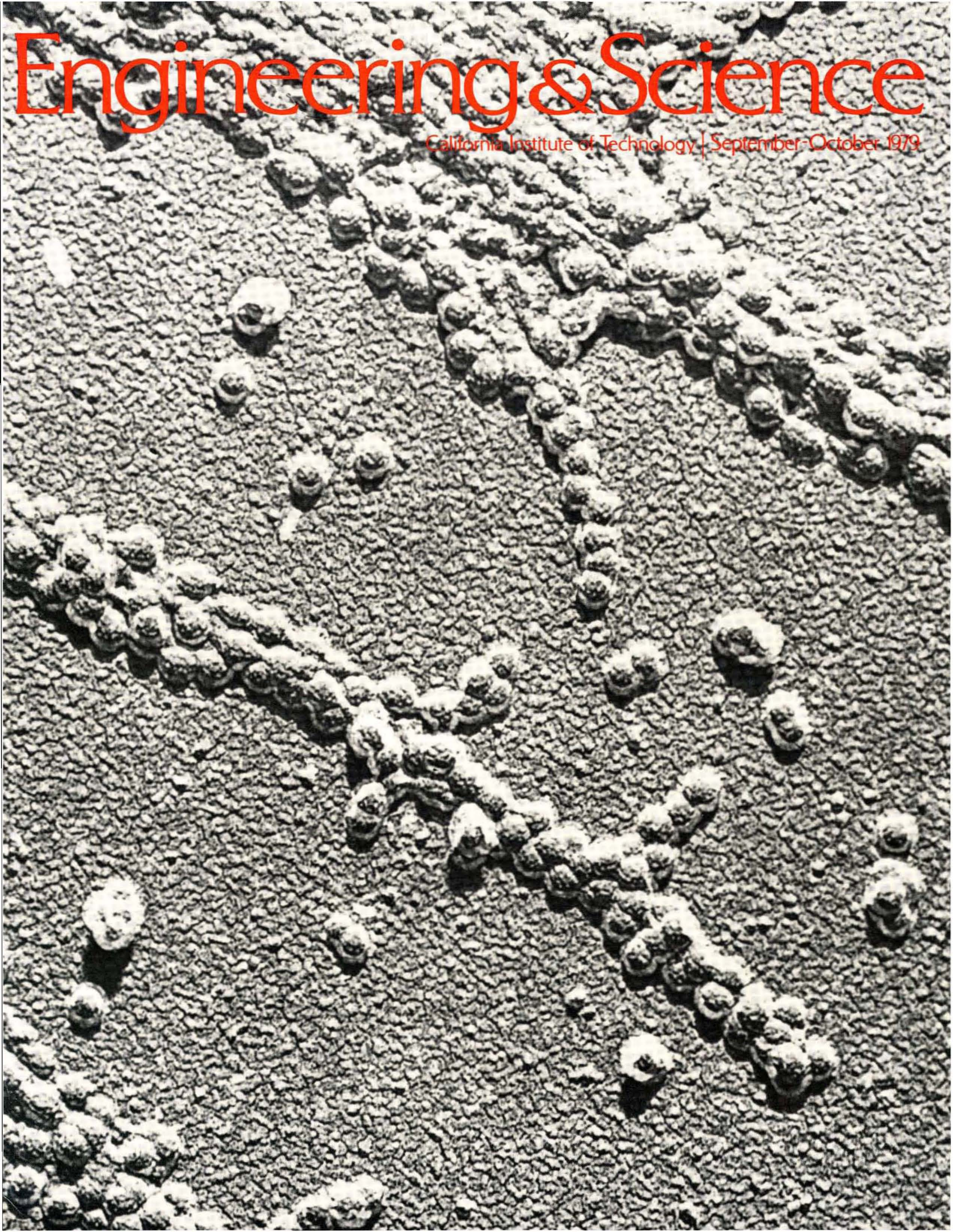
Engineering & Science

California Institute of Technology | September-October 1979



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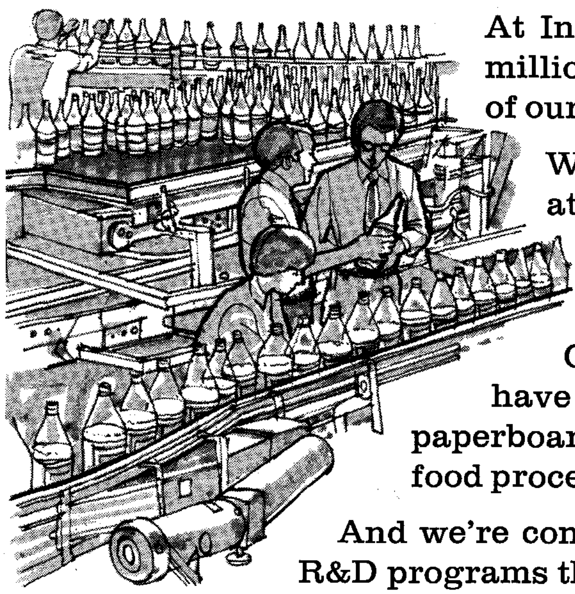

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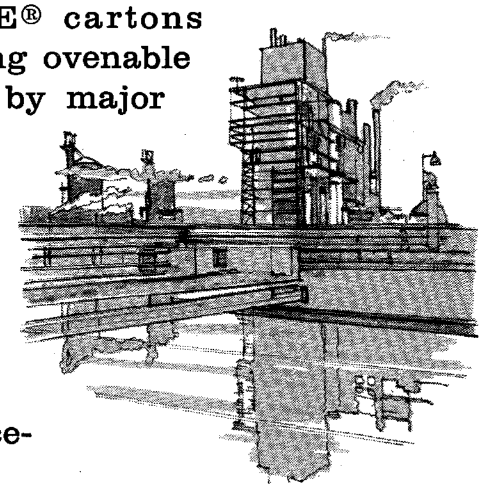

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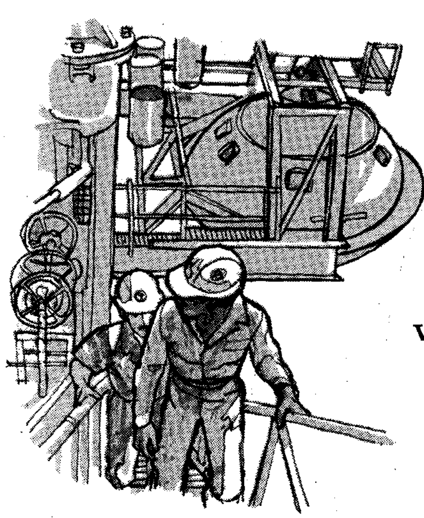

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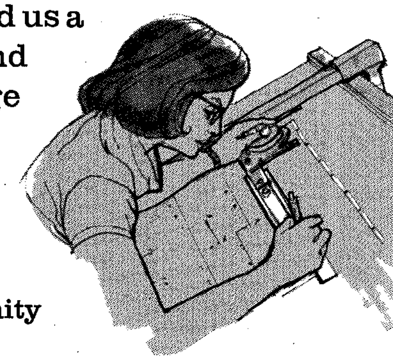

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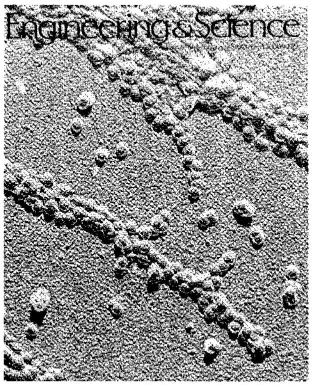


Small Type

On the cover — an electron micrograph of Sindbis virus budding from a chick embryo cell six to seven hours after infection, magnified about 40,000-fold. Sindbis virus is made up of a single thread of genetic material covered with a protein shell, and enveloped in a lipoprotein membrane. This membrane is acquired as the virus buds through the cell surface. Although Sindbis virus is not a human pathogen, it is closely related to numerous viruses causing disease in man, and has been used as a model system for studying both viral replication and membrane biogenesis.

In a Watson lecture last spring, James H. Strauss, associate professor of biology, discussed Sindbis virus as well as many other viruses, and what he and other virologists know — and are trying to find out — about them. "Viruses of Mice, Mosquitoes, and Men: A Primer of Virology" on page 11 is adapted from that talk.

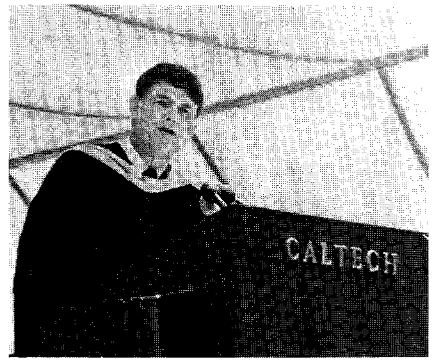
Texas-born Strauss came to Caltech in 1960 to work toward a PhD in biochemistry, which he received in 1967. He then spent three years as a research fellow at Albert Einstein College of Medicine, where he studied the replication and physical properties of Sindbis virus. He returned to Caltech as an assistant professor of biology, and became associate professor in 1975. His chief research interests are in the molecular biology of animal virus replication and the structure of the cell surface and its modification by virus infection.

Life in the lab for Strauss involves looking at a lot of very small things, but for recreation he widens his horizons considerably. He and his wife, Ellen (who is also a Caltech graduate and a member of the biology faculty), are dedicated to the proposition that camping is the best way to go and taking superb photographs of their outings is the best way to prove it.

The China Syndrome

The study of earthquakes and their effects has a long history in China, partly because that country occupies one of the world's most seismically active regions — a fact that has made the Chinese very receptive to the possibility of earthquake prediction and active in finding ways to achieve it. Fortunately, the recent thaw in diplomatic relations between the People's Republic and the United States has made it possible for American scientists and engineers to learn more about what the Chinese are doing and how successfully.

Among the Caltech faculty who have visited China to evaluate not only prediction but also geological effects and construction practices and damages are geologist Clarence Allen and engineers George Housner and Paul Jennings. "The Real China Syndrome: Earthquake Prediction and Engineering in the People's Republic" on page 17 is a review by Dennis Meredith, director of Caltech's News Bureau, of some of their oral and written reports. The article is illustrated with photographs of some of the devastation caused by the Tangshan earthquake of July 1976, the largest earthquake to strike a populated area in the 3000 years of Chinese history.



Commencement 1979

For the last several years those in charge of choosing Caltech's commencement speakers have been reaching into the ranks of their colleagues and coming up with winners — Feynman, Gray, DuBridge, and Delbrück, for example. This year was no exception, with Bruce Murray, professor of planetary science and director of the Jet Propulsion Laboratory, taking over the podium.

Murray has wide-ranging social interests and concerns and considerable ability to express himself. He combined those two attributes in June to analyze what makes Caltech tick as well as it undoubtedly does and what it is going to have to do in the future if it is to continue to function with distinction. "The Challenge of Success" appears on page 6.

STAFF: *Editor and Business Manager* — Edward Hutchings Jr.
Managing Editor — Jacquelyn Bonner
Photographer — Richard Kee

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Don Hartman found a "model" way to troubleshoot the network.

The nationwide telecommunications network carries over 515 million phone calls on an average business day. Only a small number of them run into trouble, such as failing to go through the network, getting noise on the line, or being disconnected prematurely. Craftspeople in Bell telephone companies fix most of these problems quickly. But the causes of some can be difficult to find among one-billion-plus miles of circuits and thousands of switching offices.

For several years the Bell System used its computerized Network Operations Trouble Information System (NOTIS) to try to pinpoint those causes by analyzing trouble reports from all over the country. NOTIS was good. But Bell System managers wanted it to be better, more precise in identifying possible trouble spots. And they wanted the data in compact, easy-to-use form.

We assigned a new employee, Don Hartman, to improve NOTIS. Don came to us with a B.S. from the University of Texas and an M.S. and Ph.D. from Massachusetts Institute of Technology. He and his associates developed a second-generation system (NOTIS II) that does the job superbly.

For the new system, Don developed a mathematical model of the telecommunications network, including 28,000 local and long-distance switching offices and nearly a half-million circuit groups. Don also designed the system software and served as a consultant to the team of Bell System programmers assigned to the project.

Each day trouble reports from the entire country are sent to the NOTIS II center in Atlanta. Overnight, the system analyzes the reports, processes them through the network model, and discerns trouble "patterns" which help identify potentially faulty equipment. By 8 a.m. the next day, via data links, analysts at phone company service centers receive information on troubles traceable to circuits or switching equipment in their territories. Result: Better equipment maintenance. And better service.

With NOTIS II up and running, Don has moved on to other projects. Today he's a supervisor with broad responsibilities for planning the telecommunications network of the future.

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The Challenge of Success

by Bruce Murray

Oftentimes in recent years, Caltech commencement speakers have been members of the Caltech faculty — Feynman, Delbrück, Gray, for example. Each of them has focused on the accomplishments of hard science and the importance of high standards. I would like to complement this approach by focusing on the people of Caltech who practice and teach this hard science, who exhibit those high professional standards, and who carry out research and engineering of unexcelled quality.

Fortunately, I have a most convenient point of reference. At this very locale 13 years ago John Gardner gave the convocation address for Caltech on its 75th anniversary. He had some very thought-provoking words, which I find to be highly relevant to the present. After noting that he had spent the prior 20 years appraising the promise and performance of institutions of all kinds, he said:

> . . . and those years have taught me to give free reign to my gratitude and my awe when I have the privilege of knowing an institution in its moment of greatness.

That phrase — "moment of greatness" — really caught my thoughts and feelings. Indeed, all of us at Caltech have shared the rare privilege of working together in an institution in a moment of greatness; that shared experience binds us together for life.

A commencement ceremony brings together some who are leaving and some who will stay. Those of us who are staying are staying to consume ourselves in the operation of this rare institution. Those who are leaving are leaving to become part of a future at which we can only guess, involved in institutional associations that, in part, haven't even been conceived. But, I think it can be useful for all of us to reflect on Caltech the institution, especially when we consider Gardner's next statement:

> I don't want to alarm you by that phrase "moment of greatness," but in the perspective of decades and centuries institutional greatness is a transitory thing.

So, 13 years later, it seems appropriate to ask: How has Caltech fared since 1966 in character and reputation? Have we remained that unique small place of the highest quality?

In terms of size, the answer is that we *have* remained unique and small. From the end of World War II until the late 1960's there was rapid growth. But the present faculty numbers only about 3 percent more than that of 1966. There are about 10 percent more freshmen and about 13 percent more graduate students, but the total number of degrees being conferred today is about the same as in 1966. The campus staff has not increased significantly in size in the last 13 years. So the campus has changed little in total number of people. There has been a modest increase in the number of employees at JPL, but that number fluctuates due to the vagaries of the space program, and there have been other times since 1966 when a comparison would have shown virtually no change at all.

In terms of the campus's physical plant, there has been a very large growth — about 40 percent increase in building space since 1966. Therefore, the campus is better housed and thus better supported for research and for education than in 1966. In the same interval, JPL space has increased about 17 percent. Over all, the campus's size has remained small, and that of the Laboratory has increased only slightly. Both are better equipped than they were then.

For another basis of comparison, consider faculty reputation and quality. That is a difficult subject to investigate in a precise way, but there are some indicators. For example, members of the National Academy of Sciences and National Academy of Engineering are elected through a very careful national process. In 1966, 32 members of the Caltech faculty, including professors emeriti, were members of the National Academy of Sciences. Today there are 49. In the case of the National Academy of Engineering, the number has gone from 7 to 28.

Another indicator of reputation in basic science is Nobel laureates. In 1966 there were two on the Caltech faculty. There are two now also who are teaching, as well as two others who have only recently retired. If we take the cumulative Nobel laureates — that is, those who have been students at Caltech as well as faculty here at the time they received the award — that number has grown from a total of 11 in 1966 to 17 at this time. I believe that together these figures are valid evidence that the Caltech faculty is unparalleled in research reputation. Personally, I don't know of a better place to practice science. This campus is run by the faculty and, in many ways, *for* the faculty.

Another sign of the greatness of an institution is how it contributes to national leadership. The last 13 years have shown some outstanding achievements. Lee DuBridge retired in 1969 after 23 years as Caltech's President and became Science Adviser to then-incoming President Nixon. The present Science Adviser, Frank Press, spent many years here before moving to MIT. And DuBridge's successor as Caltech's President, Harold Brown, moved on after 8 years at Caltech to become the first scientist ever to head the Department of Defense, making him the second-highest ranking scientist in government. (For the highest ranking scientist currently in government, of course, we have to credit the Naval Academy!) As a matter of fact, that is a pretty scary pattern for ex-Caltech Presidents: Science Adviser to Nixon, Secretary of Defense for Carter . . . Sleep well, Murph.

Over the past 13 years, JPL has also had momentous achievements. In 1966 the first United States unmanned-vehicle surveys of the Moon were being completed, and the first probes to Mars and to Venus had just been accomplished. In the intervening 13 years, Mars has been opened up, mapped, and explored in an extraordinary manner by JPL's Mariners 6 and 7 flybys and by the Mariner 9 and Viking orbiters. The Viking lander was developed by other elements of the national space program, but the entire Viking mission operation was — and is still — run out of JPL.

In the case of Venus and Mercury, JPL's Mariner 10 opened up these planets to initial scientific understanding as well as public enthusiasm and involvement. And, of course, Voyager 1 has passed Jupiter on its way to Saturn, and Voyager 2 encountered Jupiter on July 9th of this year. It too will continue on to Saturn, and, if our luck holds, Voyager 2 may even make it to Uranus. Voyager constitutes an unparalleled mission of discovery. Quite a number of people at JPL have spent many of the last 13 years making Voyager happen.

How about the Caltech students now versus those of 1966? One good thing is that the mix is becoming more representative of our society because in 1966 there were no women freshmen and this year they will be about 16 percent of the class. In the case of graduate students, there were 4 percent women in 1966 and about 12 percent now.

An indicator of student quality is the freshman entrance exam scores. In 1966 they were incredibly high for mathematics, and they remain so. In English there are some fluctuations year-to-year, but the scores remain very high and leave no basis for concern regarding the quality of the incoming class. What is especially significant about the continued superlative performance on the College Board's Scholastic Achievement Tests is that during the same period of time SAT scores of incoming freshmen for all U.S. college entrants have dropped conspicuously in both math and English. Caltech students have not been a part of that unhappy national pattern.

Running down the statistics I've given and reflecting on John Gardner's "moment of greatness" remark, after 13 years we can say that Caltech still is great in terms of student, staff, and faculty quality, in terms of research achievement, and in terms of its national significance. The unique formula of small campus and excellent institution still seems to be working.

As a matter of fact, you might be led to ask almost the opposite question: Isn't it remarkable that Caltech has been so *unaffected* by the turbulent events of the sixties and seventies when such resounding changes have taken place in the United States? And if you ponder that anomaly and then go on a little further in Gardner's speech, you may be troubled by the following words:

> The appearance of greatness is more enduring. Reputation and tradition are effective cosmetics for a fading institution. But what is all too transitory is that fine moment when an institution is responding with vigor and relevance to the needs of its day, when its morale and vitality are high, and when it is holding itself to unsparing standards of performance.

I don't believe anyone who is a part of Caltech can have any doubt about the "unsparing standards of performance." The "morale and vitality" are high, and the "vigor and relevance to the needs of its day" probably are high also; but these things, being more subjective, are harder to assess than standards of performance.

Let me ask my question in a different way: What changes *have* taken place at Caltech in the last 13 years? Well, the faculty itself has remained small, but it's an older one and more settled. The average age has increased from 44 to 47. And if current trends continue, the average age will increase to nearly 49 in the next decade and will not return to its present level until well past the turn of the century. The percentage of tenured faculty increased from 73 percent in 1966 to a high of 84 percent in 1977. It is now at about 80 percent.

How about the formation of new educational and research programs or departments in that time? There was a burst of innovation in the late sixties. The Environmental Quality Lab was started and has become a permanent fix-ture on the campus. (But that "model" hasn't been repeated, even with an important interdisciplinary subject such as energy.) The Social Science program was started also at the eve of the last decade; it's now a widely recognized, highly respected program. Behavioral Biology was started. My own program of Planetary Science was only three years old in 1966, and we had just produced our first batch of PhD's. It is now 16 years old and, as academic programs go, approaching middle age. The 1970's were dominated by the flowering of bold innovations (for Caltech), stemming from the 1960's, along with a modest level of new 1970's innovations such as Computer Science, Applied Physics, and Cell Biology.

The questions in my mind are: Are we ready for another round of real institutional innovation comparable to the EQL and Social Science experiments? Have the necessary ferment and creative thinking taken place or just a continued maturing and aging of existing arrangements?

Another Caltech index about which we are always concerned is the percentage of federal support for the campus budget. In 1966 that was about 60 percent if we take into account all sources of funds. And that was a worrisomely large percentage. Today it is still only about 60 percent. It has gone up and down as various factors have influenced the totals, but now a disturbing trend is setting in. The campus budget is now shifting steadily toward increased percentage of federal support at the rate of one-half percent per year. This is because private endowment and other non-federal funds cannot keep up with the high rates of inflation as well as can the sources of federal funds.

At JPL the average age of scientists and engineers has also increased 3 years — in this case from 38 to 41, although it has now leveled off. The average length of service is about 16 years. JPL still is the center for planetary exploration, but the pace has slowed, and national support is more precarious. This circumstance reminds me of an old cartoon from *The New Yorker* that shows Queen Isabella sitting on her throne. Columbus is kneeling in front of her, obviously pleading for money to buy the ships to search for the New World. Queen Isabella is saying a bit petulantly, "Why do you need three ships to discover the New World? Why won't one ship do?" In fact, the Voyager mission may be the last of the two-ship U.S. missions of planetary discovery. We are down to one ship for the new missions we're planning.

Increased age, increased degree of tenure, increased length of service, all mean that it has been longer since the people who compose Caltech were somewhere else — longer since they acquired experience in different institutional circumstances. Being more settled could mean that there is a greater willingness to accept the status quo and to resist institutional change.

As a matter of fact, institutionally, Caltech is an anachronism. If this campus disappeared for some reason, I think it's doubtful it would be replaced. It was a product of an expanding and optimistic private sector, back in the days when progress was spelled with a capital "P," and Millikan's "truth shall make you free" was implicitly scientific truth. "Free" also implied "happier." If JPL disappeared, it would not be replaced either. It was a product of the partnership between a private university and the federal government to deal with the crisis of World War II and the Cold War days that followed.

In general, throughout the United States, most excellent institutions of science and technology are out of equilibrium with our society. This is because there once was unwarranted American faith in the perfectibility of humankind and its institutions through increased knowledge; that has given way to an unwarranted confusion as to our national and global purposes and to the role of science and technology in them.

Imagine that you are an anthropoid ecologist from Alpha Centauri, visiting the Earth in a disguised form and that you have a special interest in institutions. You might describe Caltech as a "highly specialized colonial organism, invulnerable to external or environmental change because of its extraordinary reputation, and with little evidence of internal motivation for change for the same reason."

Caltech is so concerned with maintaining high standards and quality that it moves in a very conservative path as an institution. But I wonder if perhaps the concern for quality isn't the main problem. Too much emphasis on any single aspect of life — even Vitamin C — can cause side effects, because other "vitamins" may be excluded. Too extreme a regimen may thwart the continual experimentation needed for evolution. Sustained success plus no growth creates a new and unfamiliar threat to Caltech (and to similar institutions). I would ask whether our greatness can continue without institutional evolution. Can evolution occur without institutional experimentation? And how can we have institutional experimentation in an era that affords little or no net growth?

In 1968 Caltech invited John Gardner back as the first recipient of the Robert A. Millikan Award. Again he challenged us to reflect on our institution in terms of the rapid technological and social changes in our country and the world, which he lumped together as "revolutions."

> The swift pace of these revolutions makes it desperately necessary that our institutions be adaptable.

> When they are not, the sweep of events isolates them and dramatizes their anachronistic character. Even institutions that are fairly young, as history goes, find themselves woefully out of date. The rush of change brings a kind of instant antiquity.

These words bring to my mind another of the *The New Yorker* cartoons that say so much. This one shows an ancient landscape with a big adult brontosaurus impatiently explaining to a young, questioning brontosaurus, "Look kid, we're aware of the problems besetting our society. We're workin' on 'em."

What does all this mean for you who are leaving? You're not going to be here, so why should I take up your time talking about family matters? The reason is to urge you to reflect on what you have received from Caltech. The most important part — which you should treasure and nurture — is your ability to actually practice science, the science of Feynman and Delbrück and Gray. You are playing God's game by His rules. You are touching a tiny bit of the fabric of reality itself, and you should try to maintain the fierce standards of Caltech science and apply them to the more complex, amorphous, and critically important problems involving human beings.

But I would urge you not to accept uncritically the rest of the trappings of Caltech. We faculty and administrators are already obsolete in some ways. You must continually renew yourselves to grow beyond us, to be capable of leadership in a world we can't even envision. You must accept the challenge to try to be *complete* intellectuals, not just specialists in narrow parts of science and technology. Most of all, you must not permit yourselves to take refuge in the cultural and social myths, the prejudices, and the unexamined assumptions that we who make up Caltech necessarily exhibit. You must try to separate God's rules — that's science — from man's constraints and myths about "how things ought to be."

But what about those of us who remain? Are we doomed to a gradual decline into a genteel irrelevance? Where will Caltech be on its 100th anniversary, compared to the 75th, in its moment of greatness? I would answer, first of all, by noting that Harvard is entering its fourth century of greatness. So surely we can renew Caltech for a second century if we but accept the fact of our institutional maturity, the possibility of our renewal, and the necessity for the impetus for our change to come from within.

How can Caltech do that? We can do it by renewing and evolving our relationships with our students, our society, and our colleagues. In the area of education — particularly graduate education — we can consider how the ablest graduate students should be challenged in *breadth* as well as depth. For example, we could give special honors and awards for those students who take a PhD in a hard science and a real, meaningful minor in a soft science, or the reverse. There are even more ambitious ways to create unique opportunities and incentives for graduate student breadth if we're willing to take institutional risk.

In the case of undergraduates, our opportunity lies in looking at them in a holistic way. We have to remind ourselves that students learn from each other and from all the other environmental factors in their existence as undergraduates. This consideration suggests greater concern over the educational and emotional significance of the total undergraduate existence at Caltech. It may be that more mixed educational experiences warrant greater emphasis. The 3-2 program — which is in effect but not much used — is one approach. And, we could encourage a year off for industrial experience or to go overseas or even to work at JPL or some place such as that, in order to create a broader set of experiences. Thus we could aim at facilitating a broader total education but still with the same high standards of quality that we now have.

In this regard, I'm encouraged because President Goldberger has expressed as deep a concern as any of the faculty over the quality of our undergraduate education. There is now a new trustee committee on student life. I think these are good omens. I think we have the beginnings of some important change here.

But how about the institution — Caltech itself? In looking outward, we must realize that our partnership with the federal government is mature. There are a lot of frustrations in it. It's not going to grow much larger in real terms, and it is not likely to improve. The new opportunities for Caltech lie in expanding its partnership with the private sector. There has been a very good recent innovation by computer scientists Ivan Sutherland and Carver Mead setting up the Silicon Structures Project in partnership with the manufacturers of semiconductors. In this instance, the industrial sector participates with both money and people, and a way has been found to combine mutually overlapping interests of Caltech with a part of the industrial sector. There is something in it for both sides. I think we have to develop other mutually beneficial partnerships.

Another approach for Caltech is to respond to the new reality for the United States — that technology transfer and development are now paramount matters of national economic security. They are just as serious to us today as was World War II weapons research to our national military security in that time period. In 1941 we had a Pearl Harbor, and, as a result, innovative and unprecedented partnerships were formed between this private university and the federal government. And that proved to be good for Caltech and for the United States. I think we have now entered a comparable era of national challenge. So far, however, lack of a "Pearl Harbor" to energize us nationally has made it much more difficult to make people at Caltech and elsewhere aware enough of the challenge to be motivated to experiment institutionally.

Finally, we must maximize the potential within our institution. Recently, enhanced campus/JPL activities have been developed, especially in astronomy, with the wide-field planetary camera for the Space Telescope, the Infrared Astronomy Satellite, and parts of high-energy astronomy. I think some good things are beginning to happen. But we need to push further, not just in the narrow pursuit of each individual science, but in recognition that the process of working together in different ways is in itself an important form of innovation.

Institutional change, however, ultimately depends on individual change. There cannot be lasting institutional renewal unless the people who make up the institution become renewed themselves. In that regard we have to face a re-examination of the whole concept of tenure and faculty retirement. Traditionally, the purpose of tenure has been twofold. First — and, in my view, most important — is the need to afford protection for talented individuals who should be free to comment on whatever their intellect leads them to, including what their society is doing. These individuals must have protection from political and social harassment, which is likely to arise if they are saying unpopular things. In my view, freedom of expression is the unique attribute of a modern university. However, it does not automatically follow that the only way it can be accomplished is by guaranteeing a person a job "for life," which can mean 35 or 40 years.

The second purpose of tenure traditionally has been to afford economic stability to professors in research environments so they can carry out creative research without having to be overly concerned about their salary support running out when a particular contract ends.

Financial stability is very important, but I suspect it too can be accommodated with something less than 35- or 40-year guarantees. For example, 5-year rolling contracts might afford a realistic alternative. It is most important that tenure be used only for the right reasons and not appear to reflect a guild or elite mentality.

The tenure system affords a *disincentive* to change — both institutionally and personally — especially when combined with our faculty retirement system. Our present retirement system is satisfactory if one lives beyond 68 and can use it, but it provides very little money until then. That mitigates against mid-career changes by our faculty. Most professors don't have enough personal net worth to easily permit mid-career changes. It's not at all clear to me that these circumstances work in the best interests of Caltech or of the individual faculty members. It denies the freedom of choice that individuals of comparable ability have in business, government, and many other professional activities. (Of course, aging professors can always be farmed out to head JPL or places like that, but we haven't that many JPLs!)

We also have to face emerging issues on the role of our research personnel who are not part of the teaching staff. How do they relate to the institution? Is the relationship a marriage, a friendship, or a transitory acquaintance? These are some tough issues that must be faced because of the national need for continuing research of the highest standards and of substantial volume here at Caltech even though there is little prospect of net growth and, therefore, new individual opportunities for some who are here on research appointments.

But Caltech has tremendous reputation and quality, so it can afford the risk of innovation. It has always been well supported because it has been unique and, thus, attracted special treatment making it possible to have an incredibly low student-to-faculty ratio here, unlike anywhere else in the United States. But we must seek our new institutional uniqueness, not merely presume that past patterns of support and activity will continue unchanged.

In summary — for those who are staying as well as those who are leaving — we all must aspire to be an elite of performance, not privilege, and to become part of the community of quantitative intellectuals, most of whom have not even yet been born. Real limits to growth are being reached globally: The whole world has to come to terms with a steady state rather than an explosively growing circumstance. Our historic circumstance is to live at that singular period when man the toolmaker faces his most promising and yet his most dangerous era, when his technical powers outstrip his social structure. Our chosen destiny must lead toward an era when quality will again rule over quantity; when man's incredible potential for greatness and achievement can unfurl free from the self-destructive tendencies so evident today.

Our primary institution is not Caltech, it is planet Earth. Our primary constituency is not our fellow scientific colleagues, it is *Homo sapiens*. All of us, those who are leaving and those who are staying, must together accept the challenge of continuous renewal so that our institutions, indeed, our world, can evolve rapidly enough to succeed. The future deserves nothing less. Godspeed, graduates! □

Viruses of Mice, Mosquitoes, and Men

A Primer of Virology

by James H. Strauss

A Caltech biologist reviews the structure, assembly, and replication of viruses – and the nature of research into ways to control them

All of us have been exposed to viruses at some time in the past, usually with unpleasant consequences. However, there is also a less personal view of viruses, the science of virology, which is the study of what viruses look like, how they assemble, and how they replicate. Virologists hope that an understanding of viruses will eventually lead at least to an amelioration and perhaps to eradication of viral disease.

First of all, viruses are very small, simple creatures that can infect either animals or plants or bacteria — and in some cases more than one of these groups of living things. They possess a single species of nucleic acid (either RNA or DNA but not both) containing the genes of the virus, the units of heredity that direct its replication. Wrapped around the nucleic acid is a protein shell that protects the virus and its genes as it moves through the universe looking for a person or a cell to infect. Having found a susceptible host, the protein shell carries out its second function, getting the nucleic acid inside a cell. Once inside, the nucleic acid goes about the business of a virus, that is, replication. The nucleic acid takes over the metabolic machinery of the host cell, an act that leads to the production of more virus particles rather than more cells.

Because a virus can only replicate within a living cell, the question has often been asked whether or not the virus itself is alive. The answer is semantic. If "alive" describes an organism that can replicate itself and has an independent evolutionary history, then clearly viruses are alive. If, in addition, "alive" implies metabolism and the production of energy, then viruses are not alive. A virus replicates but does not metabolize.

The very simplest (and smallest) viruses can code for only three genes, but with these three genes (and, of course, the entire host cell) they can manufacture replicas of themselves. The largest and most complex viruses contain as many as 200 genes, which is still very few compared to other living creatures. For example, a bacterium, a single microscopic cell, contains a few thousand genes. A mosquito has 100,000 genes, and a man carries several million. It is interesting to compare the size of a bacterium to that of a virus that might infect it. Bacteria, which are visible in the light microscope, are on the order of 2 or 3

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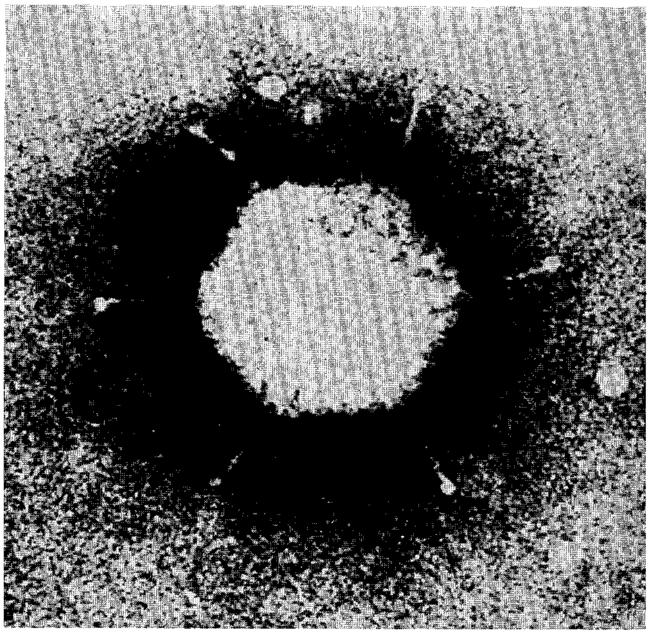
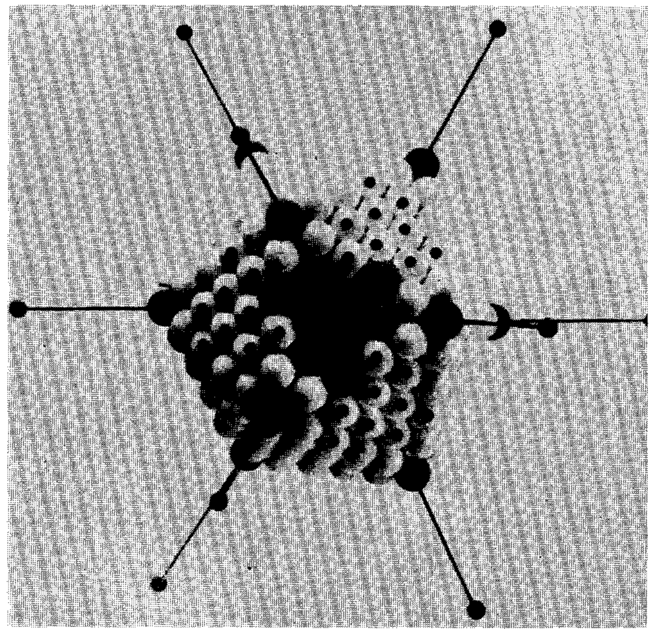
From Valentine and Pereira, *Journal of Molecular Biology* 13 (1965): 13. Courtesy Dr. R.C. Valentine.



An adenovirus, which causes respiratory disease in man, magnified 500,000 times in the electron microscope.



A model of the structure of an adenovirus, an icosahedron with fibers at the 12 vertices. Many animal viruses have icosahedral symmetry.

microns in length. There are 25,400 microns to the inch. Viruses are many times smaller, ranging between 20 and 250 nanometers in diameter. A nanometer is a thousand times smaller than a micron, making 25,400,000 nanometers to the inch. Anything this small is not visible in the light microscope, and it is only since the development of electron microscopy that virologists at last have a means of making viral structure visible.

WHAT DO VIRUSES LOOK LIKE?

Viruses are relatively simple in construction, because they have a limited number of protein molecules with which to form a protective shell around the nucleic acid. Many identical copies of only a few protein types are used, and their placement around the nucleic acid follows certain geometric principles. One popular model for virus construction is the icosahedron, a Euclidian solid that has 20 faces, each of which is an equilateral triangle. (A more familiar example of icosahedral construction is the geodesic dome.) Two common icosahedral viruses infecting humans are poliovirus (the infectious agent in infantile paralysis) and adenovirus (which causes a respiratory infection and was first isolated from human adenoids).

Another model for virus construction is the helix, in which the protein subunits are wound around one another like the steps of a spiral staircase. The helix is coiled very tightly, so that from the outside a helical virus looks like a long rod. One commercially important helical virus is tobacco mosaic virus (which infects plants of the nightshade family, especially tobacco).

A number of viruses go one step further and have a lipid-containing envelope, a membrane, wrapped around the protein-nucleic acid core, which in these cases can have either icosahedral or helical symmetry. These enveloped viruses include the herpesviruses, such as herpes simplex virus, which causes fever blisters; genital herpes virus, which causes similar eruptions on the genitalia and has been implicated in cervical carcinoma; and varicella virus, which is responsible for chicken pox. Influenza virus, mumps virus, rubella virus (German measles virus), and a host of other human viruses are also enveloped.

Another group of viruses has a curious bullet-shaped structure — round on one end and blunt on the other. These viruses have a modified helical core in which the helix is wound into a very tight coil like a ball of string, and is surrounded by a lipid envelope. Two such viruses are rabies virus and vesicular stomatitis virus, a virus of cattle and pigs that has been extensively studied in the laboratory.

In addition to the animal and plant viruses that have either helical or icosahedral symmetry, there are a number of bacterial viruses that have an even more elaborate structure in that they have a tail. Max Delbrück pioneered the study of these tailed bacteriophages, as they are called, here at Caltech 40 years ago. Bacteriophages have a head with cubic symmetry properties (usually roughly hexagonal in outline but not strictly icosahedral) and a tail with helical symmetry. At the end of the tail may be found a base plate with long fibers attached to it. They are much more complicated in their construction and have many more types of proteins than the other viruses I have discussed.

HOW DO VIRUSES REPLICATE?

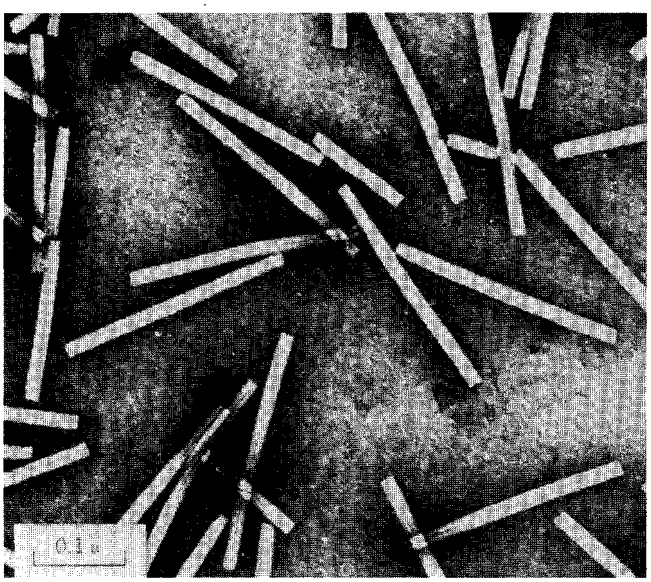
From the point of view of the virus, its sole function in life is to persuade a cell to make many more viruses just like itself. To do this, the first step is to ensure that the viral nucleic acid and its genetic information are introduced inside the host cell.

In the case of the tailed bacteriophages, the virus attaches to the bacterium through its tail, and a contraction of the tail forces the nucleic acid inside the cell. Animal viruses, however, do not have tails and have evolved other strategies to enter a cell. In one strategy the virus attaches to a specific receptor on the outside of a cell, and then the cell engulfs it and takes it inside. Cells normally take in a variety of substances, such as metabolites, and respond to molecules attached to their surfaces like chemical stimulants and hormones. The virus probably makes use of these mechanisms and tricks the host into internalizing it. Enveloped viruses, on the other hand, can enter a cell by fusing their lipid membrane with the lipid membrane of the cell, which puts the core into the cellular cytoplasm. In this case also the virus first attaches to a specific receptor on the cell surface before entry.

Once a virus's nucleic acid gets inside a cell, it can begin to replicate, and that's the crux of the whole issue — how does a virus make more of itself? In short, the virus introduces a few new genes inside the cell, and these new genes take over the host cell's metabolic machinery, causing the cell to reproduce the virus instead of reproducing itself. A cell that literally is performing millions of functions before infection stops doing all the things it normally does and instead produces hundreds of thousands of virus particles. The time required for virus replication depends upon the virus and the cell. For bacterial viruses, 30 minutes suffices. For animal viruses, hours and even days are required.

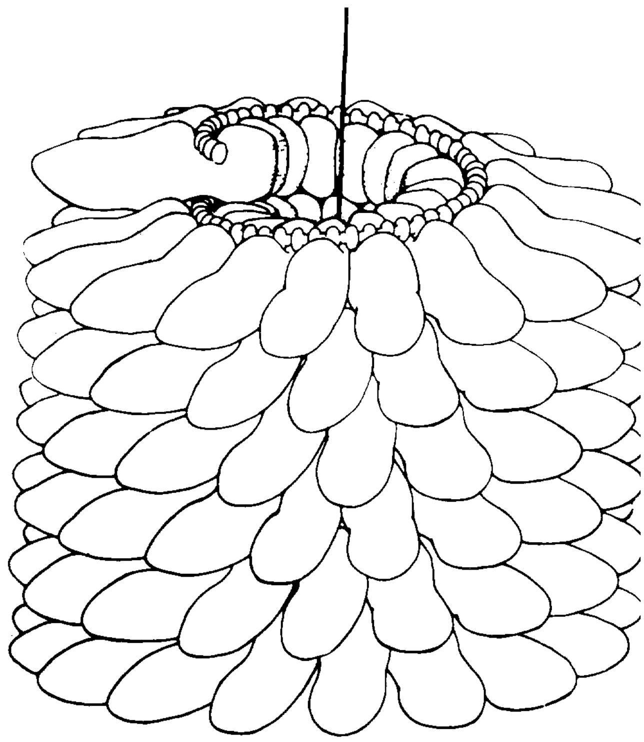
As an example of virus replication, consider Sindbis

From *General Virology*, by Luria, Darnell, Baltimore, and Campbell. Original courtesy of J. T. Finch.



In the electron microscope, tobacco mosaic virus particles appear as rigid rods with a hole down the middle. These viruses are magnified about 150,000 times.

From *Viral and Rickettsial Infections of Man*, Horsfall and Tamm, eds. J. B. Lippincott Company, 1965.



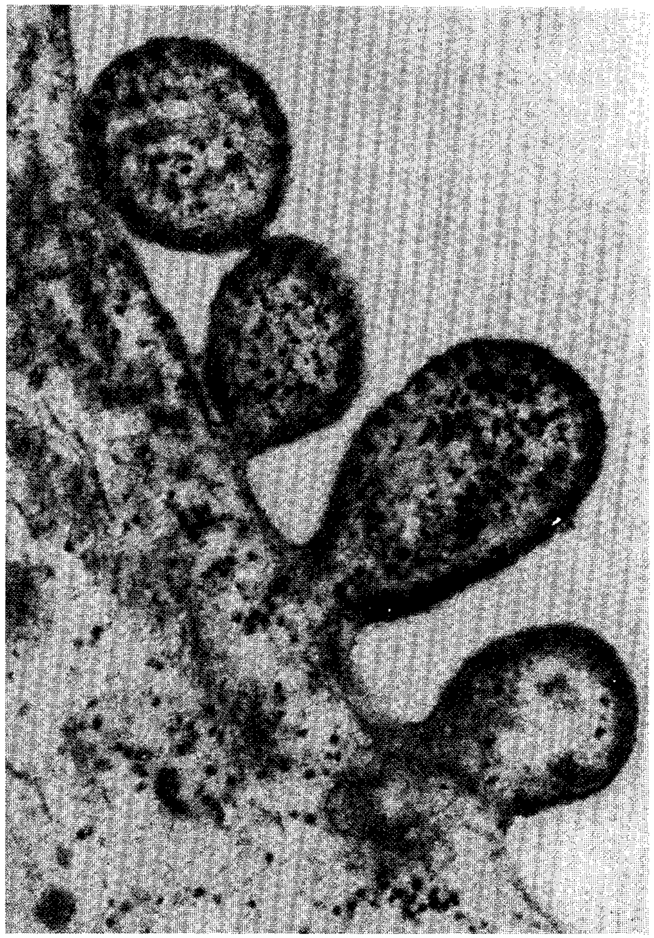
A model of the structure of tobacco mosaic virus shows an inner coil or helix of nucleic acid, encased in globular protein molecules that are arranged like the steps of a spiral staircase. This drawing shows about 1/20th of a full-length virus rod.

A Primer of Virology

virus, a virus that has been intensively studied in my laboratory for several years. Sindbis virus, named for the town of Sindbis, Egypt, infects a wide range of animals in nature, including mosquitoes, birds, and man. In the laboratory we study the replication of the virus in tissue culture cells derived from chickens, hamsters, mice, monkeys, or mosquitoes. After infection, the RNA of the virus goes to the ribosomes of the host cell, which are the factories for protein synthesis inside a cell. There the RNA is translated into protein. These new proteins are able to replicate the viral RNA, specifically using that viral RNA as a template to make new copies of it. In addition, a second virus messenger RNA is produced and translated into a second set of proteins — those that make up the protein shell of the virus particle. These proteins assemble with viral RNA to form progeny virus.

All viruses replicate by these methods. Messenger RNA is made and translated into proteins. These proteins are of two types: those that replicate the virus's nucleic acid and those that make up the structure of the virus particle. If you have a DNA-containing virus rather than an RNA-containing virus, you simply have one more step. The viral DNA must replicate, and it must be transcribed into messenger RNA.

From *General Virology*, by Luria, Darnell, Baltimore, and Campbell. Micrograph courtesy of S. Rozenblatt and C. Moore.



Measles virus budding from the surface of an animal cell is magnified about 500,000 times. The helical nucleocapsid migrates to the surface of the cell and buds out as a completed enveloped virus.

HOW ARE VIRUSES RELEASED FROM THE INFECTED CELL?

Once a virus has infected a cell and replicated there, it must get outside so it can find another cell to infect and start the cycle all over again. There are two ways for this to happen. One is a process of lysis, or cell destruction, in which the cell simply falls apart and releases its contents. In the case of bacterial viruses, lysis is an active process. The viruses actually make something that causes the cell to disintegrate. In the case of animal viruses, usually the virus simply kills the cell by causing it to stop growing. As soon as the cell's normal maintenance functions are no longer performed, the cell dies, disintegrates, and spills its contents into the medium.

In addition to lysis, viruses can get out of the cell through the process of budding. Sindbis virus buds, and my laboratory has taken advantage of this to study the processes of membrane structure and assembly. When viruses bud out, they may kill the cell, but they don't have to. Instead, they may set up a persistent infection. Perhaps the classic example is rubella virus. This is the well-known German measles virus that causes developmental abnormalities in a fetus when a woman is infected with it within the first three months of pregnancy. It is thought that maybe the virus causes developmental abnormalities because it grows in the embryo without killing the cells that it infects. Perhaps because the cells are virus-infected, they may grow more slowly or not divide at the right time. Perhaps because the virus is budding, new surface proteins — antigens — are introduced into the surface of the cell and cause the embryo to develop improperly.

THE EPIDEMIOLOGY OF VIRUS INFECTION, OR HOW DOES A VIRUS GET FROM HOST TO HOST?

Getting the virus outside the cell is not the end of the story, obviously, because in order to continue its lifestyle, the virus must find another living organism to infect. If we're talking about a human virus, it must get from one human to the next, and there are a number of mechanisms by which it can do so. One is the oral/fecal route. This usually involves viruses that grow in the gut, which are spread by contamination of food or water supplies. Polio and hepatitis are examples of these kinds of viruses.

A second method of getting around makes use of blood-sucking arthropods — mosquitoes, ticks, and the like — as an intermediate. There are quite a number of viruses that are able to grow in humans or in other higher vertebrates and that are also able to grow in mosquitoes or ticks. Sindbis virus is such a virus. An infected mosquito transmits the virus when taking a blood meal. The virus then grows and circulates in the blood of the vertebrate and can be in turn transmitted to an uninfected mosquito. In nature these viruses thus alternate between vertebrate and invertebrate hosts. It is interesting that there are viruses of plants that have the same sort of life cycle. They can grow in both plants and in insects that feed on plants, such as leafhoppers and aphids.

A third method for getting around, which is used by respiratory viruses, is human contact. These are the viruses we know most about in personal terms. The simple act of breathing excretes cold viruses, influenza viruses, and other respiratory viruses because they grow in the upper respiratory tract. Since these viruses are likely to cause coughing and sneezing, the efficiency of their transfer from one host to another is often increased.

We usually think of respiratory viruses as being those that spread chiefly in wintertime. The very name "cold" implies that. It turns out that probably colds are more numerous in wintertime *not* because people are more susceptible to them then, and *not* because the weather is cold, but simply because human beings behave differently in the winter than they do in the summer. They stay indoors more, under more confining conditions, and so they are exposed to viruses more. Conversely, there are viruses, such as poliovirus, that are epidemic in the summer.

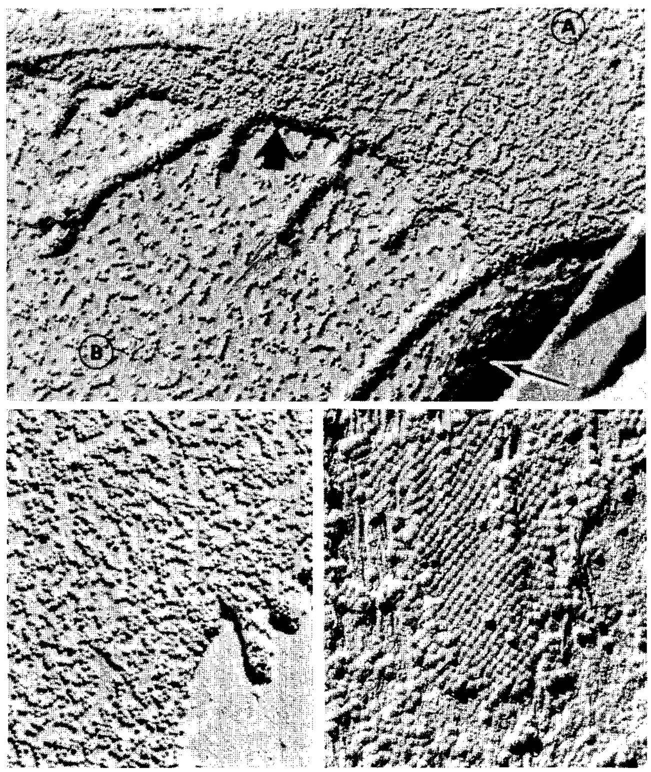
WHAT CAN WE DO TO CONTROL VIRUS INFECTIONS?

The first line of defense against virus infection is the body's immune system. When the body is infected by a foreign agent like a virus, it responds by producing antibodies against the virus in order to inactivate it. It's been known for centuries that infection with, say, smallpox — if a person survives the disease — confers permanent immunity. This principle has been used as a method for preventing viral diseases for 200 years, since Jenner reported that contraction of cowpox, which causes an innocuous infection in humans, confers immunity to smallpox. The recent campaign by the World Health Organization to vaccinate the entire susceptible population of the world has succeeded in eradicating smallpox as a disease agent in humans.

Many other viruses have now been controlled through immunization — polio, measles, mumps, and so forth — using either live virus or killed virus vaccines. Prior to 1964 there were on the order of 100,000 cases of measles in this country every year. In 1963 measles vaccine was licensed, and a mass eradication program was initiated in 1966. It has succeeded in virtually eliminating measles virus as a severe pathogen in the United States.

One virus that has resisted immunization up to now is that of influenza, because the virus changes very rapidly. The surface proteins of a virus, the antigens, are what the body's immune system recognizes, and these mutate when the virus replicates. As these proteins become less related to what they were through mutation, it becomes harder and harder for the immune system to recognize them and so to

From "Distribution of the Receptor Sites for Sindbis Virus on the Surface of Chicken and BHK Cells," by Charles R. Birdwell and James H. Strauss. *Journal of Virology* 14 (1974): 672-678.



Sindbis virus adsorbed at 4°C to the surface of chicken cells in culture. In the upper panel the solid arrow and the black-and-white arrow indicate the edges of cells A and B, respectively. Each cell has receptors for many thousands of particles, but not all cells have the same density of receptors. When adsorption is performed at this temperature, the virus tends to cluster on the surface (lower left), at times forming paracrystalline arrays (lower right).

successfully counteract the virus particle. Thus there are recurrent epidemics of influenza, and no vaccine can be introduced that is effective for more than a year. Last year's vaccine doesn't work against this year's virus.

Every so often, every ten years or so, there is a dramatic change in the antigenicity of these proteins that is thought to occur by means of recombination between a human influenza virus and an influenza virus of other animals. When this change takes place, no one is immune to the new virus, so it sweeps through the human population with the speed of jet travel, causing literally hundreds of millions of cases of influenza.

It is important to remember that because all viruses do mutate they will adapt to changing circumstances, so we can't become completely complacent about the fact that a number of virus diseases have been controlled by our having a vaccine against them. It's always possible that new virus strains will arise in nature and infect the human population. For instance, even though smallpox virus has been eliminated, there are lots of other pox viruses in nature — pox viruses of monkeys and other primates as well as many other animals. It's always possible that variants of monkey pox could arise, which could infect humans and spread. So it's necessary to continue our efforts to learn as much about the disease agents as we can.

A second approach to the control of virus infections is to try and find some chemical that suppresses virus growth. We are all aware of the tremendous success achieved in controlling bacterial infections with antibiotics. Pneumonia, for example, was once a great killer, but is now easily controlled. Antibiotics are compounds that are for the most part produced by molds as a form of chemical warfare against bacteria, which share the same habitat. But antibiotics make use of the fact that a bacterium is a free-living organism whose metabolism differs in many ways from that of a mold or from that of humans. The antibiotics are directed against that different aspect, so that you can in effect poison the bacterium without poisoning a human or a mold.

The concept of an antibiotic is very different for a virus, which replicates inside the cell as an integral component of it. The only promising compound for the suppression of virus growth that has been identified so far is a protein called interferon. Interferon is synthesized by animals in response to virus infection, and it does inhibit the growth and replication of many viruses. The fact that people don't make inteferon all the time but only in response to virus infection probably means that the compound is not completely atoxic. It represents an emergency response to a virus infection in order to try to control it until the body's immune system can take over. But if human interferon were available in quantity, its potential for the control of virus disease is enormous.

A number of clinical studies have shown that interferon has great potential also as a therapeutic agent in treating certain cancers. These studies indicate that interferon is useful in treating osteosarcoma, a form of bone cancer, and multiple myeloma, a cancer of the immune system.

The problem has been to get enough interferon to study. For one thing, only human interferon is effective in humans. For another, it is produced in exquisitely minute quantities, as is shown by the fact that the cost of interferon for the clinical trials referred to above was about $70,000 per patient. Getting enough of the material even to study its chemical properties has proved all but impossible up to now, but perhaps with the development of new microsequencing techniques in the laboratories of Leroy Hood and William Dreyer at Caltech it may be possible to learn more about the chemical structure using the small amounts of material that are available.

One promising approach to obtaining more human interferon for study makes use of the new techniques of recombinant DNA technology in the cloning of genes. A number of laboratories, including some at Caltech, are cloning human genes, which simply means taking a small part of a human chromosome and amplifying it enormously in bacteria. The purpose is to get enough material to study and thus try to learn what the structure of the human chromosome is. These studies may make possible the treatment of many human diseases, such as thalassemia (a form of anemia). In the case of interferon, it may be possible to clone the human interferon gene and use the bacteria as a factory to produce enough interferon for study and perhaps for therapeutic treatment.

VIRUSES AND CANCER

It is known that some human cancers are caused by viruses. For instance, a certain rare form of nasopharyngeal cancer is probably caused by a virus called Epstein-Barr virus, which also causes mononucleosis. It is probable that human cervical carcinoma is caused by a strain of herpes virus. It is also known from certain animal studies that leukemias are caused by viruses that are infectious agents. These viruses can spread the disease — through the cat population, for example. For this reason and for many others, a continuing study of viruses should lead to improved human health. It may also help to satisfy human intellectual curiosity, specifically about the lifestyle of these fascinating creatures called viruses, and more generally about the nature of living processes. □

The offset in this row of trees is due to horizontal tectonic movement in the Tangshan earthquake in northern China on July 27, 1976. The magnitude 7.8 earthquake, the largest in Chinese history to hit a populated area, exhibited fault movements of up to three meters. This picture, and all the others in this article, were given to members of the National Academy of Science's Earthquake Engineering Delegation by Chinese seismologists and engineers.

The Real China Syndrome

Earthquake Prediction and Engineering in the People's Republic

The Real China Syndrome

China, the world's most populous country, is also among the most earthquake prone. According to historical records, at least 345 earthquakes greater than magnitude 6 have rumbled beneath the land over the past 2,500 years, wreaking terrible death and destruction. Until recently, however, international politics have made it difficult, if not impossible, for the outside world to study Chinese earthquakes. But since the beginning of the thaw in relations between the United States and the People's Republic of China, visiting American scientists have been able to glean more and more useful information about the fascinating scientific puzzle of the tremors that vibrate along the spiderweb of faults beneath China.

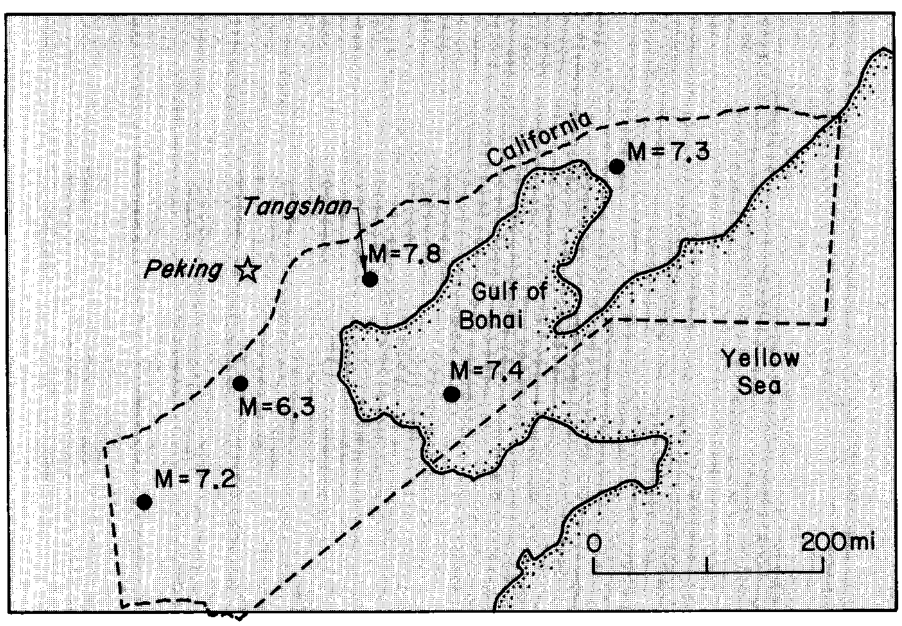
Just as fascinating, they have been able to observe the phenomenon of China's massive program of earthquake prediction. It's an amalgam of science, politics, public relations, and mythic folklore as revealing of Chinese society as it is of the country's geology.

Prominent among the observers of China's prodigious earthquake prediction effort have been seismologists and earthquake engineers at Caltech. Professor of Geology and Geophysics Clarence Allen, who visited China in 1974 with a group of seismologists, has embarked on another trip this fall. And George W. Housner, the Carl F Braun Professor of Engineering, led a delegation of earthquake engineers to China in the summer of 1978 — the National Academy of Sciences (NAS) Earthquake Engineering Delegation — which also included Professor of Civil Engineering Paul Jennings. The NAS will publish a report of the findings of the group in November.

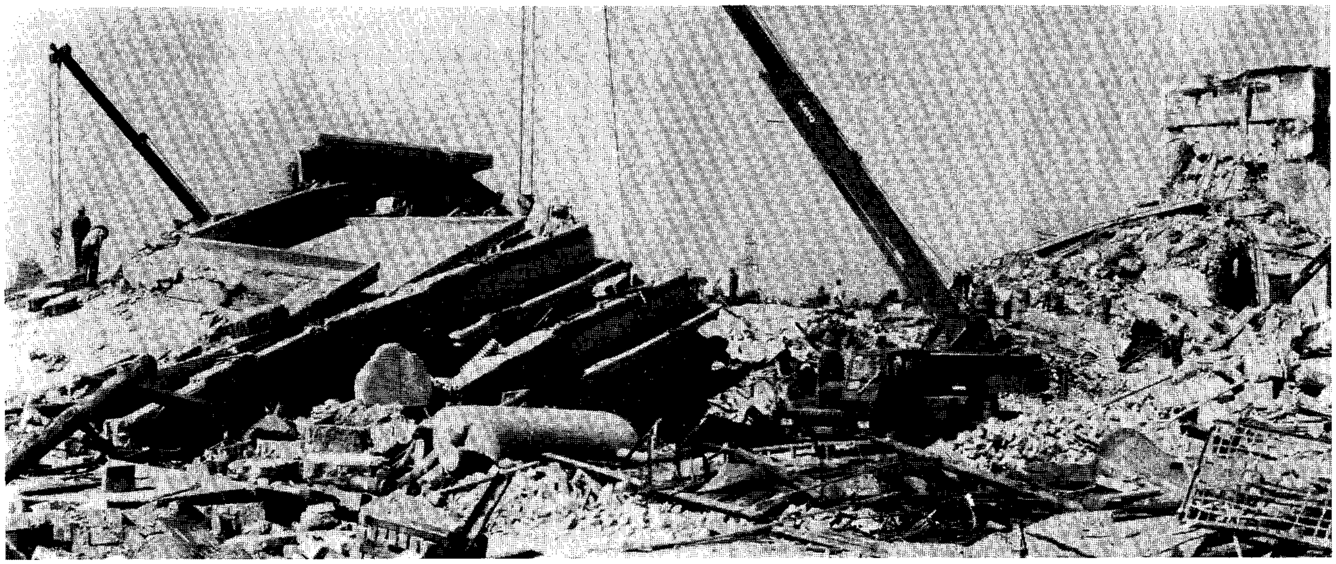
According to the NAS report, earthquake prediction in China is very much a "people's science," involving about 10,000 workers in the provincial seismological bureaus, of which about half are technically educated. However, many times this number of amateur seismologists throughout China gather the masses of data used in prediction studies. These amateurs work with the provincial seismological bureaus and brigades, carrying out their earthquake activities in addition to their regular work as farmers, tradesmen, students, and the like. They work in over 10,000 mass observation posts, gathering an incredible range of data on every natural phenomenon that could possibly pertain to earthquakes. Some of the measurements made by the provincial seismological bureaus are the same as those made by U.S. seismologists. For example, records are kept of seismic activity, velocity of seismic waves through the earth, magnetic fields and electric currents in the earth, groundwater levels, and changes in the amount of radon in groundwater.

Other phenomena carefully monitored by the Chinese have received little attention from American seismologists. According to the NAS report, among the records kept by the Chinese in the case of one earthquake prediction were:

- Lights and outgassing seen prior to the earthquake, in the form of columns, fans, balls, and sheets of fire. For



This map of the Bohai region of northern China gives the epicenters and magnitudes of major earthquakes that have taken place in that area since 1966. The dotted outline of the state of California is superimposed to show that within less than 15 years four earthquakes of magnitude greater than 7 and one of more than 6 took place in a region approximately the same size as California.



This building of the Kailuan Coal Mine General Hospital in Tangshan collapsed in the earthquake. Because of a failure to anticipate the possibility of such a large quake, few buildings in the city were designed to be sufficiently resistant to shaking. Thus, 85 percent of the 916 large buildings in Tangshan collapsed or were severely damaged, and only one percent escaped damage.

example, before one earthquake, the head of the provincial seismological bureau described seeing a fireball 75 kilometers from the epicenter. The fireball reportedly originated at the ground surface 100 meters from where he stood, shot upward and began shrinking, and then curved over, falling to earth. The light dimmed and brightened, and small wisps of white smoke swirled around it. A slight crackling sound was heard, and an odor of garlic or sulfur was detected.

- Earthquake sounds, which in China are called "sounds of the mountains." Experience with these sounds, commonly low rumblings coming in short bursts, is so extensive that people often leave their houses upon hearing them.

- Abnormal growth or withering of plants. Before this earthquake, fruit trees bloomed twice in one season and bamboo withered, the latter apparently because of a drop in groundwater.

- Nausea and shock before the earthquake. Scientists suspect this phenomenon, if true, may be due to an increased level of microseismal activity.

- Unusual animal behavior before an earthquake. Also studied in the U.S., this has been a part of the Chinese folk wisdom for so long that most of the Chinese scientific community seems to accept it as a fact. The Chinese have even constructed tables that purport to list the period of time before an earthquake during which, for example, dogs, fish, or snakes exhibit strange behavior. Examples of strange behavior include an unusual number of rats on telephone or power lines, or rabbits climbing thatched roofs to escape the ground.

Although much of the reported animal behavior does have some conceivable scientific explanation, other instances are far less obvious.

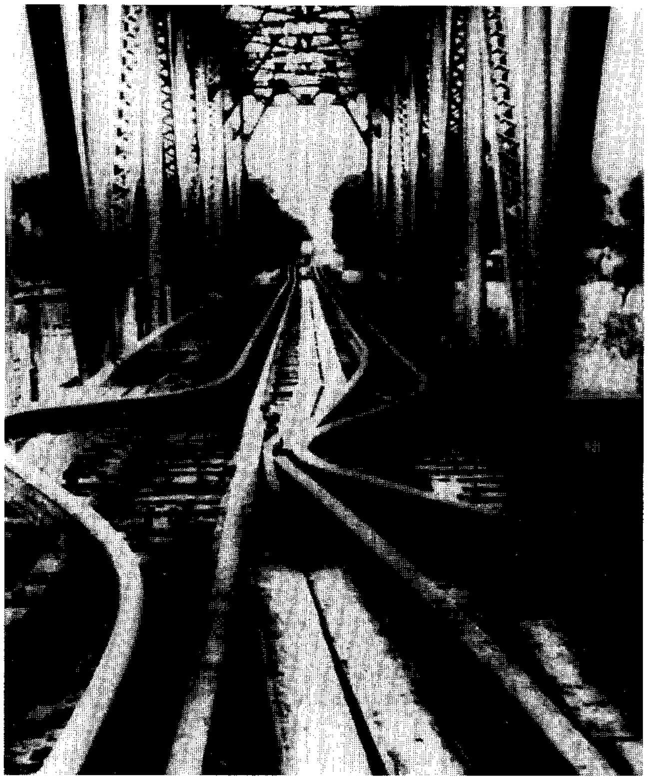
"It was reported to us that the tiger in the Tientsin zoo became lethargic a few hours before an earthquake in 1969," says Clarence Allen, "— so lethargic that the zookeeper finally called the local earthquake prediction brigade and warned them of an impending earthquake, which subsequently occurred.

"In general, though, we came away from our tour with the impression that, even if perhaps 95 percent of the work going on in China may not ever yield useful results, the other 5 percent represents a far more massive effort than is going on in all the rest of the world combined."

But why such a huge grass-roots effort?

"First of all," Allen says, "earthquakes are a very real problem in China — much more so than in this country. They have had major disasters for thousands of years.

"Furthermore, earthquake prediction in China can save lives. The great numbers of Chinese killed in earthquakes have been killed in their own homes — particularly in the



Steel rails on the railway bridge across the Ji Canal buckled during the earthquake. Soil spreading caused inward movement of both bridge abutments, shortening the distance between them by 2.1 meters and causing the distortion of the rails.

countryside where the homes are built of tamped earth. In this country, for the most part, family dwellings are among the safest places one can be. Consequently, there is not the benefit associated with prediction that there might be in China."

And finally, Allen points out that the social system in China permits leaders to take strong action with less worry about the consequences of public reaction.

"If the chairman of the local committee says evacuate the village — by God, they evacuate. If Mayor Bradley were to order evacuation of parts of Los Angeles, I don't know what would happen."

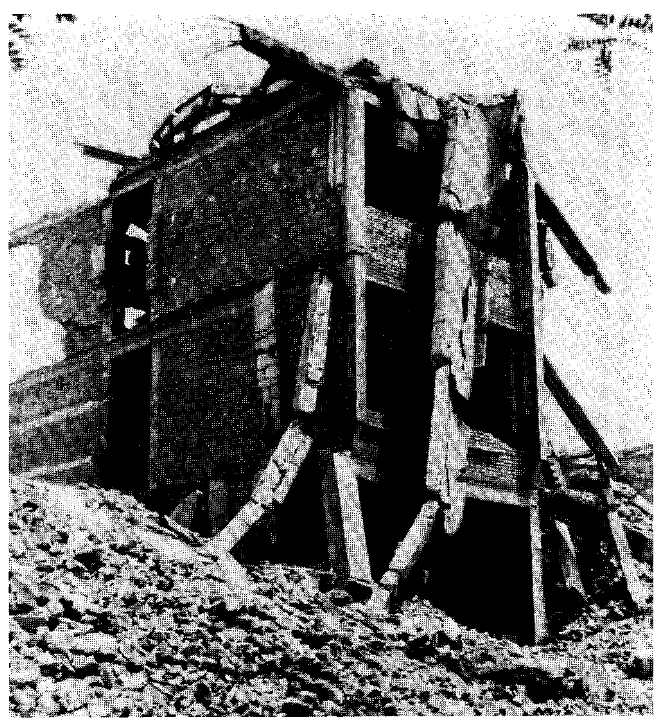
George Housner notes a strong public relations element in the Chinese effort.

"Some of this enormous public effort is also aimed at taking the heat off the government," he observes, "— to show that they are doing something about the very real threat of earthquakes."

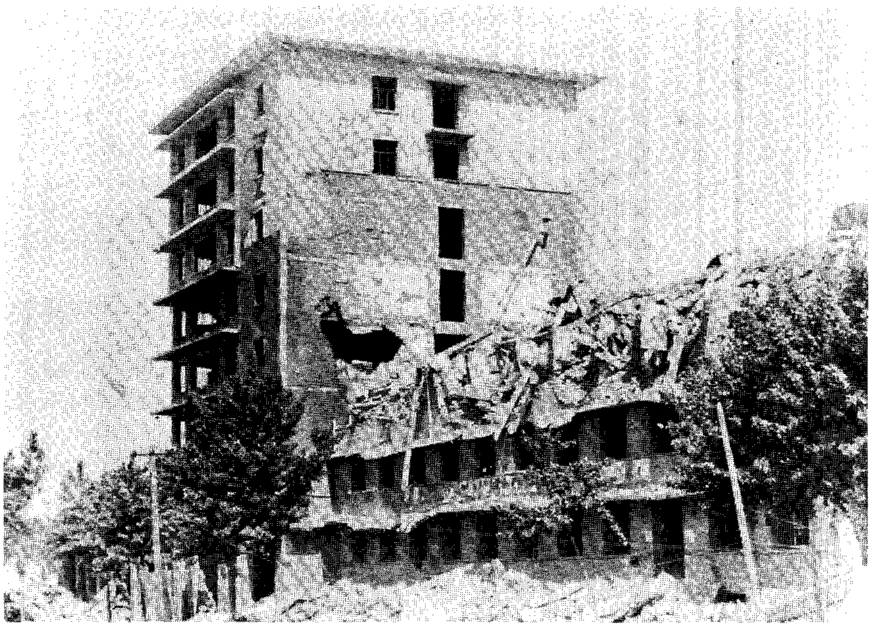
During their visit, Housner's NAS group was briefed on how the Chinese predicted a series of three large earthquakes, around magnitude 7, that occurred in Szechuan Province in August 1976. The briefing revealed how the massive state bureaucracy turns itself to the task of issuing a prediction.

The prediction process began with data from routine field monitoring about six years prior to the earthquake. Seismologists' studies of historical data showed that whenever a moderate or large earthquake occurred in one fracture zone of the area (called the Lungmenshan fracture zone), it was usually followed by a similar event in the Sungpan area of Szechuan. So on February 24, 1970, when a 6.25 earthquake occurred in the Lungmenshan area, an extensive program of seismic monitoring was begun. In November 1975 seismological workers submitted their opinion, based on this work, to the State Seismological Bureau in Beijing, concluding that an earthquake of greater than magnitude 6 might happen during the next six months in the Sungpan-Mouwen region. They had observed changes in the pattern of seismicity, in the velocity of seismic waves through the earth, in the tilt and level of the ground, in the radon gas content of groundwater, and in groundwater levels.

Later, in January 1976, this opinion was confirmed, and even closer monitoring was begun. In June 1976 further



The reception building of the Kailuan Coal Mine also collapsed.



The eight-story Hsin-Hua Hotel was badly damaged at the fifth-story level, but it remained standing. In the foreground is a six-story building that collapsed completely. The behavior of soft soil had an appreciable effect on the damage sustained by structures; that is, buildings located on thin soil, on thin layers of rock, or on thin layers of soil over rock did not exhibit the same degree of severe damage as buildings located on less firm soil.

data led seismologists to submit another report that stated there was a high probability of a greater-than-magnitude-6 earthquake in one to two months.

Following discussion and debate, the State Seismological Bureau gave permission to alert the local government, and the provincial revolutionary committee issued an "urgent announcement" that the earthquake was expected. In the affected regions, anti-earthquake commands were established, and professionals converged on the suspected source region with a variety of instruments. Along the Lungmenshan fracture zone, the number of observation posts manned by the masses increased rapidly from 280 in 1975 to 4800 just prior to the earthquake.

Finally, the accumulation of new data prompted the issuance in early August of earthquake reports stating that around August 13, 17, and 23 there would be earthquakes of greater than magnitude 7 in certain areas. Specific formulas provided the basis for predicting the dates and areas, but the NAS group was not given details. Apparently, though, the formulas were rules based on past experience that related a given precursor, such as radon content changes, fireballs, unusual animal behavior, and the like, to the expected time of the earthquake.

On August 11, 1976, the seismological bureau submitted an urgent report to the provincial revolutionary committee. Early on the morning of August 12, the committee issued a bulletin ordering an emergency alert and called for limited evacuation of women, children, and older people from their houses. Adult workers remained on their jobs. Four days after the order of the state of alert, on August 16, a magnitude 7.2 earthquake occurred, and on August 22 and 23 two other earthquakes occurred, though none of those were near a city. When asked if recordings had been made of the strong ground shaking, the seismologists said that Szechuan Province had no strong motion seismographs but that the adjoining province had some that had been installed at the boundary between the two provinces about 100 miles from the earthquake epicenter.

The most spectacular failure of the Chinese prediction effort was, no doubt, the failure to predict the massive Tangshan earthquake of July 28, 1976. At 3:24 a.m., the terrified residents of that industrial metropolis in northern China were jolted by the largest earthquake to hit a populated area in Chinese history. The earthquake, totally unanticipated by Chinese seismologists, killed hundreds of thousands — perhaps as many as three-quarters of a million — and transformed the once-populous city into a wasteland bleakly reminiscent of Hiroshima. More than 90 percent of the brick buildings and nearly 80 percent of the industrial constructions were either totally collapsed or seriously damaged. The sudden liquefaction of the soils in the area in the course of the shaking caused heavy damage.

Even more important than the failure to predict the earthquake was the failure to anticipate with stricter build-

Roads were severely damaged by soil failure. Although all the usual manifestations of soil liquefaction occurred during the Tangshan earthquake, relatively little new information about the phenomenon came out of it, mainly because there were no strong-motion records of ground shaking in the regions where liquefaction took place.

ing code requirements that such an earthquake could occur in the area. Tangshan was zoned for earthquakes measuring no more than Intensity VI (on the Chinese Intensity Scale which, differing from the Richter Scale, measures reactions of people, structures, and so forth to the severity of the shaking), and the Tangshan earthquake was an Intensity XI.

But outside of the obvious failures and the obvious successes, it is difficult to pry from Chinese scientists much specific information on their overall record.

"The nagging question to me is how many failures and false alarms they have had," Clarence Allen says. "If you ask them, they say, 'very many.' The Chinese readily admit to many failures, and although they can rightfully claim a number of successful predictions, they do not yet claim to have a successful program of routine prediction."

Another question is whether the Chinese definition of "prediction" is the same as the U.S. definition.

"We were told that in one place in Szechuan there had been successful predictions of 17 out of 55 earthquakes," George Housner says. "However, the Chinese don't try to predict precisely the time and place of an event. What they do, I would call issuing an earthquake alert for a region."

Politics affect more than just the dissemination of information about their prediction effort. Housner discovered on his delegation's inspection trip of Chinese building sites that construction also suffers.

"An active building program is under way that is utilizing all available materials. The supplies of cement and steel are rather limited, so most buildings are brick. They are using everything they have, even adobe brick for small structures," says Housner. At one site he visited, the masonry was so poor the mortar could be crumbled with the fingers.

"I asked the building foreman why the workmanship was not better. He said that under the Gang of Four manual labor was exalted and respected, while intellectual concepts were demeaned. In construction practice this meant that the worker had increased authority and stature with respect to the engineer or inspector. If the inspector tried to disqualify work or refuse to accept poor materials or workmanship, the worker could disregard him."

Even considering the politicization of science and engineering in China, and the very different social systems between China and the U.S., can the two efforts toward prediction be usefully compared?

"Their earthquake prediction effort is certainly bigger — there's no question about that," says Allen. "But in many ways we feel that our program is more promising. The Chinese effort is terribly pragmatic.

"I think we feel that in this country in the long run the basic understanding of the phenomenon is critical to the development of prediction capability. And, thus, we have a much greater emphasis on instrumentation like seismographic arrays — sophisticated methods of really trying to understand what's going on down there.

"I do emphasize that the differences in the Chinese and U.S. programs to some degree reflect very basic differences in the needs of the two countries — in their social and political philosophies, and in the relative prosperity of the two countries. So, in this context, it would not be really fair to say that one program is necessarily better than the other.

"When we do become able to predict earthquakes on a routine basis, my hunch is that both the Americans and the Chinese will be able to look back and claim a fair share of the credit."

– by Dennis Meredith

Winchester Jones

—How It Was

Louis Winchester Jones, dean of admissions, emeritus, came to Caltech in 1925 as an instructor in English and became a member of the freshman admissions committee a couple of years later. He became dean of admissions in 1937 and associate professor of English in 1943 – and he retired in 1968. Sandwiched in among those posts and dates were a variety of other services to the Institute: registrar, assistant dean of upperclassmen, director of admissions, and director of undergraduate scholarships, for example. Appropriately enough, he was also a trustee of the national College Entrance Examination Board, membership chairman of that board, and president of its West Coast section.

At the time of his retirement, E&S *noted that "for nearly 40 years, Winch Jones has had a hand in the selection of Caltech's freshman class – and thus, a hand in shaping the kind of school Caltech has become." The truth of this observation made him a natural for early participation in the oral history project being conducted by the Institute Archives.* E&S *has made a shortened version of the original transcript of the interviews conducted by Mary Terrall and presents here Part One (of two parts).*

Winchester Jones: I was born on the Eastern shore in Maryland, but I left there when I was three or four years old, and we were abroad for a year or so. I don't remember much about it, obviously. And then we came back. My mother died when I was three. My father married again when I was about five, and came back to California where he had ranched before he married the first time — right back of the mountains here in the Santa Ynez Valley. When he came back, we lived in Montecito, and also he had a ranch over the mountains where I spent a lot of time.

There were about four or five big ranches in the Santa Ynez in those days. Now they're all cut up into smaller ranches, 100 and 200 acres, but in those days nobody knew how far they went back toward the desert. I lived there a good deal of the time and also over here in Montecito, of course — until I went East to school when I was fifteen.

Mary Terrall: What decided you to go East to school?

WJ: My father died in an accident, and nobody seemed to be very much interested in me or what I did. And I had a friend, who had gone to St. Paul's in Concord, New Hampshire, and thought highly of it. I didn't have a very good school record, but they had no boys from California in those days because it was a long way to go. People out here pretty much stayed out here. I guess St. Paul's wanted to spread their geographical distribution; anyway, they took me, and I had a wonderful time there for three years. And then I went into the Army, and then to Princeton, after World War I.

MT: What decided you to go to Princeton?

WJ: Mostly my friends at St. Paul's who were going. We got scattered a bit in 1918, and went into various branches of the service as soon as we got out of school. But I knew a lot of them would end up there, so I went there. There were some 70 or 80 of us from St. Paul's in that freshman class, which made it very nice. And then I got married five days after I graduated from Princeton — because it took five days to get out here. My wife lived in Pasadena.

MT: Had you met your wife back east?

WJ: No, I met her in Santa Barbara. In those days, it was considered cruelty to females to leave your wife or children in Pasadena over the summer. You had to send them to the beach; it was too hot in Pasadena — they couldn't possibly survive. So she used to come up here with her family every summer, and I was here on vacation from school. And we met here when I was in, I guess, about the third or fourth form. (I never can remember the equivalent to grades. The sixth form is the twelfth grade.)

MT: So this would be just after you went back East then?

WJ: That's right. In fact, we were engaged when I went back East to join the Army. Then, after four years of college, we got married and I went back East to work in a brokerage house.

MT: How did you get that job?

WJ: Well, I knew the head of the firm. He had a son who was my age, and the son got into a little trouble in school. The headmaster asked me if I could straighten the kid out — he was several forms below me — and I did, temporarily anyway. He ended up in Leavenworth, as a matter of fact, some years later. I don't think that was my influence. But, anyway, the father was very grateful for the whole thing, and said that he wanted me to come work for the firm. Well, in those days, if you wore button-down collars and a Brooks Brothers suit, you sold bonds. That was just fate.

MT: Had you taken a degree in English at Princeton?

WJ: No, I didn't, and this is something that doesn't really weigh on my conscience because it was an honest mistake. I was persuaded I had graduated in English literature. I took an awful lot of it. My diploma says I graduated in economics. I didn't find that out until years after I had worked for Caltech as an English teacher, and the diploma appeared from some drawer or other, and I looked at it out of curiosity, and found I graduated in economics. I think it's an error. I hope so.

MT: So once you got into the brokerage firm . . . .

WJ: I found that it was definitely not for me. And so we came back to California. My wife was pretty homesick for it anyway. I worked in a bank in Pasadena for two years, and we built a house there and settled down. I didn't care much for banking, and I'd always wanted to teach — I'd done a lot of tutoring in college — so I applied for a job at Caltech in the English department. I went out to interview with Clinton Judy, and he said, "Well, there isn't anything open, but I'll keep you in mind." That didn't sound very encouraging, but about a month later, he called up and said a member of the staff was ill, and would I want to take the first term. The man never came back, actually. So I stayed on as a member of the English department.

MT: When you went to apply, were you familiar with Caltech?

WJ: Not very. It happened to be in Pasadena, and that's where I wanted to live, and I didn't want to teach in public institutions. I wanted smaller classes and the kind of thing that Caltech had to offer. Also, it struck me as a very interesting kind of teaching to do. In those days at Caltech — the early twenties — you had for the most part a group of youngsters who had conscientiously avoided humanities in high school. It was a waste of time as far as they were concerned. And you had to do something to show them that it *wasn't* a waste of time, that a different type of intellectual activity could be interesting.

Well, it wasn't so difficult as it might seem, because you did what came naturally; that is, you gave an assignment, and the assignment would usually involve writing something. You would ask, "What was your idea of this thing that you read?" And you would get back fairly well-written themes; even in those days the Caltech kids were pretty smart, and they knew how to write, more or less. If they didn't, it didn't take them long to catch on. They did realize that they ought to at least be able to write; they had to write reports and things like that. They would hand these papers in without too many errors in them, and you would give the paper a "C," because it didn't have a single idea in it that was interesting. They parroted everything they had read and remembered it as though it was a mathematics text. And they gave it right back to you. I would give them a "C" for this, and they would come in, indignant, or weeping, or whatever, because they had never seen a "C" on their record — they wouldn't be at Caltech if they had.

"I don't understand, sir. What is wrong? You didn't put any marks on it."

"Well," I said, "no, there is nothing mechanically wrong with the thing."

"Then why didn't I get a better grade?"

"Well, frankly, because it bored the dickens out of me."

"Are we supposed to interest you?"

"You certainly are, if you want a decent grade."

"What am I supposed to say?"

"You aren't *supposed* to say anything. I want you to tell me what you want to say. What did you think of that thing you just finished reading for this class?"

"Well, I didn't like it very much. Seemed kind of dull."

"Fine. All I want to know is why didn't you like it?"

After they caught on to this, you couldn't hold them, because it was the first thing they had studied at Caltech to which they were entitled to their own opinion. They had good, original, creative minds, but they weren't entitled to their own opinion of Boyle's Law or the Second Law of Thermodynamics. All of a sudden, here was something they could get their teeth into and throw it back at you. Well, after that, I don't think we ever finished a morning's assignment actually, because we got to arguing, fighting about this and that. Of course, I always took the opposite side from what they took. And this turned out to be a lot of fun. Strenuous as anything, because they were smart young minds, you know, and it was risky to take the opposite side where often there wasn't much to sustain it. But I could talk faster than they could — that was my advantage.

MT: These were freshmen that you were teaching?

WJ: I taught freshmen and juniors for the first three or four years, and then I had one senior class. As I remember, in those years there was no English in the second year. That's where history came. Sophomores didn't take English, I'm sure. Just freshmen, juniors, and seniors.

MT: You came in 1925, and Munro came along about then too, didn't he?

WJ: A little later, I think.

MT: He came as chairman?

WJ: No, he was a professor of history. But he had the biggest office in Dabney Hall with an outer office for his secretary. Clinton Judy, who was the chairman, just had an office like all the rest of us, up on the top floor. Of course, poor Clinton was kind of in the shadow there.

MT: How did Munro happen to get himself the biggest office?

WJ: Well, in the first place he was a big shot. It was quite a feather in Caltech's cap to get him from Harvard, where he was the head of the history department. And he was on the Caltech Executive Council. Of course, he also was a scholar and had done a good deal of writing; he needed a secretary, and he needed that space. None of us at that time on the humanities faculty were scholars. We were teachers. We knew a reasonable amount about our subject, but I don't recall that any of us ever published anything. Roger Stanton and Harvey Eagleson and George MacMinn — no, we all had a jolly good time and enjoyed our teaching, but we didn't take writing very seriously. I don't think Clinton Judy ever published anything, but he was a true scholar of the Oxford type. He actually was, of course, a Rhodes Scholar and went to Oxford. Publication didn't mean anything to him, but knowing everything did. He had a magnificent library — I don't know how many hundred books he had, and he knew everything in every one of them.

We were, in those days — this goes for history and languages as well — a service division. We were not a scholarly division, or one in which any degree was going to be granted. That didn't come until a few years ago. So teaching was what counted. And those who were chosen to teach there were pretty much more interested in teaching. Later on, we did get some good scholars; Wallace Sterling and Rodman Paul, I think, were the first. They were both publishing, and that was new in the division.

MT: I was going to ask you about personal friendships with people in the science and engineering divisions.

WJ: Oh, there were plenty of them. There was no distinction between humanities and the other divisions. Everybody got along pretty well together, and some of your best friends might be mathematics or physics or chemistry people.

MT: What did the people in the other divisions think of the humanities? Did they think it was important?

With Lee A. DuBridge, who was president of Caltech at that time, at a General Motors scholarship committee meeting in 1960

WJ: They thought it was important. This attitude had been drilled into Caltech from the very beginning by George Ellery Hale and Noyes. Both Hale and Noyes were convinced that engineers and scientists had to know something besides engineering and science. They needed literature and history and language. So the faculty was definitely sympathetic toward it. The only antagonism I ever noticed, and it was very noticeable, was when Munro wanted to enlarge the humanities to become a scholarly division. He had some money that he could get for this purpose and he claimed he couldn't get it for any other purpose.

There was a rather bitter faculty meeting on this. Munro was there, and E. T. Bell, who was quite a character, got up and denounced the whole scheme, and Munro, and everything else. He said they were diverting funds that were necessary for science and that they didn't have enough as it was, which was true in those days. And there was quite a to-do over that. (There was another Bell, Jimmy Bell. They were known as Wild Bell and Tame Bell, and Eric Bell was Wild Bell.)

MT: So this idea didn't have support among other humanities people either?

WJ: Well, most of us couldn't have cared less whether they did that or not. We were enjoying what we were doing, and we knew we were doing a lot of good. We knew we were reaching those kids. I don't mean all of them, 100 percent, but we had a pretty high degree of success, and we knew in later years, when they came back and talked to us, how much it had meant to them. Well, this was fine with us. We weren't interested in having a big division that was going to turn out degrees.

In fact, we wondered a little bit whether people who were really scholars were going to have the patience to work with those youngsters. They took a lot of time. They were interested, but you had to bring them in and go over their papers and their ideas and their attitudes with them, and discuss; it was almost a tutorial arrangement in those days in the humanities. You called one or two students in and said, "You're not really envisaging what happens, or you're not thinking what's behind this sort of thing. Let's go over that and see what you get out of that paragraph. What does it really mean to you?" Or, "How do you think the man said this in a dialogue? What was his tone, what was his expression?" And finally, they'd learn to *read*. But this took a lot of time, a lot of energy, and I'm not sure today, with

Winchester Jones

Directing the band at Freshman Camp

publication, that the students get that much time and energy devoted to them.

All right. That's why I went there and why I enjoyed it and why I stayed on teaching until I went into administration. And I did that for one simple reason — money. I had no doctor's degree, and the attitude in the division was changing. After Clinton Judy retired (and he died shortly after), it began to build up as a more scholarly division. I could see that I wasn't going to get any promotion or salary increases to amount to anything, but I knew I could do some pretty good administration, and I went into it for that reason. I taught part-time for quite a while afterward.

MT: I believe you were on the Freshman Admissions Committee back in the twenties, right?

WJ: I was on the Admissions Committee from about '26 or '27, and I was fascinated by it. We weren't as thorough as we got to be later, but we did our best. We gave our own examinations for a number of years before we went to the College Boards. We went to them for the very obvious reason that it got to be more and more difficult to find people to proctor our examinations back in New York and Boston and wherever. By that time we were getting more people from farther away. In the early years they were mostly Californians.

MT: Was the Admissions Committee actively recruiting students in the twenties, or was it just waiting for people to apply?

WJ: It pretty well just waited for them to come in. Jimmy Bell, who was chairman at that time, did go out and do some recruiting in the local high schools. I think our freshman class then was around 120, and we might have 300 applications or something like that. Our applications were pretty good though, so even though we picked from that few, they were pretty smart fellows. When Phil Fogg took over, he combined the jobs of registrar and director of admissions, which had been split, with Harry van Buskirk as registrar and Jimmy Bell as chairman of the Admissions Committee. Then after that, I took Phil's place.

MT: And you did both also?

WJ: I did both for a while, and then it got to be too much. We were getting applications from all over the country, and they were numerous, and I just couldn't handle both jobs and do them well. So I gave up being the registrar, which was far less interesting to me than the admissions.

We didn't have interviews at the beginning. We just said that there was no sense in it. But the difference between a score of 780 and a score of 800 on a College Board doesn't mean a thing, and in the sciences and mathematics we were getting all in the 700s. So we decided that we'd try for the interview, which meant we had to try to get some money. And we got it. Millikan was persuaded of the value of it. And it was then that we started sending out the members of the committee on interview trips.

I was always amazed and enormously pleased that very busy science and engineering faculty members — publishing, teaching, doing research — were willing to take a week or two off and travel around the country, poking into little high schools and big ones, to talk to people. Now, the interview has often been misunderstood, or misinterpreted. People think of the interview as being with the student. And they ask, "What can you find out from a shy, scared little kid?" You don't find out much from him. Even after a long session, you can't tell much about him. You get the information from his teachers. You can sit down with his math teacher, or his physics teacher, or his chemistry teacher, and say, "What did this fellow do that he didn't have to do to get a good grade? Sure, he got all his homework in; so did a lot of other people. So he didn't make any trouble in class; neither did a lot of other people. Did he ever come in with the urge to go further with something? In other words, how about his curiosity — has he really got it?"

That's where we got our information. Sometimes we got bad information — not infrequently the teachers didn't know enough to know whether he was that good. But by and large the interview paid off; it wouldn't still be there, obviously, if it hadn't.

MT: What about scholarships? Was there a separate committee for scholarships in those days?

WJ: Yes. As far as I remember, there have always been separate committees for admissions and scholarships. For a time, as I recall, I was chairman of both. Then I was given the title of Director of Scholarships. The last few years I was there, they had a chairman of the scholarship committee. The faculty was beginning to feel that they wanted more direction of the administrative positions. You see, for years, Earnest Watson, who was Dean of the Faculty, and Frederic Hinrichs (and later Paul Eaton), who was Dean of Upperclassmen — and Foster Strong, Dean of Freshman; and I — used to meet and appoint all the faculty committees. Now, of course, the faculty has an election system, all very elaborate and very democratic. It wasn't democratic in those days at all. Well, the faculty finally began to feel that they wanted to get in on this process. There began to creep in, in the last two or three years I was there, some kind of feeling between the administration and the faculty, as exists in a good many other colleges and universities. But for most of the years I was there, there wasn't any feeling at all of, "Well, that's the administration," because we were *all* faculty members. Every one of us was teaching. Now you've got a number of administrators who don't teach at all, and who didn't grow up there, and there's a very different attitude. I think it's too bad that this has occurred.

MT: Back in the twenties and thirties, were there many students on scholarships?

WJ: Not very many. We didn't have very much money. The biggest scholarship we had was tuition. So these poor fellows had to scramble up board and lodging some way or other. I don't remember now when we first struck out and determined that we were going to get more scholarship money out of the budget.

Remember that a lot of that time we are talking about was the Depression. The Fleming money just vanished. And all of a sudden, there we were, $5,000,000 short. Salaries were cut, everything. That Depression lasted, you know, until the forties. For a long time, things were pretty tight. There just wasn't any money for scholarships. But as soon as things began to loosen up a little bit, and we could approach individuals, then we began getting scholarships for more than full tuition, and many more scholarships.

MT: Were scholarships awarded on the basis of scholarship or need?

WJ: At first it was on the basis of scholarship, and that was true for a number of years. Then we got more and more into the basis of need. People began to say, "Wait a minute, these kids are all good. We're losing too many fine boys because they didn't stand 2½ points higher here or there and they can't come without money." We had been on the need basis for some time when the College Scholarship Service was started, and with them you had to be on a need basis. Now that's breaking down again, I'm sorry to say. They're starting to buy students again, the way they used to, with more and more of these so-called "honors scholarships."

There used to be tough competition buying students, you know, taking them away from another college. MIT and Caltech have always gotten along pretty well, and we've had a big overlap list. I used to go back every year between the time we made up our mind on scholarships and the

On the steps of Throop Hall in 1968

time we had to notify the boys. I'd dash back for three days, and go over what we were offering with schools like MIT and Cornell and Carnegie Tech, who had many duplicate applications. We'd finally get down to the same comparable figure — depending on how much tuition difference there was, and the travel allowance from the East or from the West — so the applicants could take their choice.

MT: I wanted to ask you a few more things about the social atmosphere in the early days, before the war. I've heard that, for example, there were regular discussion meetings of some sort at Clinton Judy's house. Did you go to those?

WJ: Oh, yes. Once a week, Clinton would have us down at his house, and I suppose there would be as many as a dozen faculty members.

MT: From different fields, different divisions?

WJ: Anybody who wanted to come. And generally somebody was asked if he would prepare a short paper. And he'd give it, and the others had read — or tried to read — in the area he was talking on, and then we just sat and argued and had a fine time discussing.

MT: What were the topics like?

WJ: Well, for instance, I did one on Eugene O'Neill, and somebody else might do one on Victorian poetry. As I recall, I did one on the appearance of myth in Byron and Shelley, something of that sort, and I did one on Conrad. We were all amazed at Charles Richter, of the Richter Scale, you know — the seismologist. That man had read about everything ever written in English literature, I think. I never saw such a mind; and he retained everything he'd ever read. He gave some eloquent papers on rather abstruse poets that very few of us could recollect at all — and other times on things that were well known. He was amazing.

MT: And the other scientists gave papers on literature topics also?

WJ: Yes. It was all on literature, there wasn't any science in it. They were glad to get away from science for a change and do a little something else.

MT: How long did that go on?

WJ: It went on until Clinton began to fail, just about the time he retired. It started, as I recall, in the late thirties — somewhere around there. There was also a smaller group of seven or eight of us, including Clinton — I guess Clinton and I were the only ones from Caltech — who met every Monday night at each others' houses and had general discussions of everything. That went on right up to the time that too many of them died, shortly before I retired. That began back in 1925 or '26.

MT: You said that you were the only people from Caltech; where were the other people from?

WJ: There were two lawyers, two doctors, a book publisher, and the head of the California division, or whatever you call it, of Price Waterhouse. We were all from Pasadena.

MT: Was there any contact with the trustees back in those days?

WJ: There was very little in the early days. When Jim Page became chairman of

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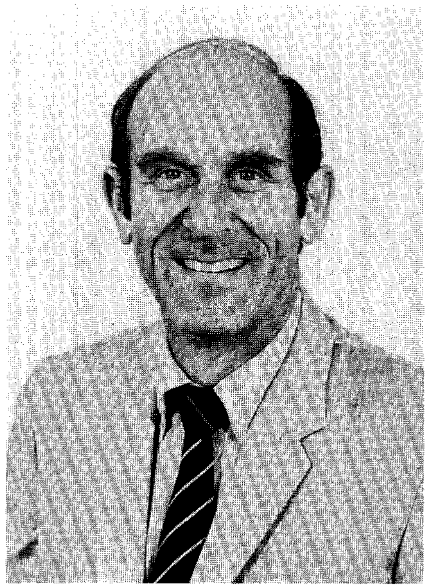
In Memoriam



Richard P. Schuster
1925-1979

Robert W. Vaughan
1941-1979

The crash of American Airlines Flight 191 in Chicago on May 25 was a personal and costly tragedy for the Caltech community. Alumnus Richard P. Schuster (BS '46, BS '49), who was director of development for Caltech, and Robert W. Vaughan, professor of chemical engineering and chemical physics, were among those who died. Alumnus Terry E. Ernest (BS '63, MS '65) also lost his life in the accident.

A memorial service was held on campus on June 5 for Schuster, at which the speakers were his long-time friend James Bonner, professor of biology; Charles Newton, lecturer in English emeritus and former director of development; and Marvin L. Goldberger, president of Caltech. C. J. Pings, vice provost, presided.

On June 14 a memorial service was held for Vaughan. Harry Gray, chairman of the division of chemistry and chemical engineering, presided and spoke briefly about Vaughan. In addition, short talks were made by his colleagues Sunney Chan, professor of chemical physics and biophysical chemistry, and John Seinfeld, professor of chemical engineering; one of his graduate students, Jeff Reimer; John Baldeschweiler, professor of chemistry and former division chairman; and Dr. Goldberger.

Below are brief biographies of Schuster and Vaughan and excerpts from some of the tributes offered at the memorial services.

Richard P. Schuster, 53 was born in Los Angeles and attended high school in Beverly Hills. After receiving his BS in electrical engineering from Caltech, he spent a year in the Navy and then returned to the Institute to earn another BS in applied chemistry. For two years he served as a production foreman at the Procter and Gamble Manufacturing Company in Long Beach, and for the next ten years he was plant manager of the Bray Chemical Company in Los Angeles. In June 1962 he joined the staff of JPL as a staff engineer working in the newly created Arms Control Study Group. In 1964 he came to the campus as director of the Industrial Associates program, was named director of foundation relations in 1976, and became director of development in 1977. He is survived by a son, Mark, and a daughter, Catherine.

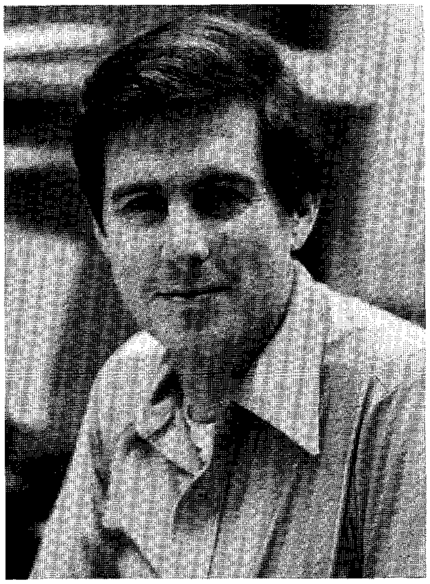
James Bonner: "Dick could do absolutely anything — electrical engineering, mechanical engineering, chemical engineering, or plumbing. He understood them all. Confidence was Richard P. Schuster's middle name. I have often thought that if I were marooned on a desert island the person I would like to have there with me to figure out how to build a boat and get away would be Richard Schuster."

Charles Newton: "I first got to know Dick when I was trying to persuade him to come down from the Jet Propulsion Laboratory and take charge of the Industrial Associates. At the end of three months of effort on my part, Dick made up his own mind. He decided it would be the right thing to do both from Caltech's point of view and his own. Dick believed in right and wrong.

"His job was a large part of his life, but it wasn't all. In 1964 Dick became a member of the board of directors of the Caltech Y, and he served on the board for years after that. He became an active alumnus, and in 1965 he was elected president of the nationwide Caltech Alumni Association. He was also a member of the Caltech Beavers, which was a considerably less formal group. He was physically active and energetic; almost every Sunday found him on the Athenaeum tennis courts, and at all times he was an endless and tireless hiker.

"Dick gave many years of his life to Caltech. We are grateful to him for the energy and devotion he put into those years and glad he was with us."

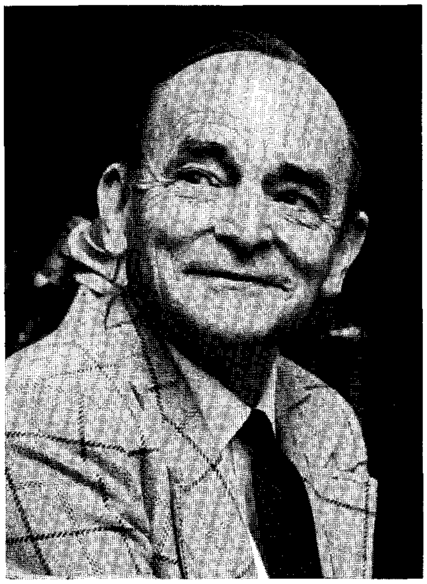
Robert W. Vaughan, 37, was a native of Oklahoma. He received his BS from the University of Oklahoma and his MS and PhD from the University of Illinois. He came to Caltech in 1969 after serving a tour of duty with the Chemical Corps of the United States Army, mostly at the Jet Propulsion Laboratory. Vaughan's research was in the general area of solid-state and surface chemistry and physics. He was the recipient of numerous awards, including an Alfred P. Sloan Research Fellowship, a Dreyfus Teacher-Scholar Grant Award, and the Fresenius Award of Phi Lambda Upsilon, a national honorary chemical society. He is survived by his wife, Sharon, and his daughter, Tena.



John Seinfeld: ''Bob's thesis at Illinois was on the Mössbauer effect, which concerns the recoil-free emission of gamma rays by nuclei in crystals. The discovery of this effect earned the Nobel Prize for Rudolf Mössbauer, a German physicist. It was on that subject that Bob spoke at his first Caltech seminar in 1969.

''The seminar that day was characterized by the non-stop, rapid-fire delivery that we came to know as Bob's trademark. All through the talk a man sitting behind me kept asking Bob polite but penetrating, questions, which Bob fielded easily and confidently. Afterwards, Bob was introduced to the man — Rudolf Mössbauer — who turned out to be quite impressed with the work Bob had done. Thus Bob Vaughan's career at Caltech began.''

Jeff Reimer: ''Despite his rank and position, Bob Vaughan was a man of humility and self-sacrifice. He was willing to come at any time into the laboratory, day or night, rain or shine, to help students with problems. Every student who ever worked with Bob can remember picking up the phone to call him at home at one or two o'clock in the morning when things weren't going right at the lab — and having him run down and help out. On his birthday, Bob and Sharon drove two hours each way to come to my wedding. He really cared about his students, and I regard the moments I worked with him side by side in the lab as very precious.''



C. A. G. Wiersma

1905-1979

On May 19, C. A. G. Wiersma, professor of biology emeritus, died after a long illness. He was 73 and had been a member of the Caltech faculty for almost 45 years. Born in the Netherlands, he studied at the universities of Leiden and Utrecht and was invited by Thomas Hunt Morgan to come to the Institute in 1934 as a representative of the then relatively new science of comparative physiology.

Wiersma's doctoral thesis in 1933 was on the nerve-muscle system of crustaceans. He maintained an interest in this class during his entire scientific life, performing pioneer work first on the neuromuscular system, then on the central nervous system, and during the last few years on the visual system. His work provided an important link between neurophysiology and behavior. At the time of his retirement in 1976 his former co-workers — undergraduate students, graduate students, and fellows — presented a symposium in his honor.

In addition to his work at Caltech, Wiersma was from 1943 to 1950 a member of the attending staff of Los Angeles County General Hospital, making studies of the myography and treatment of poliomyelitis and the treatment of schizophrenia with electronarcosis.

He was a member of the Society for Neuroscience, the American Physiological Society, the American Association for the Advancement of Science, and Sigma Xi. In 1956 he became a correspondent of the Royal Netherlands Academy of Arts and Sciences, and he was recently elected a foreign associate of the National Academy of Sciences.

Wiersma is survived by his wife, Jeanne, who lives in Pasadena. A memorial service was held in his honor early in October at which tributes were offered by Donald Kennedy of Stanford University, and his colleagues in the Caltech Division of Biology, Antonie van Harreveld and Felix Strumwasser.

Robert D. Gray

1909-1979

Robert D. Gray, professor of economics and for 36 years director of the Industrial Relations Center at Caltech, became professor emeritus at the end of the last academic year, as reported in the May-June issue of *Engineering & Science*. On July 4, Gray died after a long illness. He was 69.

In Memoriam

For all the years he was at Caltech Gray devoted energy and enthusiasm to administering the Center and to teaching industrial relations and management to Caltech students. He was concerned for the well-being of those students and determined to prepare each of them for the problems likely to be encountered in industry.

As an administrator, Gray converted a fledgling Industrial Relations Section into the present Center with its enviable reputation in the industrial community. Thousands of professionals in Industrial Relations were given extra polish in the seminars and programs of the Center, and other thousands of line managers learned the techniques of supervising with emphasis on interpersonal relationships.

During World War II Gray served as adviser to the Railway Labor Board Emergency Panel, and then he organized a massive program of war training classes at Caltech.

He further served the Institute as chairman of the Insurance and Annuities Committee and of the Grievance Committee; he was an advisory member of the committee on the Industrial Relations Center; and he also was a member of the ad hoc committees on Compensation and Continuing Education.

He served an unprecedented 19 years on the California State Personnel Board; was chosen for the Governor's Advisory Council on the Department of Employment; served as chairman of the Labor-Management Council; and was a board member of the California State Employees Retirement System. His books and research publications require many pages for complete listing, and he has become a recognized authority in the fields of compensation and employee benefits.

Gray was the recipient of many awards and honors, and he was active in civic affairs. He was a member of the Twilight Club and the Rotary Club of Los Angeles. He was elected a director or officer of the Pasadena Chamber of Commerce from 1950 to 1956, serving as president in 1953-54. He also served on several committees of the Greater Los Angeles Chamber of Commerce.

Services were held for Gray on July 7 at St. Philip the Apostle Catholic Church. He is survived by his wife, Mary, his daughter, Mrs. Mary Belinda Lucey, and two grandchildren.

Ernest E. Sechler

1905-1979

Ernest Sechler, professor of aeronautics emeritus, died on August 14. He was 73. Born in Pueblo, Colorado, he entered Caltech as a freshman in 1924 and was the first person to receive an MS degree in aeronautics from the Institute. He also held a BS in engineering, an MS in mechanical engineering, and a PhD in aeronautics. Though his PhD was awarded in 1934, he actually began his career on the faculty as an instructor in 1930. He became a full professor in 1946 and was executive officer for aeronautics from 1966 to 1971. He became professor emeritus in 1976. Over the years he performed most of the admissions work for GALCIT — the Graduate Aeronautical Laboratories of the California Institute of Technology — a vital task in the success of the option.

Sechler devoted his professional life to teaching and research on the design of safe, lightweight structures — from aeroplane fuselages and the thin shells of rockets and boosters to the shell structures that cover the 200-inch telescope on Palomar Mountain and the Cooperative Wind Tunnel. He was a consultant for many facets of the aerospace industry, and he served as a member and chairman of various national advisory committees for the Air Force and NASA. In recent years he was active in promoting windmills as a power source, and in 1973 he and his colleague Homer J. Stewart developed the course "Case Studies in Engineering," which introduced students to the problems of management of large engineering projects, including financing, customer relations, and long-range planning.

Sechler was a member of the National Academy of Engineering, the American Association for the Advancement of Science, the California Academy of Sciences, and the National Defense Preparedness Association, and was a fellow of the American Institute of Aeronautics and Astronautics.

He is survived by his wife, Margaret, his daughter, Lorraine Sechler Emery, and two grandsons. Memorial services were held at Caltech on October 12.

Beach Langston

1911-1979

A Tribute by Kent Clark

Probably no Caltech professor ever needed a biography less than Beach Langston. Although there are details like dates and places that might be supplied by a formal memoir, the really important things about Beach were obvious to everyone who knew him — that he was a gentleman in an age when gentlemen are rare, that he was a literary man to his fingertips, that he was devoted to his students, family, and friends, and that he created an atmosphere of warmth, goodwill, and affection. And so, although I like to think that I knew Beach better than any of his other friends did (we came to Caltech in the same year, 1947, shared an office for six or seven years, and got so we could complete each other's sentences), the Beach I knew was essentially the one everyone else knew; and what follows is not really for the record but only for the pleasure of recall.

One of the first things strangers noticed about Beach, besides the obvious fact that

he was about six feet four and had blond curly hair (latterly gray), was his soft, distinctive Southern accent. Sophisticated strangers, experienced with Southern rhythms, might have identified Beach's speech as basic Atlanta (where Beach was born in 1911) with a large element of Charleston, South Carolina, where he attended The Citadel and met his wife Catherine, a small touch of pre-war Florida (denatured Alabama) where Beach's father speculated briefly in the real estate boom, a significant flavoring of Chapel Hill, North Carolina, where Beach took his PhD, and trace elements of El Paso where he taught in what was then the Texas College of Mines. Beach's cadences and pronunciations, which sometimes made his colleagues sound like chattering neurotics, had some fine peculiarities of their own. Although he loved and taught poetry for many years, he could never pronounce the word, and he could not pronounce *Cooper,* Catherine's maiden name. (Philologists have not so far invented symbols to describe his rendering of the vowel sounds in either word.) The fact that Beach spent the last 32 or so years of his life with Yankees, Westerners, Techers, and other low types perhaps faded his native rhetoric a trifle, but it was still distinctive and it was still Beach.

It was also very deceptive. It conflicted so oddly with Beach's social and political views that it sometimes seemed as if he had set out deliberately to fracture every cliché about Southern conservatism and prejudice. Beach, as we all know, was a liberal in a way that would put most hereditary Northerners to shame. The key to his liberalism was his empathy, his sensibility, and his lively imagination; it was no trouble for Beach to put himself in someone else's place, and he instinctively opposed any mode of oppression, no matter how elaborately packaged. He used to say, with a laugh, that one of his friends in El Paso had called him a bleeding-heart liberal and that his friend was probably right. If Beach's liberal politics were apt to come from his heart, there was nothing wrong with his head; he understood very well how political action works and he had firm principles. He left Texas College, for example, when the president tried to tell him what causes he should support. Beach was a one-man civil rights movement long before civil rights became a popular national issue, and it is an interesting and instructive fact that when the humanities division decided to offer a course in Black literature, it was Beach, born in the navel of the Confederacy, who taught it.

Beach's religion, like his politics, was a distinctive combination. Philosophically he might be classified as a Christian existentialist, something like W. H. Auden. In practice, and to Beach practice was almost everything, he was a Langston-style Quaker. Beach's personality fitted so well with the Friends' feelings for nonviolence, persuasion, individualism, and social service, that if Quakerism had not already existed, Beach might have invented it. Anything but a fundamentalist and absolutely unconcerned with conventional piety, Beach instinctively helped people. For this reason, I suppose, he did not need to be born again; he was born right the first time. Naturally, his willingness to serve meant that we all leaned on him and took advantage of his generosity, and he spent his life doing more than his share.

If all this sounds saintly, I should add that Beach was saved from anything like sainthood by his sense of humor, which kept him from taking himself and his good deeds too seriously, by his skepticism, and by an unblinking perception of character. Without an ounce of malice in his body, Beach recognized a certifiable creep when he saw one, and although you could probably count the people he actively disliked on the fingers of one hand, he disliked enough to save himself from a halo.

Professionally, Beach had the advantage of a Southern liberal-arts education, including Greek, and a remarkably wide reading background. He also had a variety of teaching experiences, which kept him from being narrowly specialized and which fitted with the variety of his personal experiences. In his early literary career, Beach concentrated upon Shakespeare and the Renaissance and his early publications were on things as recondite as the *ars moriendi* tradition in Elizabethan literature and the relationship between Shakespeare and the Romantics. As the years went by, however (and as we developed a need for specialists in American literature), Beach's interests drifted toward modern American writers. Since Beach was himself an anthology of modern American experience, since he had tried out a number of American value systems on his corpuscles, he was perfectly fitted for studying and explicating people like Hemingway, Fitzgerald, and Faulkner. Naturally Faulkner became his favorite, not only because they both knew the same South but also because they had been through the same emotional wars. Ironically, Beach probably knew too much about Faulkner for his own artistic good. He published his first studies of Faulkner in 1961, but his book on Faulkner, which he amended and revised endlessly, is still unfinished. A month or so before his

In Memoriam

death, he told me that he needed to do some major surgery on one of the chapters and a small amount of rewriting. Perhaps Jenijoy La Belle or I can finish it. From the point of view of his colleagues and his students, however, the publication of his Faulkner studies was irrelevant. We got the benefit of his wisdom on Faulkner and other literary and non-literary themes. I suppose that a few lines from Shakespeare, Milton, and Browning, delivered in Georgia accent, are still echoing in 301 Dabney.

Naturally, Beach's professional life spilled over into his private life (if *private* is the right word), and students, colleagues and friends were apt to find themselves co-opted into his family. People who were not totally depraved were apt to find themselves honorary Langstons — almost as much a part of the family as the Langston daughters Kitty, Lewise, and Dottie. (Beach used to say that like King Lear he had three daughters but on the whole his were better behaved.) Of these honorary Langstons there were certainly dozens and perhaps hundreds, and we all benefitted from a certain warmth and informality and from a level of manners that Yankee families simply cannot achieve. Perhaps it should be added, while we are on the subject, that if Beach's social and political views would have appalled his ancestors, even Jefferson Davis would have loved his manners. In a sense, the odd combination of radical thought with conservative decorum helps to define his character.

Integer vitae, says the poet, and if we want to use two words to describe Beach, those two will do as well as any, although they suggest more stoicism and less fun than he actually had. As for Beach's friends and Caltech in general, we can be described with one word, *lucky*. We had Beach as a pure gift; and as with all great gifts we did not have to do a thing to deserve him. □

Kent Clark is professor of English at Caltech. His affectionate memories of Beach Langston quite properly are not concerned with dates, but for the record, Dr. Langston died on April 10.

Winchester Jones . . . *continued from page 27*

the board, he sought a much closer relationship, but as long as Allan Balch was chairman, the trustees and the faculty just didn't have much to do with each other. We were represented — Earnest Watson, as far as I know, and of course Millikan, sat in. The rest of us just didn't bother much with the trustees, and they didn't bother us, and it was no loss to us or to them. The much closer relationship now, where they have division representatives to the trustees and so on, is new since my retirement.

MT: But socially you didn't see them either? They didn't move in the same circles?

WJ: No, not at all, unless you happened to know them in a different way. The only time we ever mingled socially was at the Associates' dinners, where the members of the faculty who didn't feel too awkward in black tie were asked to come and be nice.

MT: Did any faculty members do fund raising?

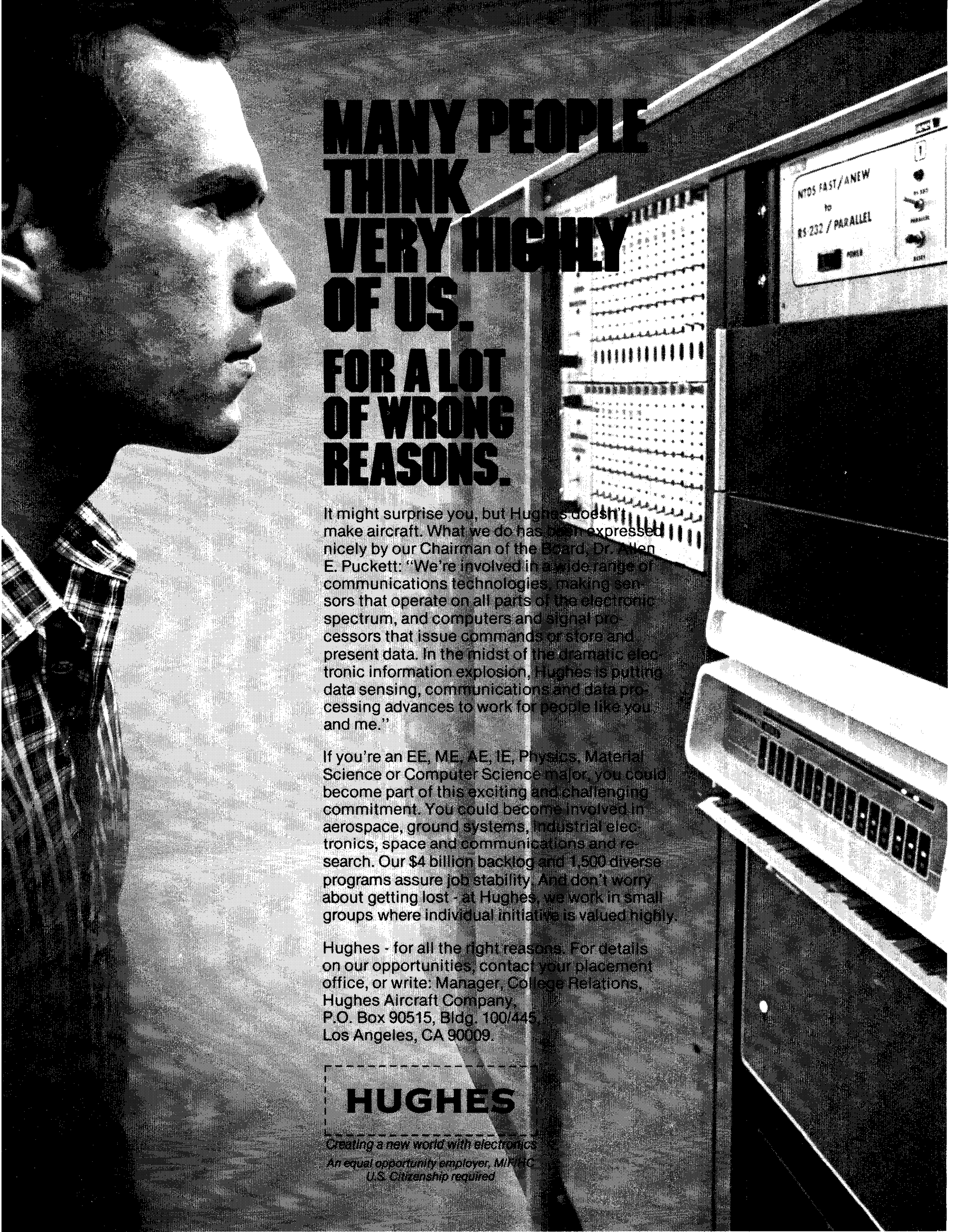
WJ: Not as far as I know. The Executive Council, perhaps, although the Executive Council as such, I believe, met only about four times in Millikan's entire career. I don't think it ever had anything to say about anything. Millikan ran the show, you know, though I think Munro had a good deal of influence on him. You see, Millikan was a great believer in democracy, provided it didn't interfere with getting what he wanted done. He never would take the title of president, because he said, "All right, we will do this in a democratic way. We'll have an Executive Council, and we will decide things in that. No one man's going to dominate." But, as I say, I don't think the Executive Council met very often. Max Mason, who was on it, told me he'd been on it for four years and he had never been to a meeting, so for sure it hadn't met in that length of time.

One thing I do have to say for Millikan. Sure, he was a dictator, in spite of all his talk about democracy. But we needed one then; we had to have one. Times were tough, and he was the greatest money-raiser that ever came down the pike. But he gave you a job and he let you alone. He never interfered. He'd gather it indirectly if you were not doing a good job, or if it were in academic administration, he knew darn well the faculty would take care of you if you weren't doing a good job. You'd come in to lunch at the faculty club and they'd say, "What the heck were you doing when you admitted this class, for heaven's sake?" So there wasn't any way you could backslide very much. And nobody wanted to.

Millikan knew that he had dedicated people there, people who wanted to do their jobs and who were good at it. He didn't need to interfere — although there were occasions, particularly in admissions, when a good deal of pressure was brought on him. I'll never forget when we turned down the son of one of the members of the United States cabinet. The cabinet member got hold of Giannini, who was head of the Bank of America, and Giannini said, "Well, I know all those trustees; your boy is as good as in." And said to Millikan, "Let that boy in." And Millikan said, "You go to Jones; I have nothing to do with it." And I think we lost some money. But Millikan wasn't going to interfere. He knew it was wrong to let that boy in when he didn't deserve it. He knew the committee knew what it was doing. No, we never had any trouble that way.

MT: Do you think things would have been different if Millikan had been president, instead of having the Executive Council?

WJ: No. There wouldn't have been any difference at all. I don't mean the members of the council and Millikan didn't talk to each other, but there wasn't any formal meeting where they voted on this or that. Sure, Millikan would ask Mason what he thought about this and he'd ask Munro, and Munro would go to Millikan and see about this or that, but it wasn't a formal meeting once a month where somebody made a motion and kept minutes. That just didn't happen. □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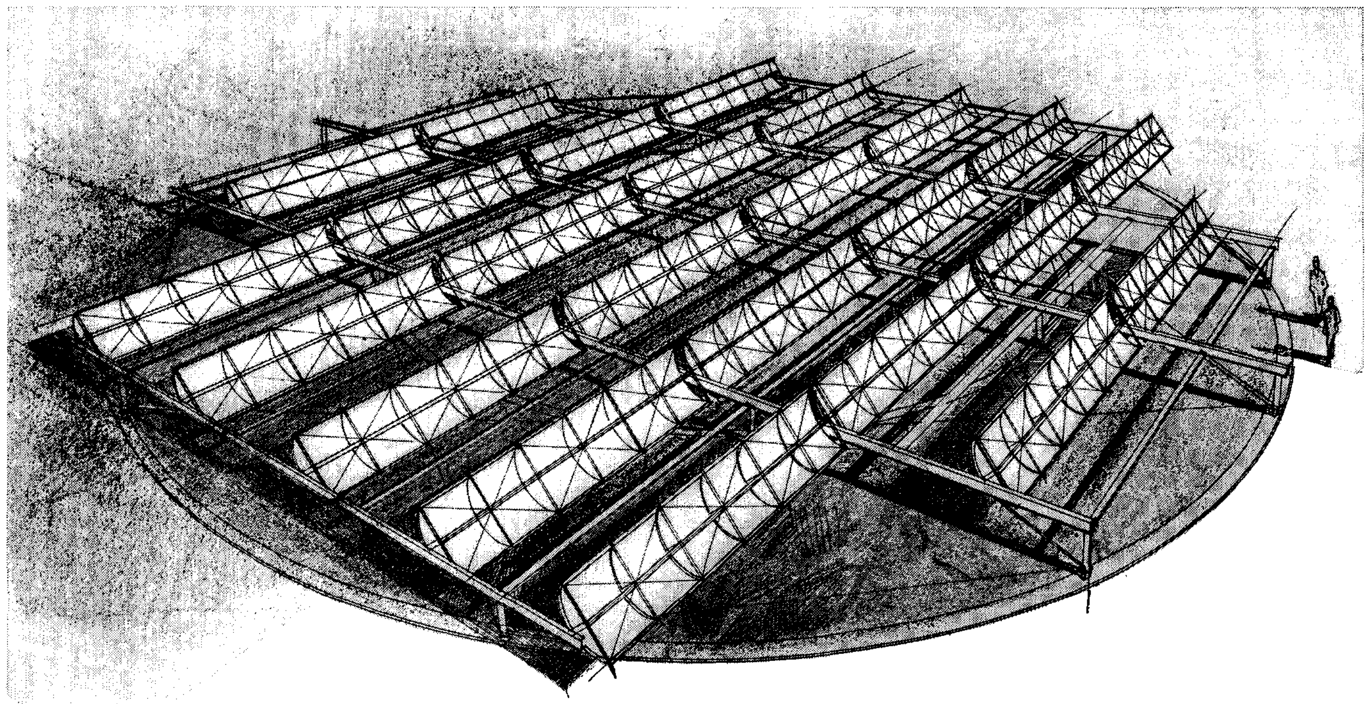

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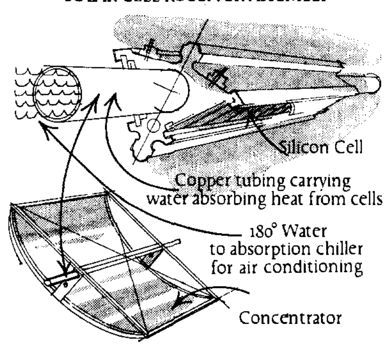

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