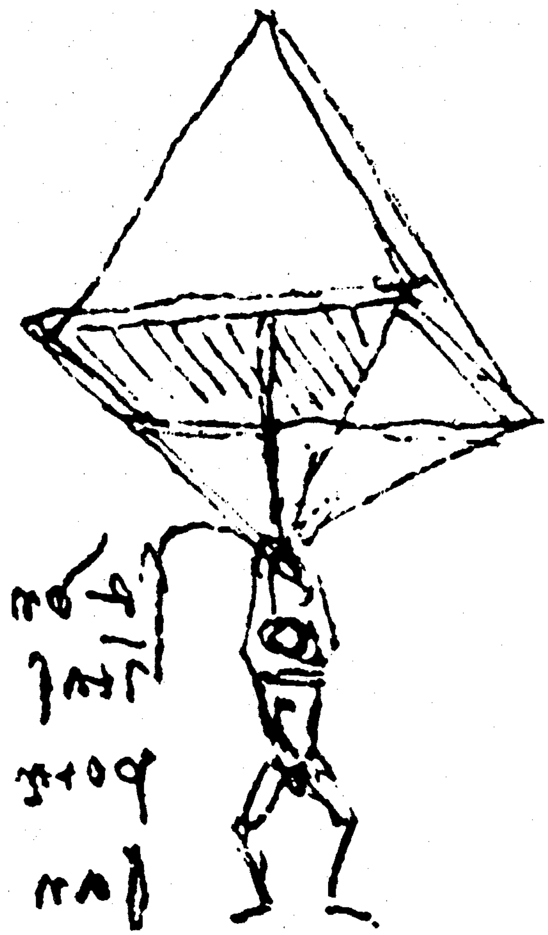
Calling us just a telephone company is like calling Leonardo DaVinci just a painter.



Leonardo's parachute.

General Telephone & Electronics is involved in domestic and international telecommunications . . . home entertainment . . . every type of home and industrial lighting . . . computer software systems . . . and all phases of advance research.

But please don't get us wrong. We started in the telephone business. We grew up in the telephone business. And we're still very much in it.

So we don't really mind your referring to us as just a phone company.

It simply serves to remind us of how far we have come.

GTE

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On your way up in engineering, please take the world with you.

The best engineers are far from happy with the world the way it is.

The way it is, kids choke on polluted air. Streets are jammed by cars with no place to go. Lakes and rivers are a common dumping ground for debris of all kinds.

But that's not the way it has to be.

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General Electric engineers don't look for overnight solutions. Because there aren't any. But with their training and with their imagination, they're making steady progress.

Maybe you'd like to help. Are you the kind of engineer who can grow in his job to make major contributions? The kind of engineer who can look beyond his immediate horizons? Who can look at what's wrong with the world and see ways to correct it?

If you are, General Electric needs you. The world needs you.

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