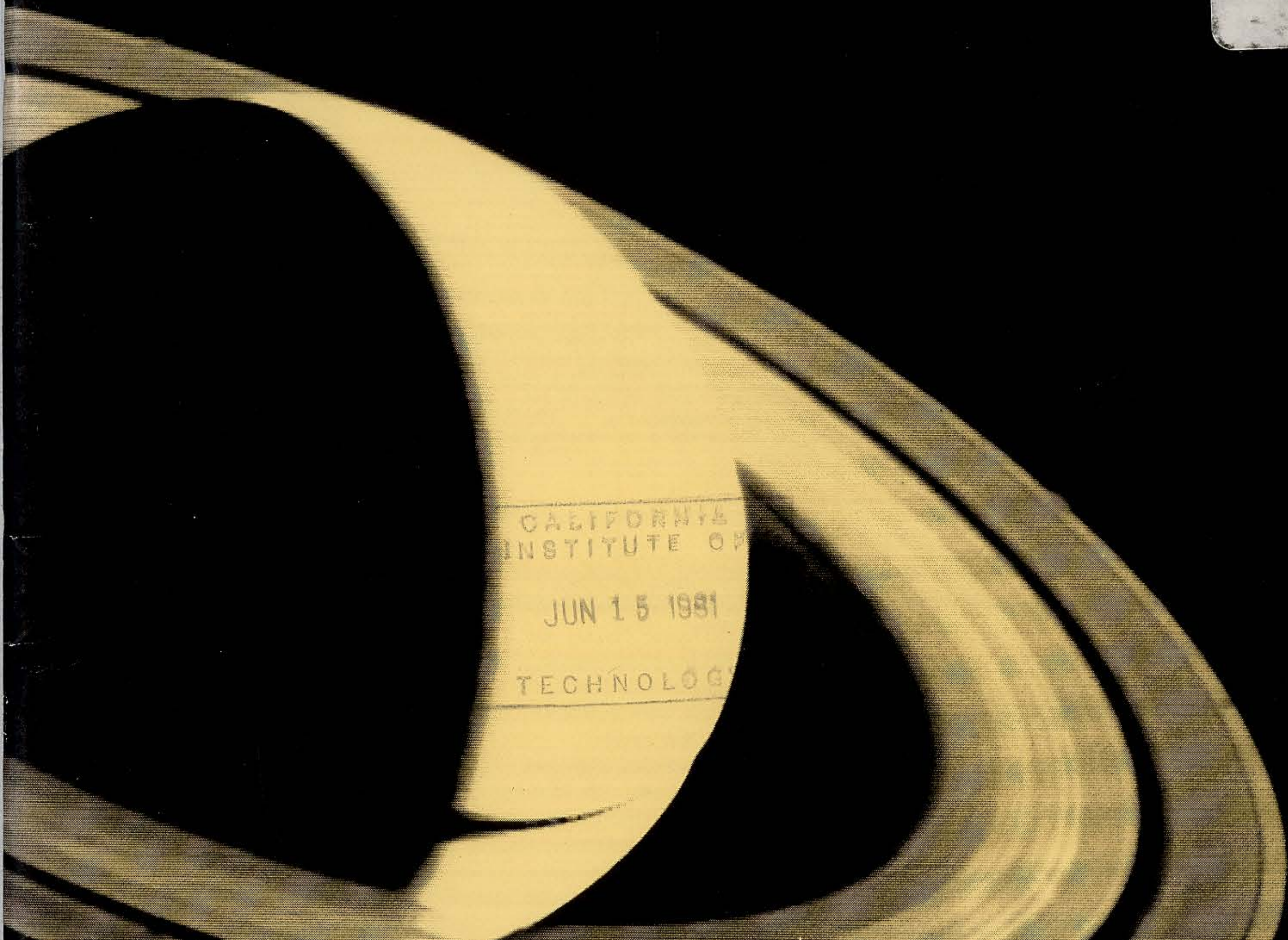
Engineering & Science

California Institute of Technology | June 1981



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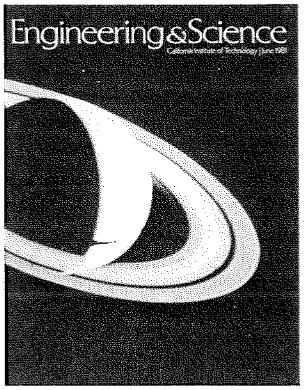
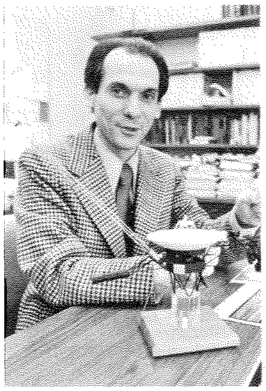
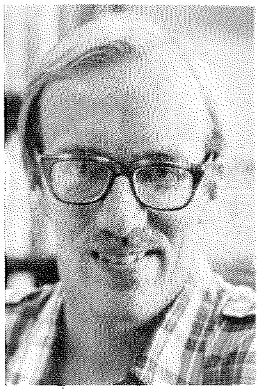



Edward Stone


James Bailey

Saturn

On the cover — Saturn seen back over the "shoulder" of Voyager 1 as it left the ringed planet last November. This different perspective comes from 3.3 million miles past Saturn, four days after the closest approach. Even as Voyager 1 moves on out of the solar system and Voyager 2 nears Saturn, data from this first encounter are still being studied and analyzed to clarify some of the startling and puzzling surprises that the spacecraft returned to scientists (and other enthralled spectators) on Earth.

Probably no one is better qualified to give an overview of these surprises than Ed Stone, whose article, "Voyager 1 at Saturn: An Encounter with a Multi-ringed Giant," adapted from his recent Watson Lecture, appears on page 6. As Project Scientist for NASA's Voyager Mission since 1972, Stone has coordinated all the scientific teams working on the two planetary exploration projects. His involvement with spacecraft goes back to his cosmic ray experiments on the Discoverer satellites in 1961; since then, he has been a principal investigator on six NASA spacecraft and co-investigator on four others. Stone came to Caltech after receiving his PhD from the University of Chicago in 1964 and has been professor of physics since 1976. Recently he received the American Education Award, the education industry's highest honor, for his work as Voyager coordinator.

Bumper Crop

Each June for many years it has been a pleasant custom for *E&S* to pay tribute to those members of the faculty who became emeritus. Usually this has been a matter of honoring 3 or 4 people — and once or twice there have been as many as 8 or 9. This year, for reasons that include changes in the federal and state retirement laws and in the Institute's own retirement policies, a number of faculty members have chosen to retire earlier than they otherwise might have. "Retirements 1980-1981" on page 24 reflects the results by honoring no less than 13 members of a very special kind of graduating class.

Fun and Profit

Usually new faculty members are given some time to settle in at Caltech before being pounced on to deliver a Watson Lecture or write something for *E&S*. But interest in Jay Bailey's area of biotechnology was so great that he was here barely six months before finding himself in Beckman Auditorium in January speaking on "Biotechnology for Fun and Profit." And an article adapted from that talk appears on page 13, only a year after Bailey arrived as professor of chemical engineering.

Bailey came from the University of Houston, where he had been on the faculty since 1971 and was also associate dean of faculties for research for two years. His BA and PhD degrees are from Rice University, and he has been awarded a Camille and Henry Dreyfus Teacher-Scholar Award and the Allen P. Colburn Award of the American Institute of Chemical Engineers. His textbook, *Biochemical Engineering Fundamentals,* is used in almost every biochemical engineering course in the United States and in 65 other countries.

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An Interview with Marvin Goldberger

President Marvin Goldberger has been at Caltech for almost three years. What problems has he found here, and what changes has he promoted? What does the future hold for Caltech? What does being president of the Institute entail, and how does he like it? These are some of the questions Caltech faculty and students keep asking, so E&S *invited a representative group to interview the president and get his answers.*

The interviewers: Tim Brazy, president of ASCIT; Eric Davidson, professor of biology; Norman Davidson, professor of chemistry; David Goodstein, professor of physics and applied physics and chairman of the faculty; Dan Kevles, professor of history and executive officer for the humanities; Albert Lin, chairman of the Graduate Student Council; John List, professor of and executive officer for environmental engineering science; Bruce Sams, writer for The California Tech*; Gerald Wasserburg, professor of geology and geophysics; and James Workman (BS '57, MS '58), president of the Alumni Association.*

DAVID GOODSTEIN: When Harold Brown was interviewed in a situation similar to this in 1972, he was asked what had been done since the beginning of his administration. Some of the accomplishments that he listed sound a bit strange now. Just to take one example, one of his accomplishments — of many — was splitting Physics 2 into two pieces. One of our accomplishments recently has been repairing the split. In view of that, what has been accomplished during the time you have been here and how will those things look ten years from now?

MARVIN GOLDBERGER: It's hard for me to separate what I can legitimately claim to have accomplished from things that already had a certain momentum before I arrived — and would have happened whether I was here or someone else. Coeducation was a fact when I came here; a concern about the addition of women to the faculty existed; quality of teaching has always been a concern; quality of student life has been a concern; and the extent to which I have made contributions in any of those areas is hard for me to judge.

I'm pleased by the fact that there are now two tenured women faculty members, and five or six non-tenured women, and that offers have been made to many other women. I'm also pleased that the percentage of women in the freshman class for the past few years has risen to 17 percent, and this year applications from women are up

15 percent, whereas applications from men are up only 9 percent.

Another area that I've devoted a good deal of attention to is an effort to strengthen the humanities program, and I'm pleased that we have made one senior appointment of a Dreyfuss Professor in humanities and we have an offer out to another senior humanities professor. So I feel that we have made some significant progress in that area.

Martin Ridge, who is a senior historian at the Huntington Library, now has a joint appointment with Caltech, and that has served along with several other appointments to greatly strengthen the traditional bonds of friendship between Caltech and the Huntington Library.

I sense a renewed interest and attention to issues of undergraduate teaching. The conference that was held last year involving students, faculty, and alumni was a very positive event, one we should probably repeat sometime in the future to see whether the customers feel that there has been progress.

JOHN LIST: Since I came here 18 years ago, there are 13 new buildings on campus. The number of faculty in that period of time has grown a relatively minuscule amount, which means that the faculty has to bring in an ever-increasing amount of research money. Do you anticipate there are going to be another 13 new buildings in the next 18 years?

GOLDBERGER: For the immediate future, there are only a very small number of building projects that we are even beginning to talk about. I believe that we are now more careful than in the past to make sure that when buildings are built a suitable endowment is provided for their maintenance. Otherwise, new buildings — marvelous gifts though they may be — eat you alive.

As far as future building plans are concerned, there are three or four conceivable building operations that I can see on the horizon. The first, though not necessarily the first to be completed, is going to be an athletic facility. We are going to do something about the athletic facilities, somehow.

GERALD WASSERBURG: Why? Building athletic facilities hardly seems that important.

GOLDBERGER: I think it is very important. A growing number of students, faculty, and people in this community are extraordinarily interested in fitness, in physical well-being. At Caltech there is a severe shortage of facilities for women. Our swimming pool is so crowded that if you want to go swimming at noon, you have to wait 45 minutes to force your way into the pool. We need another swimming pool, and there isn't a single squash court on this campus.

Now another area of concern is a truly adequate student union facility to house a whole flock of activities that are now largely unavailable on this campus. The student unions at places like Illinois and Wisconsin, for example, are a real focus for student life. In Pasadena, which is not really a college town, I think we have an obligation to provide better facilities for improving the quality of student life than those we currently have.

Another possible building project has to do with more housing for graduate students in the immediate area. These might conceivably be combined with housing for young faculty. Making it possible for faculty to have houses in the immediate neighborhood of Caltech is one of the most important things we can strive for. The atmosphere at a campus where people frequently walk over to their offices in the evening and have contact with their students is quite a different atmosphere from that of an urban campus, where by 5 o'clock in the afternoon everybody is gone. So I am eager for people to live as close as possible and for a genuine community.

We are also going to do our very best to renovate and modernize existing facilities, because that can be done at a cost which is about a half or a third of the current cost of building new structures.

WASSERBURG: The historical circumstances that led to Harold Brown's efforts to maintain some form of solvency within the Institute have resulted in a retrenchment in the staff of maintenance and technical personnel on campus. The effects have been next to disastrous — both in terms of number and variety of skilled people necessary to maintain the facilities and in the ability to pay them so they stay. Have you considered how to keep physical plant facilities staff at a level of adequate competence and dedication to maintain those buildings we have and those which some would have us have?

GOLDBERGER: The dramatic cuts in the size of the staff that Harold Brown had to institute as a result of financial stringency have stretched the staff practically to the breaking point and maybe a little bit beyond. In a number of areas the staff support is inadequate, and I would foresee a certain growth in the size of staff. A level of salaries that will compete successfully with local firms and keep the kind of dedicated people we now have is one of our highest priorities. Last year we got a variance from the Council on Wages and Prices so that we could give an anomalously large salary increase. Attracting and keeping a competent staff is something we take extremely seriously.

NORMAN DAVIDSON: How do you foresee the overall financial situation of the Institute and what opportunities for growth and improvement do we have?

GOLDBERGER: Right now is one of the most difficult times to project the financial situation for the future. One important factor is the continued vitality of JPL and the level of activity that can be expected there over the next few years. The fee from the operation of that laboratory feeds directly into our general funds and is a very important component of our income, but the outlook for the continued support of the deep space program — which is the cornerstone of JPL activity — is certainly cloudy. The support for the lab's energy work is also quite uncertain at this time. Fortunately, we have over the years built up a "rainy day fund" to compensate for a catastrophic loss of JPL. That, of course, wouldn't happen overnight but over a period of years.

As you all know, there are certain areas, primarily in the behavioral and social sciences, that are now being seriously cut back, at least in the projections of the National Science Foundation budget. Those cuts are going to have an impact on us. The physical sciences budgets seem to be holding up fairly well. If there is a general cutback on direct federal support of research, all universities will suffer, but I think we may suffer a bit less than most of them. One development somewhat on the bright side is a renewed interest on the part of a number of industries to become seriously involved in the support of research at universities in general and at Caltech in particular. We're trying to be as receptive as we can to those approaches, recognizing that just as there have been fears and concerns about becoming too heavily involved with the federal government there are fears and concerns about becoming too beholden to industry in ways that might torque our research interests and our fundamental purposes.

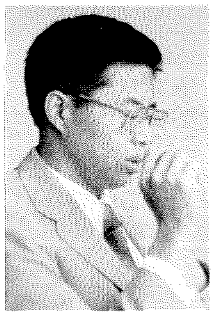
I'm very pleased by the fact that the trustees have become much more seriously involved and concerned about the financial state and outlook for the Institute and are throwing themselves into our development plans for the future.

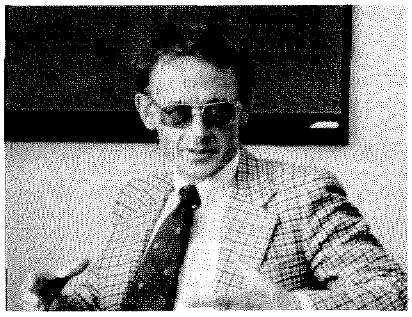
ERIC DAVIDSON: Among the specific proposals of the administration in Washington is severe reduction of the funds to support graduate education. Our sources in the National Institutes of Health tell us that training grants are liable to be very adversely affected as well as postdoctoral fellowships, which provide support for many of the people doing research in biology and chemistry. The last time anything like this happened, during the Nixon era, the Caltech administration advised the divisions to find ways of paying for more student and postdocs on their already-stretched research grants. When push came to shove in our division, we just decided we weren't going to take as many graduate students and then things relaxed. I wonder whether you've given any thought to the possibility of helping with both the graduate education costs and postdoctoral support that would otherwise have come from funding agencies in Washington.

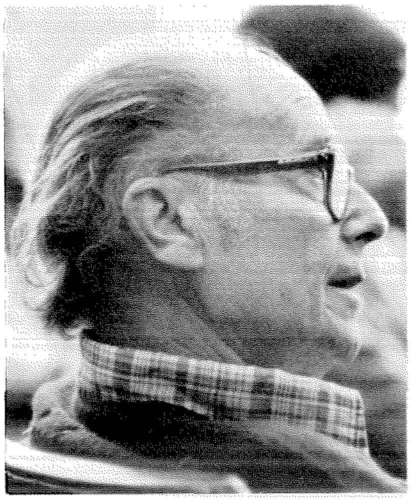
GOLDBERGER: I don't think we've really addressed the full magnitude of that problem yet. For a long time the number of graduate and postdoctoral fellowships associated with the NSF has been so negligible in the physical sciences that there's never been any source except research contracts. The situation in the biological sciences is certainly somewhat different. We're fortunate at the present time in having some new funds available from a very generous gift from Myron Bantrell for a program of postdoctoral fellowships. But we're going to have to scurry very hard to try to make up for the shortfall in the biological and life sciences if the worst happens. I don't know how we are going to do that because the amounts of money involved are rather horrendous.

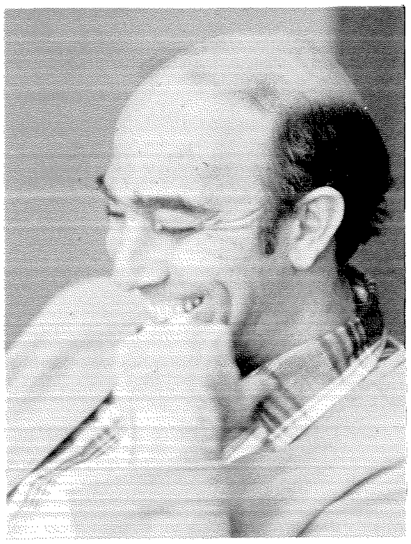
There's a similar threat, of course, for undergraduates in the cutting off of the student loan program, and we're taking unilateral action to try to provide funds for a loan pool. We hope to be able to loan students money at a reasonable interest rate so that students, particularly from middle-income families, have a fighting chance of coming to Cal-


Gerald Wasserburg and Tim Brazy

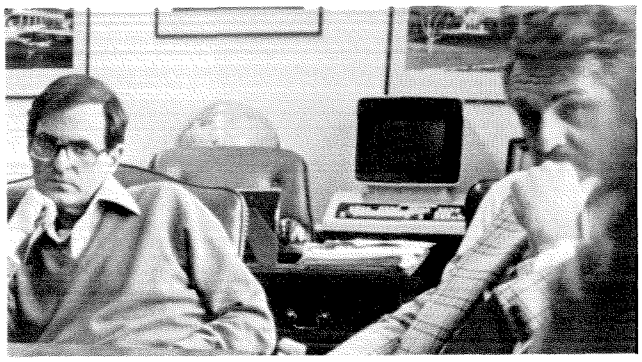

Albert Lin


John List


Norman Davidson



Dan Kevles



David Goodstein and James Workman

Eric Davidson and Bruce Sams

tech. But student aid, both undergraduate and graduate, poses an incredible problem to us. We face a loan shortage next fall for undergraduate students somewhere in the neighborhood of $400,000, which is a lot of money.

ERIC DAVIDSON: Supposing the training grants really are more or less cut, would your recommendation to the various divisions be just to take the graduate students that they can afford from other sources, or would you expect that the central administration would be able to step in with aid?

GOLDBERGER: I hesitate to make a flat statement about it. The question of how many graduate students one should have is always a very difficult one. Any study that I have ever seen comes up with the same answer: The right number is exactly the number that you have at the moment. I would be very unhappy to see the size of the graduate body here shrink appreciably at this time. We do a good job with the size of the graduate body that we have, and conceivably if we had the funds it might be larger. But I would prefer not to shoot from the hip as to what we might have to do.

ERIC DAVIDSON: If the administration in Washington is successful in blocking funds for some of JPL's deep space explorations, energy research, and other applied research, it will be very hard on the technological machine that exists at the lab. What are your thoughts on alternative uses for that machine?

GOLDBERGER: Well, one could imagine JPL becoming a laboratory with a number of specialties that would attract the research interests of various industries. These industries might be able to capitalize on both JPL's talent and its facilities rather than setting up their own independent research operations. A program was designed to set up generic research facilities that would be of interest to a whole class of industries. I think this program is targeted for budgetary extinction, but it could rise again. And maybe we can re-invent it. You know, we have an example of such an activity here on the campus in the form of the Silicon Structures Project. And that idea may be clonable. In fact, we're now considering trying to clone it in some other areas.

JAMES WORKMAN: Terms like "independent" and "private" institutions are used to describe Caltech, yet you rely very heavily on government funding for your annual operating budget. Are you terribly concerned about that and the influence the government may have because of the funding?

GOLDBERGER: I am terribly concerned about it, but not so much because I fear evil influence. One of the fears, of course, is the capriciousness of government funding, as we're witnessing particularly this year. The failure of government to support universities, in spite of all the evidence of the importance of continuity and commitment, the failure to recognize that you can't turn things on and off, disturbs me mightily. If you include our income from JPL, approximately 60 percent of our budget is dependent upon the federal government. It's not clear how we can reduce that significantly, but we can make some reductions, and I am very anxious that we do so because it makes us less subject to the vagaries of Washington budgets.

LIST: In that connection, I get the feeling that we don't spend a lot of money in raising money. Is that really the case?

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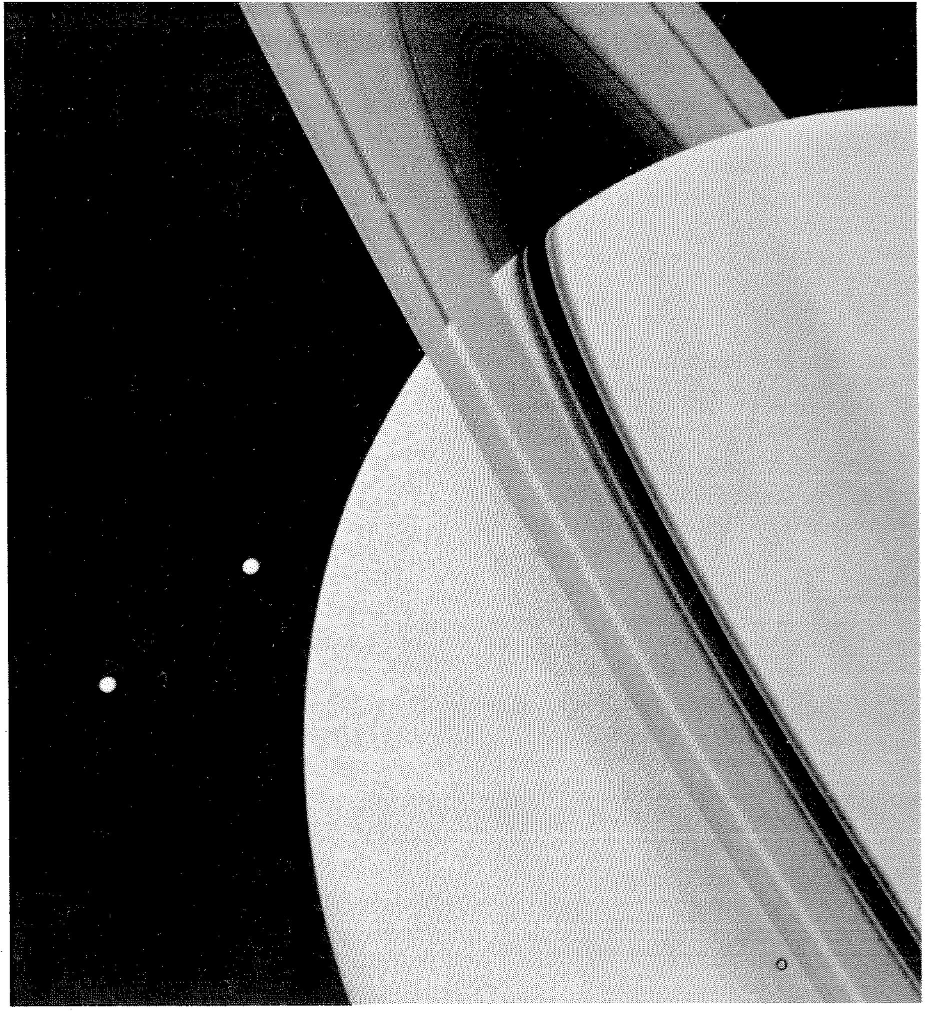


Voyager 1 at Saturn

An Encounter with a Multi-ringed Giant

by EDWARD C. STONE

The Voyager project, a national enterprise involving several thousand individuals who built the spacecraft, several hundred at Caltech's Jet Propulsion Laboratory who fly it, and approximately a hundred scientists who are analyzing the data, began, in a sense, with one man and his telescope. In 1610 Galileo turned that telescope toward Saturn and observed some odd appendages, somewhat like cup handles. It wasn't until about 45 years later that Huygens with an improved telescope deduced that Saturn had a ring. He saw it as a single ring, a solid structure somehow suspended around Saturn. It was another 21 years before Cassini found that there wasn't *a* ring, but two rings around Saturn (the division between them is called the Cassini Division). Since that time, three more rings were reported by Earth-based observers, and Maxwell proved that the rings could not be solid structures but must consist of a large number of small bodies in orbit about Saturn.

Only a short time ago, in 1979, the Pioneer 11 spacecraft visited Saturn and discovered the F-ring, making a total of six known rings at that time. That number changed dramatically with the Voyager spacecraft, which visited Saturn in November 1980 and sent back many surprises in its close-up views not only of Saturn's rings and its satellites but of the planet itself.

Saturn is one of the giant planets in the outer solar system. Deep inside Saturn is a rocky core about the size of the Earth and approximately five times as massive, but the bulk of Saturn is hydrogen, the lightest element. With so much hydrogen, the density of the planet is only seven-tenths that of water, so that if there were an ocean large enough, Saturn would float with three-tenths of its volume above the surface — a remarkably low-density giant planet.

Saturn's atmospheric dynamics, or weather, was one area that Voyager was scheduled to study, as it had already done with Jupiter's atmosphere. Jupiter has very pronounced alternating zones of white clouds separated by dark bands or belts. The white clouds form when the atmospheric gases, which become colder as they rise from the warmer interior, reach the altitude at which it is cold enough for ammonia to liquefy or freeze. Voyager's view of Saturn showed a much more subdued cloud structure but still with evidence for belts and zones. Since Saturn is twice as far from the sun as Jupiter, it is colder, and the altitude at which the ammonia freezes is deeper in Saturn's atmosphere. The clouds are thus obscured by considerable haze in the overlying atmosphere with the result like that on a smoggy day in Los Angeles — a great deal of contrast and color is lost. But it is still possible to improve our basic understanding of weather processes by a comparative study of the wind systems of Saturn and Jupiter.

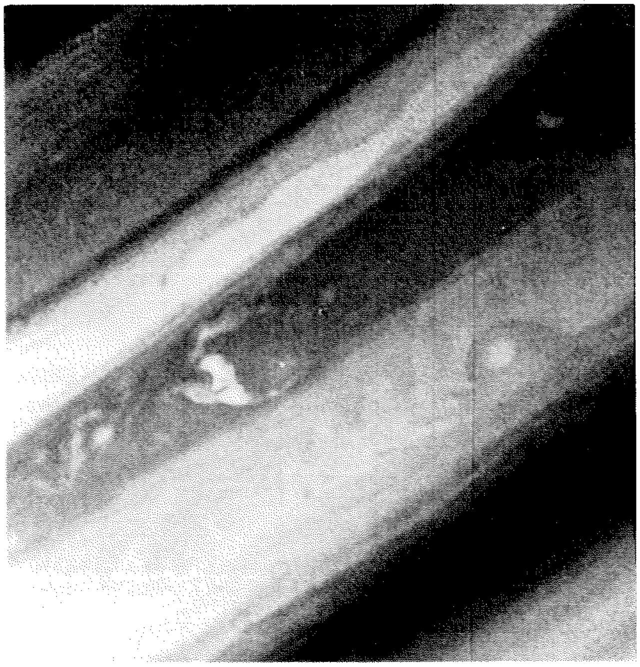
Before Voyager, Caltech's Andrew Ingersoll, professor of planetary science, developed a model to explain the formation of these belts and zones. He postulated an upwelling of gas from the interior of the planet, and the consequent formation of the ammonia clouds results in the formation of an elevated ridge in the atmosphere which would like to flow directly north or south into the adjacent lower belt regions. Because of the rotation of the planet, however, wind is deflected much as here on Earth, resulting in alternating eastward and westward jet streams on opposite edges of each zone, as suggested by ground-based observations.

So even before the encounter with Jupiter, we expected to find alternating eastward and westward jets associated with the white cloud regions. Voyager 1 found that in the equatorial region there is a 200 mph eastward wind extending from about 10 degrees north latitude to about 10 degrees south. With increasing latitude, there are alternating westward and eastward jets that are indeed associated with the white zone cloud structure.

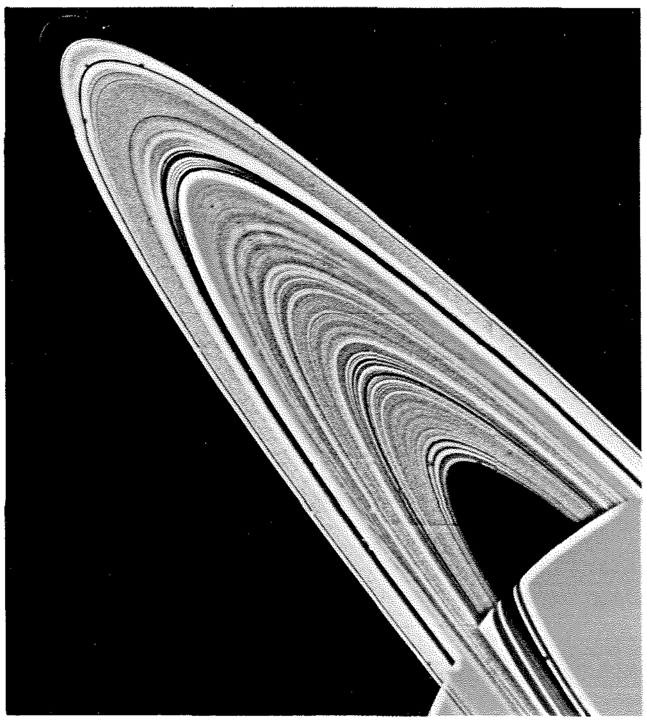
Because of the apparent lack of contrast on Saturn these zone patterns weren't as obvious. Fortunately, Voyager's images can be enhanced by making regions that are somewhat red, still redder, and regions farther north, which are a bit blue, bluer. With such techniques, distinct features emerge from the rather bland images — discrete white clouds, which are likely the result of conductive upwellings, and wavy, high-speed jet streams. There is even a small red spot which is about the size of the Earth.

Saturn's clouds can be located in several successive enhanced images, their displacement and velocity measured, and a wind pattern derived. These patterns appear very different from those on Jupiter. Not only does Saturn have an eastward wind speed at the equator of almost 1000 miles per hour, about half the speed of sound, but the eastward winds occur from about 40 degrees south to 40 degrees north. It is only above this latitude that the pattern of alternating westward and eastward jets observed at lower latitudes on Jupiter begins to appear on Saturn. In a comparison of these winds with the cloud patterns on Saturn, there is almost no correlation at all. The winds are apparently independent of the formation of the belts and zones, suggesting that the correlation found on Jupiter was not a fundamental one and that the pattern of jet streams is the result of processes other than cloud formation.

The apparent differences in the weather systems, which are related to the dissimilarities between the planets themselves, provide clues to the origin of the jet streams. Jupiter and Saturn are different in a several key ways; one of these is their interior structure. They each have a rocky core about 4000 miles in radius, very similar to the size of the Earth and not unlike each other. Jupiter's central temperature is about 43,000 degrees Fahrenheit; it's somewhat colder inside Saturn. However, the bulk of both planets is a deep envelope consisting mostly of hydrogen. In the outer regions of both planets, the hydrogen is of the common molecular form (H_2), but at higher pressures deeper inside, hydrogen becomes a metal — not a solid, but a metal in the sense that it conducts electricity. Although Jupiter and Saturn both have an inner core of electrically conductive hydrogen surrounded by molecular hydrogen, the metallic core is much larger on Jupiter than it is on Saturn. Ingersoll has suggested that the observed jet streams are the result of large-scale motions in the deep layer of molecular hydrogen and that the size of these motions may be affected by the size of the metallic hydrogen



Saturn's atmospheric structure, while less pronounced than Jupiter's, shows alternating belts and zones and discrete smaller-scale features within the bands. There seems to be, however, no correlation between these zones and Saturn's wind patterns.



This computer-enhanced mosaic of Saturn's rings shows their extraordinary complexity. Literally hundreds of individual ringlets make up the B-ring, and even the Cassini Division is filled with rings. The F-ring is barely visible to the left outside the A-ring.

core. Whether or not such a model can explain both the Jupiter and Saturn wind patterns is a quantitative question that has not yet been answered.

Another possible explanation is that the jet streams are confined to a relatively thin surface layer. In this model, the upwelling of the deeper atmosphere, which is caused by the high temperatures at the planets' centers, results in localized upward bumping or punching of the bottom of the thin layer in which the jet streams occur. Each such upward bump generates a small whirling eddy in the thin layer, which eventually combines with countless others to generate the very strong wind systems. In this case, it would have to be shown that the production and distribution of these eddies is significantly different on Saturn than on Jupiter. Again, this is a quantitative question that has not been addressed.

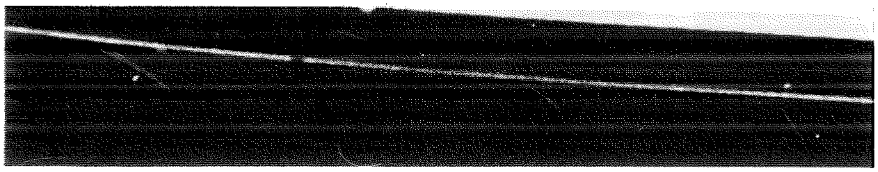
Voyager also delivered a number of surprises and raised many new questions about Saturn's rings. From Earth we can see the A-ring and the B-ring with the Cassini Division between them. Among Voyager's surprises was that the Cassini Division was not a gap at all but was filled with five rings, each about 300 miles wide. At still higher resolution those rings were found to be further subdivided into still more rings, resulting in several dozen individual rings altogether. The many particles comprising the rings are all in circular orbits around Saturn — except for the particles in a narrow ring in the Cassini Division and in another in the C-ring which are in eccentric orbits.

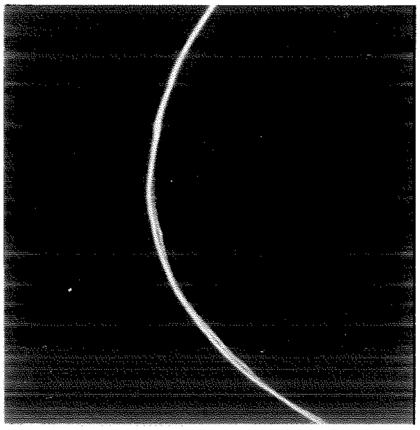
The next surprise was the B-ring, which from Earth appears as a very bright, uniform region. Voyager's enhanced images show that the B-ring is anything but uniform. It contains hundreds of individual ringlets, separated by narrow gaps through which the planet can be seen quite clearly, except in some regions where the rings are too thick or the gaps too narrow. From Earth, the B-ring appears uniform because of the unfavorable viewing geometry and great distance, although there are a few sketches by ground observers which indicated structure in the B-ring.

There were also a number of surprises associated with the very narrow F-ring, which Pioneer 11 discovered just 2800 miles outside the A-ring. Voyager found three new satellites, one just outside the A-ring and one on each side of the F-ring. Those satellites are probably why the F-ring and the edge of the A-ring are so sharply defined. Peter Goldreich, Lee A. DuBridge Professor of Astrophysics and Planetary Physics here at Caltech, and Scott Tremaine, formerly Richard Chace Tolman Research Fellow in Physics at Caltech and now associate professor of physics at MIT, published a theory some years ago to explain the rings around Uranus, which were discovered in 1977. These are very narrow rings, much like Saturn's F-ring. Their theory postulated the existence of a pair of satellites, one just inside and another just outside each ring. Because the satellites are in orbit around the planet, the closer object has to move more quickly and the one farther out more slowly. That means that the ring material is overtaking the outer satellite and, as a result, it gives the satellite a small push, increasing its energy. The ring material thus loses energy and falls in toward the planet, while the outer satellite moves outward. The inner satellite, on the other hand, is moving more rapidly than the ring material, so it's doing just the opposite — giving up energy to the ring material and pushing it outward. Thus, between the two satellites, the ring material is constantly shepherded back into place.

When this theory was proposed for the nine rings of Uranus, some felt that it was too complicated because in order to explain nine rings it was necessary to postulate the existence of 18 small satellites. Now that two such satellites have been discovered on either side of the F-ring, the theory would seem to be applicable at least in this case. In fact, the theory may also explain the multiple ringlets in the B-ring which may result from literally hundreds of tiny satellites in orbit around Saturn, shepherding the smaller ring particles into narrow rings. Unfortunately these small satellites, which may be only 1 mile in diameter, have not yet been found, since not a very large fraction of the ring was imaged at the high resolution required to find such small objects. But the fact that Voyager found the two shepherding satellites for the F-ring is certainly very suggestive that this simple, or perhaps not so simple, physical picture may also explain the structure in the B-ring.

Another interesting phenomenon that this theory might explain is the so-called "braid" in the F-ring. Rather than





Voyager discovered on either side of the F-ring two small satellites, whose presence had been previously postulated as being responsible for holding the ring material in narrow orbits. They may also deflect the ring material slightly, causing the "braid" (right) where there appear to be three separate ten-mile-wide strands.

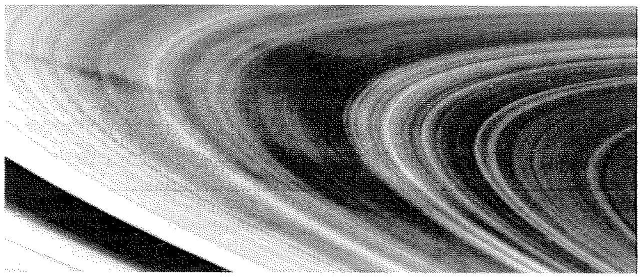
keeping the material in the F-ring nicely shepherded in perfectly circular orbits, the satellites may tend to deflect it slightly, resulting in a small ripple in the otherwise circular orbit. Rippling is exactly what the F-ring seems to do. In one place in the F-ring there are three separate strands, each about 10 miles wide. Two of the strands seem to intersect each other, forming a braided appearance.

The visible material of the F-ring is known to be very fine, a few ten-thousandths of an inch in diameter, because it scatters sunlight forward very efficiently. Clumps or knots in the braids are possible sources of the fine material, which is most likely charged with static electricity due to action of sunlight and the Saturnian radiation belts. In this case the dust will also be interacting with Saturn's magnetic field, which is rotating with a period of 10 hours and 39 minutes. To what extent this interaction contributes to the braiding of the F-ring is a detailed quantitative question which has not yet been answered. But despite some of the press reports last November, there is nothing about the rings that violates physical laws. It is rather a question of understanding which physical laws are responsible for the observed effects.

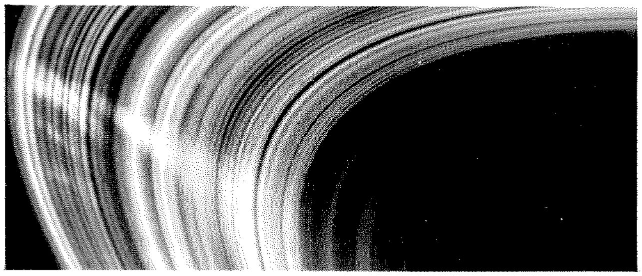
Still another surprise in the rings was the mottled dark regions in the B-ring, which were immediately described as "spokes" because of their radial appearance and because they seem to rotate with the B-ring. Although the nature of the spokes could not be discerned when first observed far from Saturn, from close-up approach images it was clear that the spokes were regions where the ring material appeared darker but not thinner or full of holes. However, the appearance of the spokes changed dramatically when viewed from behind Saturn. Since the sun and the spacecraft were then in opposite directions from the rings, the images were formed of forward-scattered sunlight and the spokes appeared bright rather than dark. Since scattering of sunlight preferentially in the forward direction is characteristic of very fine material, the spokes must be clouds of small particles — probably not rock dust but very fine ice particles a few ten-thousandths of an inch in diameter.

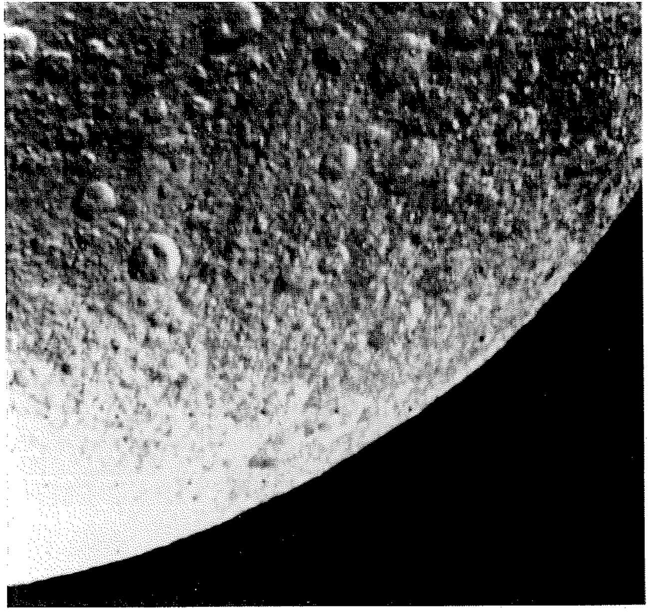
The mechanism that generates the irregularly shaped spokes isn't fully understood. They're not permanent; individual spokes appear — perhaps as the result of levitation of the small particles above the ring plane — and disappear in a matter of hours. New spokes seem to appear as the ring material comes out of the shadows behind Saturn suggesting that electrostatic charging may be involved. When objects are in sunlight, electrons are released from their surfaces by a photoelectric effect. In the dark the process ceases, perhaps resulting in a change in the electrical characteristics of the fine ring material (in a manner that is not yet well understood) that causes these clouds to form. Then they dissipate as they rotate around Saturn because the end of the spoke nearest the planet rotates more rapidly.

One other indication that electrical activity may be important in the rings comes from the two radio receivers which detected static electrical discharges of increasing intensity as the spacecraft approached the rings. The electrical discharges aren't from the planet because the lowest frequencies would have been trapped below Saturn's ionosphere and would not have been detected by Voyager. This suggests that the static discharges are coming from the rings and may be related to the electrical charging and discharging of the icy material that forms the rings.

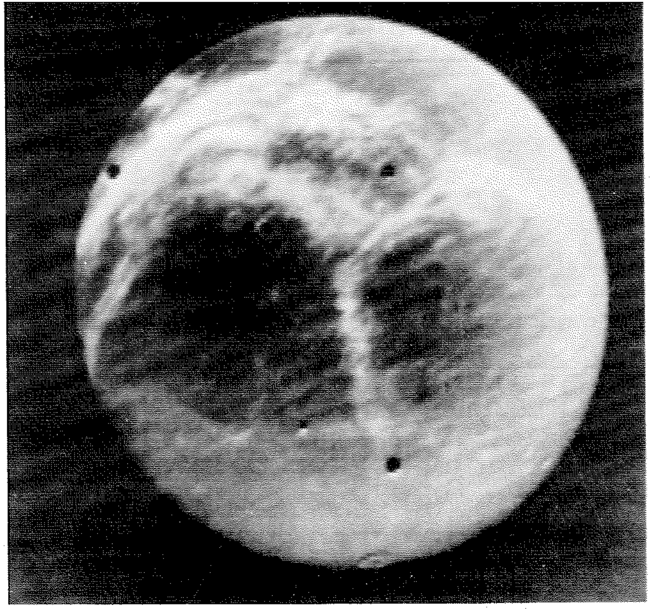


The rotating spokes of the B-ring appear dark in backscattered light (above) and bright in forward-scattered light (below) viewed from behind Saturn, indicating that the spokes are clouds of very small particles a few ten-thousandths of an inch in diameter.





Saturn's satellite Rhea is heavily cratered like the Earth's moon. The larger craters seen here in Rhea's ancient surface are about 45 miles across; the peaks in the centers are formed by the rebound of the surface after impact.



Dione's surface is marked by bright, wispy regions, which may be frost evolving out of extensive fracture systems. Rhea also exhibits these white patterns, indicating that there has been some sort of geologic activity since the two satellites formed.

Additional information on the rings was obtained when the spacecraft was behind the rings, as viewed from Earth. The attenuation of the radio beam transmitted from the spacecraft provided a measure of the amount of material in each of the rings and the size of the larger particles. The radio transmission indicated an effective size of 6 feet in diameter for the larger particles in the C-ring, 24 feet in the Cassini Division, and 30 feet in the A-ring. Since many, if not most, of the particles are somewhat smaller, further analysis is required in order to determine the range of sizes of the ring particles. What they are made of is still unknown, although ground-based studies indicate that they are at least covered with ice.

Voyager also sent back some fascinating views of Saturn's satellites. Saturn has five icy inner satellites which are a new size class; from the outside in they are Rhea, Dione, Tethys, Enceladus, and Mimas. Much farther from Saturn is the giant satellite Titan, about the size of the planet Mercury and the Galilean satellites Ganymede and Callisto. Saturn's icy inner satellites are much smaller, although not as small as typical asteroids. This is an interesting size class because some of the satellites are small enough that they might contain very little radioactive material and therefore may never have been hot enough to melt the ice. Thus, they may never have differentiated, that is, they might not have rocky cores because the ice and rock may not have separated.

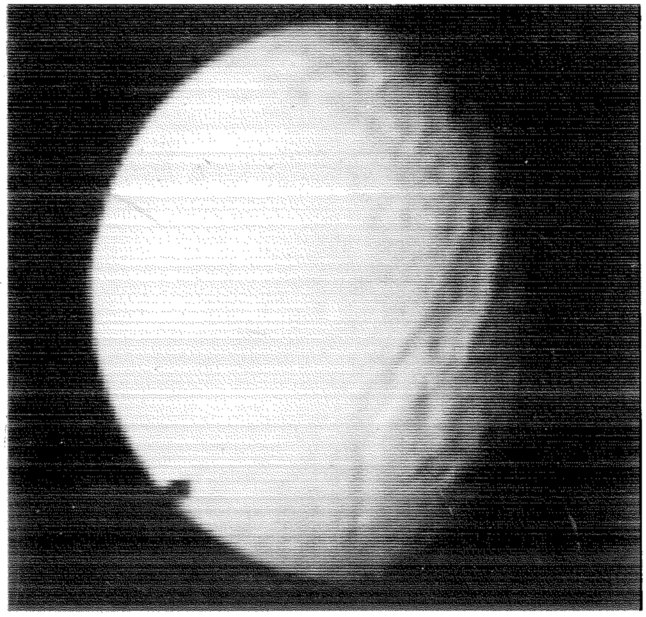
Rhea has a diameter of about 950 miles, which is large enough that melting of the ice could have occurred early in its formation, allowing the rock to sink to the center before the water refroze. The resulting icy surface was expected to be a uniform, heavily cratered old surface.

Rhea appears not to be quite as expected. From a distance, Voyager observed long wispy white regions — not a uniform surface at all. As the spacecraft came closer it was obvious that Rhea is indeed heavily cratered, with craters on top of craters, indicating a very old surface that formed when Rhea itself initially formed. In some regions, however, there is an absence of larger craters suggesting that those regions were resurfaced, possibly by flooding, shortly after the formation of the largest craters. Apparently Rhea did not form and then quickly freeze into a geologically dead object, but did manage to maintain some activity long enough to have erased some record of its early history.

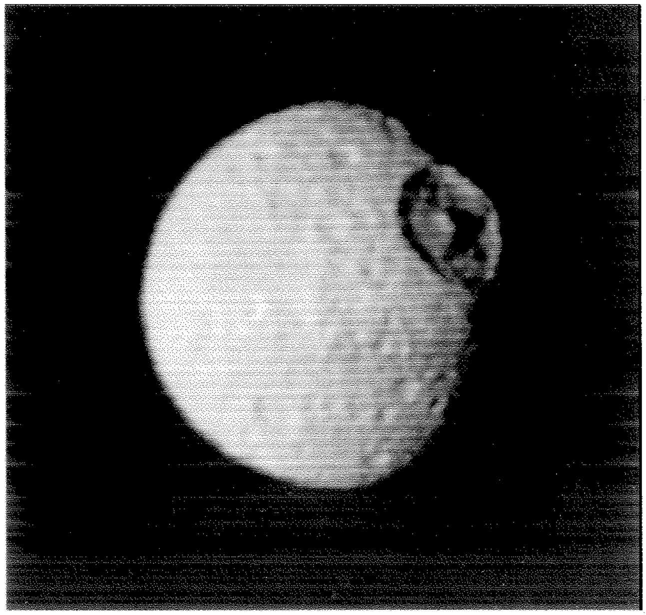
Rhea's craters are characterized by central peaks formed by the rebound where meteor impact punched the icy surface and depressed it. Because the temperature and the gravity are so low, the peaks have remained in the basic form that can be seen today. The craters are not perfectly circular. This suggests that the shock wave that traveled out forming the basin traveled in an inhomogeneous medium — probably large blocky regions of ice. The characteristic irregularity of these features is undoubtedly telling us something about the somewhat deeper surface material properties on Rhea.

Somewhat closer to Saturn, Dione and its twin, Tethys, are small enough — about 700 miles in diameter — that they might not have melted and might have rock and ice still mixed in whatever state they accumulated. If the rock had accumulated first, it would still be in the center, but if the ice and rock were mixed together as they accumulated, they would have remained that way. Dione's surface is not uniform but, like Rhea's, is marked with large, white wispy regions. There is also a region that was flooded late enough that not many smaller impact craters have since formed. This is a region that is certainly the result of some form of geologic activity occurring after the original crater formation on Dione, again an indication that these objects did not die at the time they were formed but remained active for some time.

It is also possible that the white wispy areas seen over a



Tethys has a valley, visible here down the right half of the satellite, which is about 600 miles long and about 30 miles wide. Like the white, wispy regions on its twin, Dione, this is indicative of internal stresses since formation.



A unique feature of Mimas is an enormous crater more than 80 miles in diameter and three to six miles deep. The central peak is clearly visible, and there are also many smaller, bowl-shaped craters like those on Tethys.

major part of the surface of Rhea and Dione are really very extensive fracture systems. The white is frost — a very white, bright frost of water that has evolved from the interior. This is not at all in keeping with the idea of a very simple, geologically uninteresting iceball.

Tethys has a 600-mile-long valley, perhaps 30 miles wide, that is also likely the result of some kind of internal stresses. Tethys is also cratered, although the craters tend to look more bowl-shaped than the craters on Dione and Rhea. The average density of Tethys is very near that of water ice.

Enceladus, closer yet to Saturn, is only about 300 miles in diameter. We had thought that Enceladus, even though it's small, might still be warm and that perhaps the rock had separated and formed a core. We expected that the surface might be more like that of Europa at Jupiter, that is, a smooth surface, kept young by tidal heating effects induced by other satellites. Unfortunately we did not observe Enceladus very well with Voyager 1. However, an image with the same resolution as one of Tethys that reveals enormous craters shows none on Enceladus. This certainly suggests that Enceladus might not have any substantial cratering. Voyager 2 will fly much closer to Enceladus and may be able to determine whether or not Enceladus has been recently thermally active, kept warm by tidal effects or other not yet identified processes.

Mimas is almost the same size as Enceladus, but we thought it would have a cratered surface and might not be differentiated. Indeed, Mimas has many bowl-shaped craters like those on Tethys, although there are also some linear features, an indication that this surface has also been stressed in some way. Mimas does, however, have one unique feature — a crater about one-third the diameter of the moon itself and three to six miles deep. Simple calculations suggest that if the object that caused this crater had been much larger, Mimas would have fractured into several pieces. Some of the linear features may be the result of the impact rather than the result of an internal heat source. The other satellites exhibit no evidence of a similar spectacular impact which might have caused their surface modifications.

Titan is in a more familiar size class; it's a planet-sized object. It is large enough so that the ice should have melted and the rock should have sunk to the center. Before Voyager, it was known that Titan had an opaque atmosphere containing at least methane. If that is all there had been, the pressure at the surface of Titan would have been about 2 percent of the Earth's surface pressure. On the other hand, something like nitrogen can't be detected from Earth. It had been suggested that the atmosphere could contain enough nitrogen that the surface pressure might be as much as one or two times the Earth's surface pressure and — if the temperature were low enough — there might be liquid nitrogen on the surface, since at one atmosphere pressure nitrogen liquefies at 77 degrees above absolute zero. It was also known that an opaque haze was being produced by the action of sunlight and trapped radiation on the methane. The larger haze particles probably rain down on the surface, possibly forming a layer several thousand feet thick of frozen organic ices, the result of the photochemistry occurring in Titan's atmosphere.

As expected, Voyager's cameras could not peer through the haze, but the haze layer was somewhat brighter in the southern hemisphere than in the northern, which had a dark polar hood. The difference between the north and south is probably a seasonal one. Saturn and Titan are tilted on their axes just as the Earth is — in fact, more so than the Earth. The Titan year is about 30 Earth years, and when Voyager arrived at Titan, it was the equivalent of the beginning of April; in other words, southern summer had gone and northern summer was approaching.

Since not much could be seen visually, Voyager's other instruments provided much of the key information about Titan. Several experiments depended on the fact that the spacecraft was to fly very close to Titan — about 2600 miles above the thick haze layer. It flew through the magnetospheric wake, thus named because as Saturn's magnetic field and particles rotate with Saturn, they flow past Titan, forming a wake. Voyager also flew behind Titan to view the sunset through the atmosphere, and then the spacecraft disappeared behind the satellite as viewed from Earth, making it possible to probe very deeply into Titan's atmosphere with the spacecraft radio signals.

These measurements located Titan's surface almost 150 miles below the haze layer. The pressure at the surface is about one and one-half times that of the Earth's atmosphere, and the bulk of the atmosphere is nitrogen. The temperature at the surface is -294 degrees Fahrenheit, or 93 K. That is interestingly close to the temperature at which methane can exist simultaneously as a liquid, a solid, and a gas — the triple point. On Earth, water is near its triple point so that it can either rain or snow. In this sense, methane is the water of Titan. Depending on the surface temperature, or perhaps on the season or latitude, the methane on Titan's surface could be solid methane ice; it could be liquid methane in lakes and rivers; or the surface could be a desert with no methane at all. Higher in the atmosphere the temperature drops, just as it does here on Earth, resulting in the formation of clouds of methane ice, from which methane will rain, or more likely snow, back onto the surface.

Still higher in the atmosphere it is somewhat warmer because of the absorption of sunlight by the thick haze layer. Other thinner haze layers occur at altitudes up to 500 miles above the solid surface. Some of these layers are molecular, that is, they're not composed of small particles, but of individual molecules of organic material produced from the methane and nitrogen in the atmosphere.

The chemistry that produces these organic molecules is of special interest. When fast electrons, which are carried along in Saturn's rotating magnetosphere, collide with the top of Titan's atmosphere, they excite the nitrogen molecules which then emit ultraviolet light, which was detected by Voyager. The fast electrons, however, can also break the nitrogen molecule into individual nitrogen atoms or into electrically charged ions. Either the atom or the ion can subsequently combine with methane to produce hydrogen cyanide (HCN), which is a building block for amino acids. Similar chemistry may have occurred here on Earth, eventually leading to the formation of life. Undoubtedly the process does not progress nearly as far on Titan because the temperature is so cold. The HCN itself is further affected by sunlight, which can cause it to polymerize and form large haze particles. So it is possible that the orange haze on Titan is a haze of HCN or some compound related to it. Not all of the nitrogen reactions result in HCN, however. Some of the nitrogen ions are stripped away from the top of Titan's atmosphere, forming a long plume that is carried away by Saturn's rotating magnetosphere.

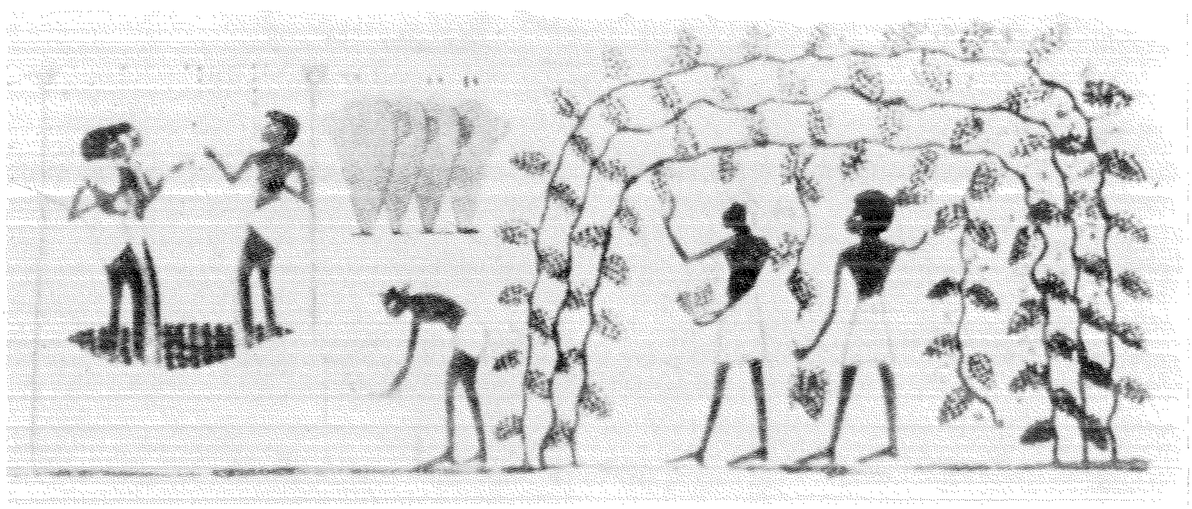
Voyager made a number of other discoveries about the magnetic field and trapped radiation belts comprising Saturn's magnetic field. The discovery of periodic radio bursts from Saturn's rotating magnetosphere provides the first accurate measurement of the length of a Saturn day — 10 hours, 39.4 minutes. As the magnetic field rotates, it carries with it a plasma of electrically charged ions from Titan, the icy satellites, and the rings, forming an equatorial disc about Saturn. There is also a tenuous torus of hydrogen, which glows with ultraviolet light, filling the region between the orbits of Rhea and Titan.

These are just the highlights of what we are beginning to understand from Voyager 1. The spacecraft is now on its way out of the solar system. It has finished exploring the planets, but it hasn't finished exploring the solar system. Voyager's trajectory will take it up out of the plane of the planets toward the heliopause, where the solar wind, which is blowing a bubble in the interstellar medium, ceases. We don't know exactly where that is; it may be anywhere from 30 to 60 astronomical units or more from the sun (the Earth is at 1 astronomical unit). By 1990 Voyager 1 will be some 40 astronomical units from the sun, and it is possible that somewhere in that time Voyager might leave the solar wind behind and for the first time enter the interstellar medium consisting of material coming from other stars. We expect that even before Voyager reaches the end of the solar wind, the cosmic ray investigation led by R. E. Vogt, professor of physics at Caltech, will begin to detect for the first time the low-energy cosmic rays coming from nearby regions in the Galaxy.

As far as the planets are concerned, Voyager 2 will be taking a very close look at Tethys and Enceladus in August. It will also investigate the braided F-ring, taking stereo images in order to determine whether it is in a two-dimensional or a three-dimensional braid. We will investigate the structure of the braiding in the vicinity of the shepherding satellites and search for any changes in the braiding when in Saturn's shadow, as might be expected if electrostatic charging is important. Other new investigations will include observations of a star through the rings, which should provide detailed information on the location and width of each of the hundreds of ringlets.

Although designed just for the four-year mission to Saturn, Voyager 2 will nevertheless head toward Uranus, arriving there in 1986, when the planet's spin axis is pointed at the sun. Uranus is tilted on its side — a most unusual configuration. The nine known narrow F-type rings and five icy satellites form a bull's-eye pattern around Uranus, but if Saturn is any indication, there is much more waiting to be discovered. Extending our hopes still further, Voyager can swing past Uranus toward a 1989 encounter with Neptune and its satellite Triton, a possible twin of Titan.

In the meantime, tune in again in August for the next program from Saturn. □



Biotechnology has been around for a long time, as this example of winemaking in ancient Egypt indicates. (Illustration reprinted from *Chemical Technology*, edited by F. A. Henglein. Pergamon Press, 1969.)

Biotechnology for Fun and Profit

by JAMES E. BAILEY

Biotechnology will be defined here as the application of chemical processes that use either biological catalysts or biological raw materials and that occur in vats or tanks of man's design. We can illustrate each component of this definition with the example of home winemaking, in which the biological raw material is sugar and the biological catalyst is yeast. As the fermentation occurs, alcohol is formed as the major product, along with carbon dioxide and more yeast cells.

This way of making alcohol dates to ancient times. The Babylonians, for example, were brewing beer as early as 7000 B.C. The original manufacture of wine and beer was done on a small scale, but it did not prove too difficult to make fermented alcohol in large casks and vats and probably our ancestors were thankful for that.

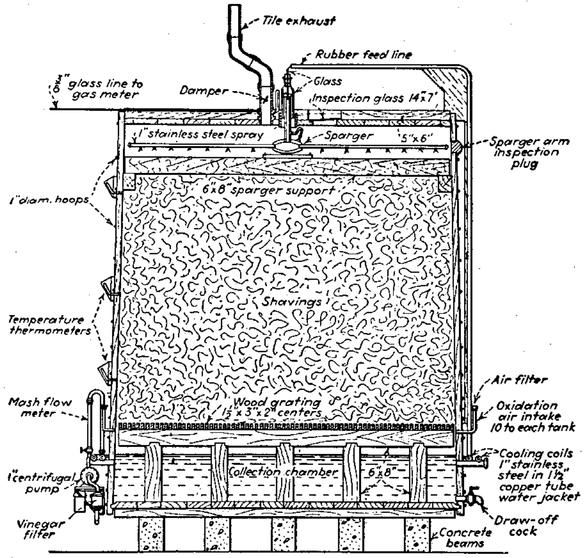
The situation was much different in another very old process of biotechnology — vinegar manufacture. Here early practitioners were not successful in translating the small-scale process (carried out in shallow wooden tubs) into big tanks in order to make large batches of vinegar. Why not? The basic difference is in the chemical reactions involved. In vinegar manufacture, microorganisms grow on the surface of a liquid where they convert alcohol into vinegar. In the process, they require oxygen, and they generate heat. That reaction causes no problems in shallow vats because the oxygen comes from the air over the liquid, and the cells grow as a film on the liquid surface. Any heat that is released is easily dissipated into the atmosphere. Productivity is not increased in a deeper tank because everything occurs on the surface.

An ingenious invention was developed in the early 19th century, however, in order to "scale-up" the process. A large chamber was filled with wood shavings, on the surfaces of which the microorganisms could grow and convert ethanol (alcohol) into vinegar. In this case, the reaction occurs throughout the system by creating on each wood shaving exactly the conditions that are necessary for the microorganisms to carry out the reaction. The heat generated from the reaction in the pile of shavings causes air to flow by natural convection through the bed of shavings,

From *Industrial Microbiology*, 1968, L. E. Casida Jr. John Wiley & Sons.

Early vinegar manufacturing processes used shallow vats; the chemical reactions involved in vinegar production required large open surfaces for oxygen supply and release of heat.

From *Fundamentals of Microbiology*, 8th edition, J. Martin Frobisher. W. B. Saunders Co.



The 19th-century solution to large-scale vinegar manufacture made use of wood shavings to provide surfaces for the microorganisms to convert alcohol to vinegar. Heat generated by the reaction causes air to flow through the chamber by natural convection, simultaneously cooling it and supplying oxygen.

cooling it and supplying fresh air to the bugs.

Something else about this process illustrates an important engineering concept. In the early vinegar process and the making of wine, things are done in batches, but that isn't a very efficient way to manufacture products in quantity. Whenever a vat sits empty and idle, capital investment is being wasted. In addition, someone must be paid to fill the vats, and, when the batch operation is over, further labor is necessary to empty the vats, clean them, and start the process again. It is much cheaper to try to keep the desired chemical conversion processes operating continuously. In the 19th-century vinegar-making procedure, alcohol is sprayed continuously through nozzles on the top and trickles through the wood shavings to produce a continuous flow of vinegar at the bottom.

Conducting these early processes was really an art rather than engineering. In fact, the participation of microorganisms in catalyzing these chemical changes wasn't realized until Pasteur's work in the mid-19th century. It was Alexander Fleming's discovery of penicillin in 1928 that eventually led to the development of a large-scale industrial process. This story has historical interest and also provides an opportunity to discuss several engineering ideas.

Although penicillin was discovered in the late 1920s, it wasn't until the fire bombing of London in early World War II that it was recognized — on a crisis basis — that there was a need to find better treatments for burn victims. At that point intensive efforts were launched to find a way to make large amounts of penicillin. The vessel for the original discovery of penicillin was a petri dish, and it's clear that such a "reactor" is of virtually no value in making practically useful quantities of penicillin. By the early 1940s, the largest available reactor produced about twice as much penicillin in a single batch — in a bottle or flask. There were two ways of growing the mold on this scale: on solid bran or under water.

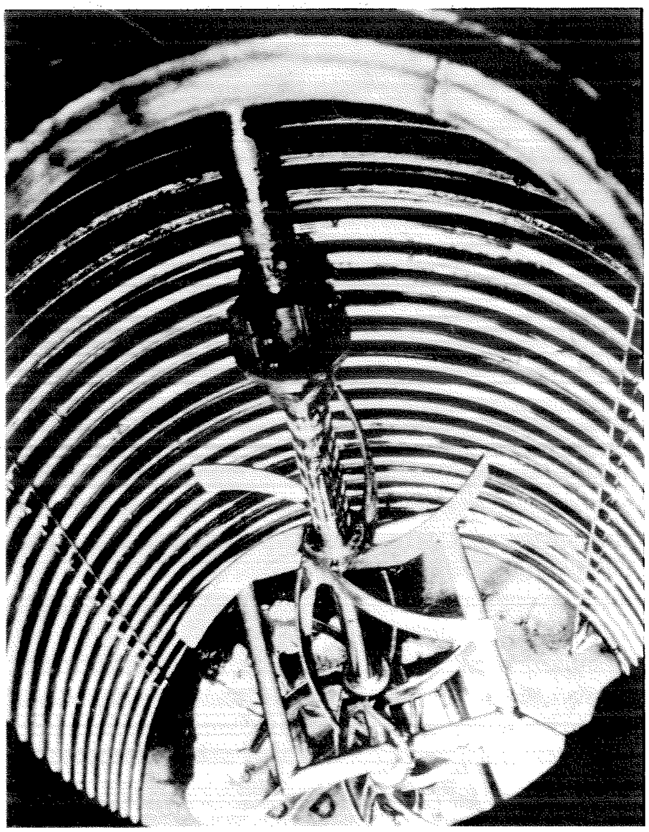
The mold grew readily on bran and produced penicillin, but that process couldn't be scaled up because in a large mass of bran the amount of heat released by the oxidation reaction kills the mold. If the mold is grown under water, controlling temperature is much easier, but there are problems in supplying oxygen to the mold. This consideration was already mentioned for the vinegar process, but there's an important difference between growing penicillin mold and growing the organism that makes vinegar. When vinegar is produced, if any stray contaminating microorganisms happen to find their way into the process, they won't grow well in the alcohol and acid environment. Thus contamination is not a serious problem for vinegar manufacture, and that process can be operated in the open air. On the other hand, the liquid solution that is used to grow penicillin is a very good nutrient for many natural organisms, and consequently they must be prevented from entering the penicillin culture. In the early 1940s, cotton stoppers were used to prevent contamination of flask cultures, but no one knew how to provide a large amount of air to a large volume of growing microorganisms while at the same time keeping the air sterile.

At first, in the absence of any engineering knowledge as to how to make penicillin in bigger batches, a brute force approach was adopted — the *Penicillium* mold was grown in many small bottles. To treat one person with penicillin then required the output of about 40 bottles. But penicillin was so urgently needed that in the space of about a year all of the engineering problems were solved, and the transition was made to growing the mold in agitated vessels holding about 10,000 gallons. By 1944 it was possible to make in one of these tanks enough penicillin to treat 2,500 people, and similar vessel designs remain in many respects the workhorses of today's pharmaceutical industry.

From *The History of Penicillin Production*, Albert L. Elder, ed., published by the American Institute of Chemical Engineers in the Chemical Engineering Progress Symposium Series 100, Vol. 66, 1970.

At first it took the contents of 40 of these bottles to treat one person with penicillin.

From *The History of Penicillin Production.*



By 1944 the engineering problems had been solved, and enough penicillin to treat 2,500 people could be made in one of these 10,000-gallon tanks.

Biochemical engineering was really born with the development of this large-scale penicillin process. Since then, the attention of biochemical engineers has gone beyond physical problems such as aeration and mixing to problems involving the chemical reactions that occur in biological systems and the biological catalysts involved.

The elementary unit of catalysis in biological systems is the enzyme. Enzymes are protein molecules that are synthesized by living cells, but they don't grow or reproduce by themselves. There are many different kinds of enzymes, and they are necessary for life processes.

In biochemical engineering, enzymes are now applied in many ways which relate to the specificity of their catalytic action. If we want to decompose starch molecules to form simple sugars, for instance, a catalyst is needed that works on many different sizes of starch molecules — the enzyme glucoamylase has this property. If we want to remove stains from fabric, it's desirable that the enzyme not care whether the stain is chicken or beef, and several protein-splitting, or protease, enzymes will serve this purpose.

On the other hand, most enzymes are extremely selective, and this is one reason why there is now great impetus in industry to expand application of enzyme catalysts. One example of the use of an extremely selective enzyme is analysis of blood sugar. In the presence of the very complex mixture of different sugars, proteins, and fats in a blood sample, this enzyme will carefully and fastidiously select only the glucose, causing it to form, for one thing, hydrogen peroxide. The amount of hydrogen peroxide liberated can be determined, thus providing an accurate measurement of the glucose concentration in the blood. Other kinds of selective enzymes can be used to cut DNA at specific points.

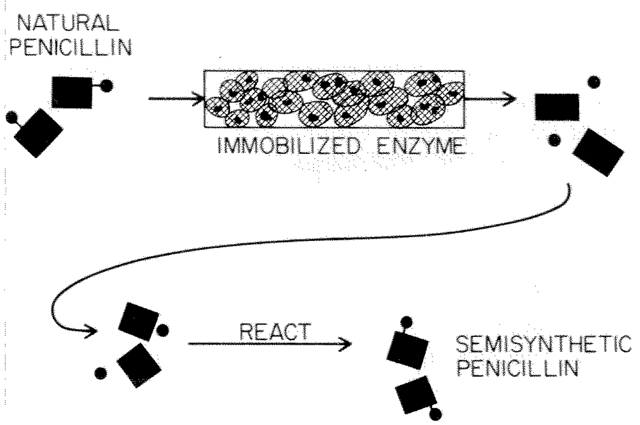
It should be obvious, since enzymes are necessary for all life processes, that they work well at about one atmosphere pressure and at, say, 20 degrees C. Very active catalysis at these temperatures and pressures is of great interest in the chemical industry because most processes there are done at high temperatures and pressures, which require complicated equipment and great amounts of energy. Since energy is becoming increasingly scarce and precious, major efforts are now in progress to discover enzymes that can conduct on a more energy-efficient basis some of the reactions important in the chemical industry.

Until about 1960, all of the applications using enzymes were started by mixing together enzymes and reacting molecules such as starch. At the end of the reaction, the product — here sugar solution — was harvested and used, perhaps by pumping it into a fermentation tank to make alcohol. A major problem with this approach is loss of the enzymes with the product, an often uneconomical situation also sometimes causing undesirable contamination of the end product. We now know that the enzymes can be attached to solids or trapped in a cage of solid material or fiber.

This development actually had its origin in bionics, which means "the study of systems that either mimic or carry out functions similar to those found in biological and natural systems." Since many enzymes are attached to surfaces in the cell interior rather than floating around loose inside, biochemists wanted to study these enzymes in an attached state and therefore invented enzyme immobilization in the early 1960s. Study of the preparation of enzyme catalysts and the influence that binding to a solid surface has on them is one area of my research.

One recent discovery in my laboratory pertains to the glucose oxidation reaction mentioned earlier. The hydrogen peroxide produced quickly deactivates the enzyme involved, limiting the useful lifetime of the enzyme. By chemical coupling of the enzyme to activated carbon, which itself is an effective catalyst for hydrogen peroxide decomposition, a multifunctional solid surface-enzyme catalyst is obtained in which the hydrogen peroxide enzyme poison is converted to harmless compounds as it is produced. This strategy should prove useful in enzymatic processes for manufacturing fructose and gluconic acid, important in the food and pharmaceutical industries.

As an example of an immobilized enzyme reactor, consider a tube filled with solid particles in which enzymes are trapped. These catalyst particles are held in place by screens on either end of the tube. With the enzymes im-



Immobilized enzymes — trapped inside a tube so a continuous process can use them over and over — can selectively remove side groups from the core of natural penicillin. Substituted artificial side groups create a semisynthetic penicillin with more desirable properties.

mobilized in this way, we can use the enzymes and carry out the desired reaction continuously as the reaction mixture flows through the tube. One application of this technology is found in the manufacture of semisynthetic penicillin. The natural penicillin molecule can be viewed as having two parts: a core and a side group. An enzyme is known that will selectively remove the side group without disturbing the core. If we pass natural penicillin through a reactor containing this immobilized enzyme, we can obtain the core, which, after separation from the cleaved natural side group, can be reacted with a new and artificial side chain to produce a semisynthetic penicillin.

These semisynthetic penicillins have some interesting properties relative to natural penicillin. Some are absorbed more easily through the intestine than many natural penicillins, which means they can be taken orally rather than by injection. Several semisynthetic penicillins tend to cause fewer allergic reactions than do their natural counterparts, and, by varying the synthetic side group, the drug becomes lethal to certain bacteria which are resistant to natural penicillins.

A much more complicated subject is the catalysis of desirable reactions in the living cell. The living cell is truly an amazing chemical reactor, and we have only begun to learn how to use it to make useful and valuable products. In this discussion, cells may be viewed as self-contained chemical factories with many assembly lines which loop and branch in many directions. Each machine in one of these assembly lines is a particular enzyme, so the cell synthesizes a different enzyme for each of many different chemical reactions.

To use the chemical factory for our purposes, its major control systems must be considered. First of all, the cell's DNA contains all of the blueprints to make the enzyme machines and other important cell components. Based on the environment in which the cell finds itself, appropriate segments of the DNA instructions are turned on or off. In a solution that is rich in the amino acid histidine, for example, a bacterial cell will not waste energetic and chemical resources to make the enzymes needed for histidine synthesis. If that assembly line isn't needed, it isn't produced.

Besides acting on the DNA to determine which enzymes are made, the environment also influences the enzymes to determine the rates at which the various assembly lines work. A major objective in biotechnology is to learn how to manipulate the factory for particular purposes. There are two basic kinds of such manipulation: One is regulation of the environment, and the other is changing the blueprint, changing the DNA.

A recent strategy for maximizing production of food yeast illustrates an engineering contribution. Yeast is sensitive to the amount of sugar in its environment. At high sugar concentrations the yeast makes alcohol and carbon dioxide. However, if producing yeast cells is the objective, sugar shouldn't be wasted to form alcohol. This undesired result can be avoided by maintaining a sufficiently low sugar content in the yeast growth medium. Since no one has yet found a way to measure the sugar level or to keep track of how rapidly the yeast are growing inside large fermentation tanks, an indirect method for estimating these important process parameters has been devised. The fermenter exhaust gas carbon dioxide content is measured, and based upon that data, a computer controls the rate of sugar addition to the growing yeast culture. A high CO_2 level corresponds to too much sugar in the growth medium, so addition of sugar is stopped. When the CO_2 level falls, more sugar can be pumped in. My colleague Greg Stephanopoulos is extending this strategy, using the latest sensor technology and modern control and estimation theory, to improve process controllers for microbial reactors.

In considering the motivation for changing the DNA of an industrial microorganism, it is important to recognize that the control system of the cell has been designed over time by evolution to fulfill its own natural objective — the survival and growth of the organism. But often maximizing cell growth is not man's main purpose. In the case of making penicillin, our objective is not mold production but penicillin synthesis. To improve a microorganism's productivity, it's desirable to find ways to manipulate the cellular chemical factories to make them produce more of the desired compound. This has been done quite successfully in the case of penicillin by classical methods of industrial genetics. The original penicillin culture in 1941 produced about 4 units per tank. After a worldwide search to find the best penicillin producer in nature and then after some mutation programs, yields increased to around 1,000 units. Since then, by mutation and selection processes, the productivity has been improved to more than 10,000 units per tank.

These improvements were done by a very slow, arduous, and imprecise technique. The mold was exposed to radiation, such as ultraviolet light, or to chemicals that caused changes in the DNA blueprints of the mold. Which part of the DNA would be changed in this fashion and what effect the change would have on penicillin productivity was unknown. Most of the mutated organisms produced by such treatment died. However, a small fraction of cells produced by this shotgun program was able to grow and to synthesize more penicillin. It's by this strategy that many genetic improvements in commercial microorganisms have been obtained — a sequence of carefully selected good luck.

Recently there has been a dramatic improvement in man's ability to manipulate DNA. Often called genetic engineering, recombinant DNA, or gene-splicing technology, these methods can instruct a bacterial cell to synthesize new products that aren't ordinarily found in microorganisms. For example, we can now put into microorganisms DNA molecules that will cause them to produce human proteins. This has tremendous potential. It means that drugs such as insulin and interferon can be produced in bacteria very rapidly, cheaply, and in relatively large quantities.

Interferons are proteins that have potent effects against many viruses. Until quite recently, the world's major source of interferons was the Finnish Red Cross, which extracted the protein from white blood cells. The total amount of interferons produced was minute — about 2/1000 of an ounce per year. Last year it was announced that the interferon blueprint had been moved into bacteria using recombinant DNA technology. Using the published yields that have already been achieved, one 10,000-liter vessel can produce six times the current world's production of interferon (assuming the same productivity on that scale as can now be obtained in the lab). Making that batch would take several days at most. It is therefore anticipated that the current interferon price of about $50 per million units will be reduced to pennies or even fractions of a cent thanks to gene-splicing technology.

Genetic engineering has many more potential applications. One which has been recently explored is a process designed to make animal feed. In this case the goal is to produce microbial cells, but to do this the original cell has to use some of its internal energy to assimilate nitrogen from the environment. Genetic engineering adds a missing enzyme and makes it possible to incorporate nitrogen without using any of the cell's energy. Presumably this energy can be redirected, used in other places, and the end result should be more cells for the amount of raw material used. At this point, the modified organism produces only about 4 percent more cell material as a result of the genetic modification, but the cell's chemical factory is very complicated and we have only begun to try to improve it with the methods of recombinant DNA. If we consider the cell as a factory with a collection of assembly lines, we have now managed to upgrade the loading dock. Improving the factory itself is something for the future.

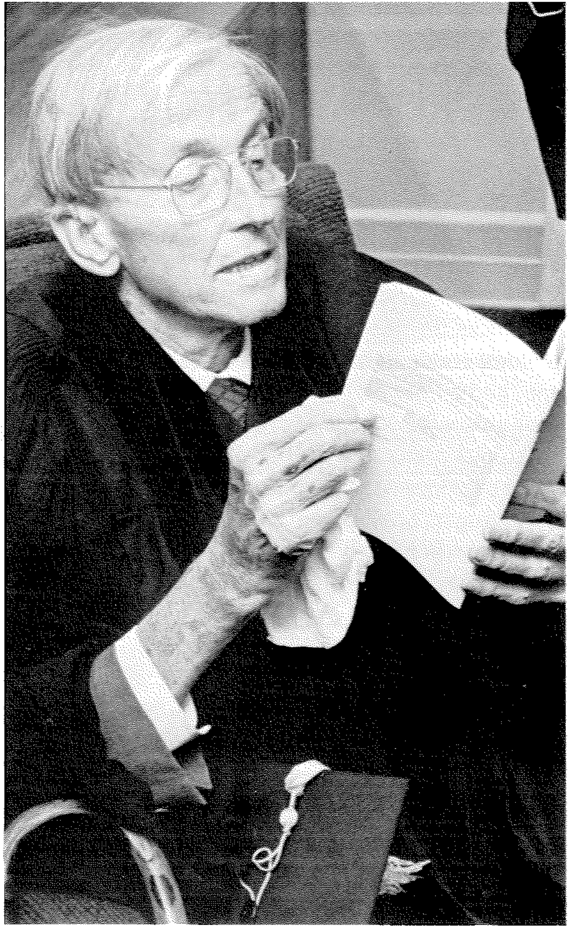
Gasohol, which is of great public interest now, is a liquid motor fuel that is a mixture of 90 percent gasoline and 10 percent ethanol. In order to qualify for some current tax incentives, the ethanol must be made by a biological process using natural materials. Gasohol production has been granted special tax treatment because, according to its proponents, the use of domestic agricultural and waste materials to make fuel reduces the need for imported oil. Gasohol gives mileage comparable to that of gasoline, a slight increase in octane, and a little bit higher emissions.

The alcohol portion of gasohol is currently produced by treating corn physically to make small particles, which are subjected to decomposing enzymes to break down the starch to form sugars. The sugars are then fed to yeast — essentially the same process as in brewing and home winemaking — producing a solution which contains about 10 percent ethanol in water. The water then must be separated from the ethanol. The solid residue from this process — the yeast and the debris from corn — is good animal feed.

However, using corn to make alcohol is controversial, partly because of the net energy benefit. There are widely divergent opinions on this; according to a recent report by the Alcohol Fuels Policy Review, fermentation ethanol production yields only about 5 percent more energy than must be used in the ethanol manufacturing process using corn as a raw material. This is a major concern of gasohol opponents. Net useful energy yields can be enhanced by using low-grade fuels for the distillation separation of alcohol from water (the most energy-demanding part of the process), or by using other separation methods. Vigorous research on these problems is now in progress.

Another debate surrounding gasohol is the propriety of using corn that could be used for food to make a motor fuel. Without using any of the corn that currently goes into food, sufficient grain is available in the United States to produce 5 billion gallons a year of alcohol. That may sound like a tremendous amount, but it is a small fraction of the estimated 1990 gasoline consumption of over 100 billion gallons per year. Consequently, it is important to identify new raw materials for this process — perhaps cellulose. There are vast amounts of cellulose in agricultural and urban wastes and in plant matter. If we could find an economical way to break this cellulose down into sugar, it would yield significantly more alcohol and other useful products.

My research group is working to support and expand microbial reactor technology through several research projects which are investigating protein and DNA synthesis in individual microbial cells, immobilized cell physiology, and scale-up problems and opportunities for reactors using recombinant microorganisms. The major aim of this research is to improve the fundamental bases for microbial reactor design and operation by synthesizing the key biological and engineering factors into a quantitative process description. The research problems are fascinating, and the scope of application of their solutions is rapidly expanding. It is an exciting time for all involved with biotechnology. □



Max Delbrück

1906-1981

Max Delbrück, Nobel Laureate and Board of Trustees Professor of Biology Emeritus, died on March 9 at the age of 74. A service in celebration of his memory was held at Caltech on April 19, with a program arranged as requested by the family and presided over by Seymour Benzer, who is Boswell Professor of Neuroscience at Caltech. Speakers included Jonathan Delbrück, the eldest of the four Delbrück children; Nobel Laureate James Watson of Cold Spring Harbor, New York; David Presti, Weizmann Research Fellow in Biology at Caltech who was Delbrück's last graduate student; Gunther Stent, professor of molecular biology at UC, Berkeley; and David Smith, associate professor of literature at Caltech.

The service concluded with the Bach Cantata No. 106, requested by Max 20 years ago in anticipation of such an occasion. The cantata was performed by a group of friends under the direction of Helene Hancock, with the Delbrück's daughter Ludina as cellist.

Excerpts from the tributes given at the service follow.

JONATHAN DELBRÜCK: I think if Max were here today he'd be inclined to ask quite seriously, "Why is everybody being so serious today? Why do you people think of this as such a serious occasion?"

A year ago today we were on the annual Joshua Tree camping trip, an event we've done every year for the last eight years, so there's a hard core of Joshua Tree people who have been on every trip, but there are always many new faces too. It was officially designated as a trip to introduce undergrads at Caltech to being in the desert and to Max's unusual way of doing science in the wilderness. A distinguishing feature of this trip every year was what Max called the "Death March." He was never able to go on the whole trip himself, but he dubbed it that when he saw the exhausted people staggering in. He probably would have made a good joke about that on this occasion.

So let's remember Max for his sense of humor and his acceptance of and his interest in people from all walks of life — farmers in the fields and people we ran into on camping trips, scientists and politicians. He took an interest in everybody. Let us remember his great spiritual strength, which bore up those of us who had more problems than he with his passing through this last great adventure. That was how he saw the experience of dying. Let's celebrate our good fortune in having been able to partake of Max's benign and generous influence on all our lives.

SEYMOUR BENZER: After abandoning physics, Max moved into biology, and his career there covered two major phases: The first one was bacteriophage, and the second was Phycomyces. In each of these he fostered the development of innumerable young people. Some gave him more trouble than others. He once said to me of Jim Watson, "Jim used to love me like a father, and now he hates me like a father." Jim will speak of Max as a scientific father in the phage era.

JAMES WATSON: My initial glimpse of Max came soon after I entered Salvador Luria's lab in 1947 to work on phages, never doubting that there in that lab would be another way to truth. Instantly the hero worship I had felt from my reading of Max's writings became adoration, and I wanted to be as much like him as I could, possibly including marrying a girl as wonderful as Manny. For Max was no ordinary very bright mortal, but a graceful god sent into the world of biology to rescue it from its complexity by placing into its hands those marvelous replicating phages, which Max made us call T1 or T2 or T4, but never T100 because that would have been too much complexity.

My approach to science as well as to people became indelibly fixed the following summer when we all came together at Cold Spring Harbor — the Delbrücks, the Lurias, Gunther Stent, Seymour Benzer, and I — in an atmosphere that I can never remember as less than perfect. Now I realize that all the personality of Cold Spring Harbor, which I so loved then and still do, was given to it by Max. He abhorred the petty, and in searching for the deepest of theories he insisted that we work together in a collective, generous fashion. The selfish and the avaricious were not tolerated, and those unfortunate souls who could only survive with those traits were not for Max or for those of us who without being ever formally ordained knew we were the apostles of phage.

Max also had no use for stuffiness or protocol. He was never Professor Delbrück or Dr. Delbrück, but always Max to all who would learn with him. There was no hierarchy in which to fit, and the informality in which ideas were accepted or rejected gave us all the chance to do our best — and to dream that we might find out later the ultimate of answers. Never did Max divert toward his own glorification the talents of his disciples, but he always made sure that when we claimed a decisive result that he was also convinced so that we would not be led astray by the haste of our youth.

I still cannot accept that Max is not here and worry that my words will not please him. I want badly to say what I never had the courage before to reveal save now for my wife and children — that Max meant more to me than anyone else. I hope I did not too often needlessly disappoint him.

BENZER: One example of Max's originality is that he was one of the first molecular biology dropouts, around 1952. So he felt at liberty to turn to another problem that greatly interested him — that of sensory transduction. He chose Phycomyces as an organism and produced a whole new brood of Phycomyces biologists. One of these is David Presti, who will speak of Max as a scientific father in the Phycomyces era.

DAVID PRESTI: I knew Max over a period of several years as a teacher and as a colleague, but most importantly I knew him as a close friend. Max's scientific achievements were certainly significant, but his impact on the development of molecular biology came at least as much through his influence on individuals by way of personal interaction as through experiments done by his hand.

One of Max's great virtues was that he was always willing to give enthusiasm a chance, and thus he launched a number of successful careers in biology. Those who often saw Max as humorless and scolding might ponder Voltaire's statement that "God is a comedian playing before an audience that is afraid to laugh." For a deep sense of humor really did pervade his relationships. He also often expressed an attitude that was extraordinarily skeptical and scathingly critical, but at the same time very tolerant. Although he would all too often be heard to say, "I don't believe a word of it!" he would lend support while you proved him wrong. In fact, in a way, I think he actually delighted in being proved wrong, just so long as the proof was solid.

Max brought to his work great intellectual curiosity and incisive analytical thought, and also his own healthy childlike enthusiasm, which he made no attempt to suppress. This excitement was manifest in many circumstances. For example, several months ago Max suffered a small stroke in his visual cortex that produced a blind region in part of his visual field. When I visited him in the hospital shortly after this mishap, at a time when most people would have been exceedingly depressed, he was excited and very interested in the possibility of doing experiments on himself that might shed some light on human visual cortical functions. Such was his enthusiasm.

Max always insisted on openness in scientific research and had little regard for empire-building at the cost of openness. A spirit of integrity and cooperation pervaded his laboratory at Caltech, from the era of phage research through the days of the Phycomyces sensory transduction group in which I worked. Max's style of doing science may be approaching extinction as scientific research becomes more and more a big business — more a domain of the ambitious and less a "haven for freaks," as Max liked to say. He truly gave science a human touch, and I feel greatly privileged to be among the many people upon whose lives he had a major influence.

BENZER: Gunther Stent was a postdoc in Max's lab at Caltech in 1948-50. Gunther, who shared Max's German background and deeply ingrained interest in philosophy, was intrigued by the notion that Max got from Niels Bohr that there might be some kind of uncertainty principle at work in biology — similar to the one in physics — such that complete predictability of the future or an organism would be incompatible with the living state. Gunther will speak of Max as a philosopher.

GUNTHER STENT: As everyone who had even the slightest acquaintance with Max realizes, it was his extraordinary personality that made him the spiritual force which affected the scientific and personal lives of so many people. It may be less well known, however, that Max's personality and scientific attitudes reflected, and were probably shaped by, a particular brand of philosophy — the so-called Copenhagen Spirit.

It was thanks to the Copenhagen Spirit that Max could take the remarkable sovereign attitude that he had in controversial matters. This is not to say that Max was invariably right. On the contrary, as is well known to his friends, in scientific decisions that, for lack of critical data, had to be based on intuition rather than logical inference, Max was very often wrong. But he was never unreasonable and always appreciated the full depths of the problem addressed, often better even than the person who eventually found the correct solution. And so I want to say something about this Copenhagen Spirit, without an understanding of which I think Max cannot be understood.

It was Niels Bohr who found in the Taoist symbol of Yin and Yang an appropriate symbol for the Copenhagen Spirit. He points out explicitly in his great "Light and Life" lecture that the Copenhagen Spirit addresses the same kind of epistemological problems which thinkers like Buddha and Lao-tzu, who inspired Taoism, had confronted when they tried to harmonize our position as spectators and actors in the great drama of existence. The most distinctive feature of these problems is that they pose deep paradoxes, which arise because there is something inherently paradoxical about our intuition about the world.

Most contemporary philosophers of science know all about the Copenhagen Spirit, of course, and they are fully aware of the role it has played in the development of present-day physics. I think it is fair to say that, with Max, Bohr found his most influential philosophical disciple outside the domain of physics, in that, through Max, Bohr provided one of the intellectual fountainheads for the development of 20th-century biology.

Fortunately Max left us his own explication of the Copenhagen Spirit in the form of an unpublished book-length manuscript entitled *Mind from Matter?* a transcript of a series of 20 lectures on the origins of human cognitive abilities, particularly as they pertain to the sciences. He gave these lectures in 1972 in a course he called "Evolutionary Epistemology."

Toward the end of the book Max — having pointed out that in the far reaches of our search for understanding of the world deep paradoxes are encountered — raises a yet larger paradox. How is it possible, he asks, if it is indeed true that the categories of space, time, number, truth, and so on were put into our brain by evolution, that we are able to transcend them now and finally reach a higher level of understanding that was never selected for. That is to say, he asks, how is it possible that in natural selection so much more was delivered than was ordered?

In line with Bohr's closing statement of "Light and Life" that any meaningful sense of the term "explanation" precludes any attempt to explain our own conscious activity, Max does not really answer that final question. Nevertheless, he feels that, thanks to the Copenhagen Spirit, mind has come to look less psychic and matter less materialistic. He closes his last lecture by asking his students to bring him their answers by next Thursday at 5 p.m. Or, he says, better yet, why don't you rephrase my questions, bring them into sharper focus, and then spend the rest of your lives trying to answer them?

As for us, we can't bring Max our answers, but some of us at least will spend the rest of our lives trying to find them.

BENZER: Max's method for learning was to teach, and every year for about 40 years he would assign himself the task of teaching a course in some new subject that *he* wanted to learn. This ranged all the way from statistical mechanics to epistemology. So Max became an expert in every one of those subjects. As recently as a year and a half ago, long after he had been officially retired, he volunteered to teach freshman physics here at Caltech as a sort of refresher course for himself.

David Smith, our next speaker, is in the Division of the Humanities and Social Sciences here at Caltech. He is founder of the Baxter Art Gallery, and he and his wife, Annette, have been close friends of the Delbrücks over many years, sharing an interest in literature, poetry, and art. David will speak of his and Annette's reminiscences of Max as a humanist.

DAVID SMITH: A few weeks ago Annette and I received a gift from Max and Manny of Walter Kaufmann's *Twenty-Five German Poets*. Max had been enjoying it and had ordered copies for some of his friends. Kaufmann uses a wonderful phrase to describe certain poets, whom he calls "human archetypes." With that phrase in hand, I realize that I always thought of Max as a human archetype. Taken whole and entire, he was one; but we all knew him serially too, in his parts and moments; and it is sometimes hard to sustain that archetypal sense. It keeps returning as the sum, but so do the parts — his love of paradox, his impatience as well as his playfulness, the intensity of his play, and his lack of self-seriousness.

One time when a bunch of us went camping, Max and his younger son, Toby, failed to arrive until late at night, long after the rest of us. They had, it seems, spent four hours looking for a Yo-Yo string. His playfulness translated quite literally into plays, the marionette shows he put on with his children, in which in a marvelous conceit, he often took the role of Uncle Max, the fusty professor with a thick German accent. Max was Max, and sometimes he played Max. He also proposed to play Samuel Beckett, threatening to give the latter's Nobel acceptance speech for him when he failed to go to Stockholm.

He particularly admired the work of Beckett because, almost as a scientist, Beckett had reduced the complexities of human intercourse to their elements, a series of games turning in an eternal round. A few years ago, when relevancy was the word, a graduate student induced a number of notables here to discuss the relevancy of their science. Max discomfited them all by using Beckett's Molloy and his obsession about putting his sucking stones in order as a metaphor for why people do science. Scientists "are hooked," he would later say, "like an addict or a nut who likes to solve puzzles."

His interest in the humanities was profound, of long duration, and of increasing intensity. And it was a matter of day to day, practical observance, as most things profound are. He often attended humanities seminars. He even sponsored one. He and Manny regularly attended youth recitals and concerts. He was the most active supporter of the art gallery on the faculty. He was interested in the whole education of students. For years Max and Manny organized wonderful camping weekends — huge tribal affairs with undergrads, grads, postdocs, faculty from here and sometimes afar, and, of course, families and dogs.

He took an ironic pleasure in pointing out that he was a relic of another day. Translated and interpreted, what that really meant was that he was that rare bird, a man of general culture, an intellectual. He did not like the schism which produced the "two cultures," and, in fact, his commencement address in 1978, "The Arrow of Time," warned against it. Time was the controlling metaphor of that talk, and it was for him a central issue, both intellectually and personally. Time, he pointed out, differs in the physical and biological sciences, the one reversible, the other directional. But this arrow of time is also the specialty of poetry, time "whose bending sickle's compass comes" to menace constancy in the endless Platonic struggle between the biological clock and human hope. He corresponded with poets about it.

If in these past months Max had begun to concentrate on poetry (which he did with intensity and luminosity), the immediate reason was that he had been invited to give a lecture at the Poetry Center in New York. It was about Rilke, the most intuitive of the German poets, that he intended to talk. He had completed some nine pages of his talk, titled "Rilke's Eighth Duino Elegy and the Unique Position of Man," when he had to suspend work on it. He was intrigued by how Rilke, without scientific knowledge, with no more than the chaotic intuitions he had during his stay in the Duino Castle, could finally arrive at a view of man entirely in keeping with one science might approve of today.

In keeping with Max's sense of humor, I should like to return to a more Delbrückian mode. A few years ago, in another commencement talk at another university, he addressed himself to the moral dilemma of the scientist who must follow mother nature with unblinking eye. "There is great happiness in doing research," he said, "but nature is full of hard truths. How do we live with what we discover, with the broom and bucket we can't stop?" He put his answer in a parable of his own, a true Maxim, set as it was against the expectations of loftiness which attend such events. "Let me come, at long last," he concluded, "to the plot of *The Pig and I*. It is simplest if I show you the play. Here is Wilbur, the pig, very much beloved by my daughter Ludina, and very much scorned by her older brother Toby. Here is the professor and he has advertised for a companion to live with him. The lion comes and brags about his courage and ferocity. The mouse comes and shows its cheerful disposition and playfulness. The dog comes and offers himself as a friend and servant. The cat comes and shows its elegance and languor, and finally the pig comes and says, 'I am down to earth, I have my nose to the ground. I don't look up to anybody, I don't look down on anybody.' So the professor decides: 'You are my choice; you are what I want. Dogs look up to us; cats look down on us; pigs is equal.' So this is my advice to you. Hold on to the pig if you want to keep your sanity in the difficult years ahead."

We shall try, Max. □

Putnam Problems–Play by Play

For the past ten years on a Saturday in early December a Caltech team has fought for the school's honor (and individual glory) in national competition and has emerged victorious five times. It's not the football team — or the water polo team — but, what else, the mathematics team. It actually did not extend its winning streak this year but still came in a very respectable sixth in the William Lowell Putnam Mathematical Competition out of more than 200 teams from the United States and Canada. (Well, you can't win 'em all; USC didn't get to the Rose Bowl this year, either.) In the 41 years of the Putnam Competition only Harvard has won more times (9) than Caltech (8); besides the latest winning streak, the Beavers won in 1950, 1962, and 1964. In the past ten years only one other school has won more than once — Washington University (in St. Louis) in 1977 and 1980.

William Lowell Putnam, Harvard class of 1882, believed strongly in the merits of intellectual intercollegiate competition. In 1927 his widow created a trust fund to establish such a competition, which is now under the administration of the Mathematical Association of America. The first match was held in 1938. Among the early winners was Richard P. Feynman, then an undergraduate at MIT, now the Richard Chace Tolman Professor of Theoretical Physics and Nobel laureate.

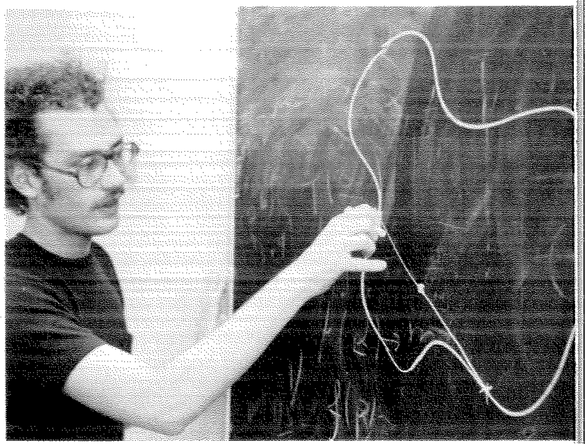
Nowadays more than 2,000 undergraduates enter the Putnam competition every year. On the first Saturday in December they spend three hours in the morning and three in the afternoon taking a written examination at their home schools. Everyone gets the same six problems in the morning and six more in the afternoon, made up each year by a committee of three mathematicians (often former Putnam winners). The problems are designed to test "originality as well as technical competence," which means simply that they tend to be very hard. The top grade last year was 61 percent, and only 43 students scored over 33 percent.

Contestants compete for cash prizes as well as personal glory (mathematicians don't have to worry about their amateur status). Prizes are awarded to the top five winners, whose ranking is not revealed, and to sixth through tenth place. To make things more interesting, some of the students are chosen in advance as team members representing their schools. Each team has three members, whose individual rankings on the test are added together to get the team score (the lower the better, just as in cross-country). The top five teams also win prizes for their respective mathematics departments.

Coach of the Caltech team is Gary Lorden, professor of mathematics, who doesn't really do much coaching. The Putnam team doesn't have to endure long hours of workouts — just one "how to solve it" session Lorden holds a few days before the exam. Although not requiring an extremely advanced or sophisticated knowledge of mathematics (freshmen enter as well as seniors), the examination presupposes familiarity with "subtleties beyond the routine solution devices" of differential equations and expects that "elementary concepts from group theory, set theory, graph theory, lattice theory, number theory, and cardinal arithmetic will not be entirely foreign to the contestant's experience." But ingenuity is the key to success.

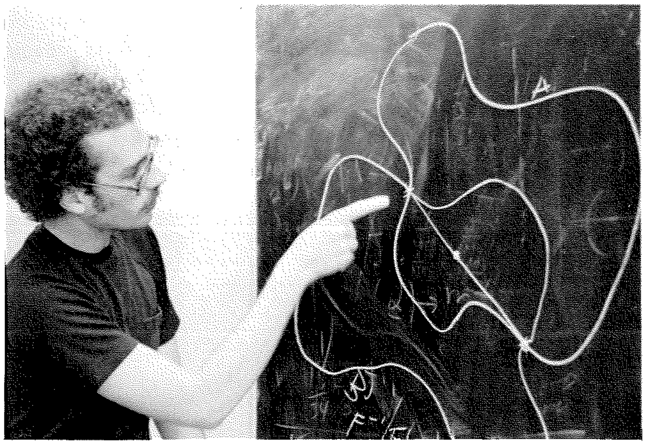
The problems need to be fair as well as interesting, according to Lorden — fair in the sense that you shouldn't be able to arrive at a solution by luck, and interesting in that the solution should be intriguing to other mathematicians. Many of the problems also involve a kind of mathematics folklore — ideas that people who delight in solving this kind of problem usually seem to be familiar with. But it's not just a matter of a bagful of tricks either, says Lorden. "There aren't that many tricks to learn; mostly it's just being sort of clever and that is something that's hard to teach."

One of the cleverest of Caltech's recent entrants has been senior Peter Shor, a veteran of four years of the team. He came in sixth as a freshman, was ranked in the top five the following year, and then, with old age taking its inevitable toll a bit early, fell to 8th and 13th place respectively as a junior and senior. Shor had particularly good qualifications for the Putnam team; in high school he was a member of the U.S. team that won the 1977 Mathematics Olympiad against high school teams from 21 other countries. Any coach would welcome an Olympic winner! After graduation Shor plans to study math in graduate school at MIT.

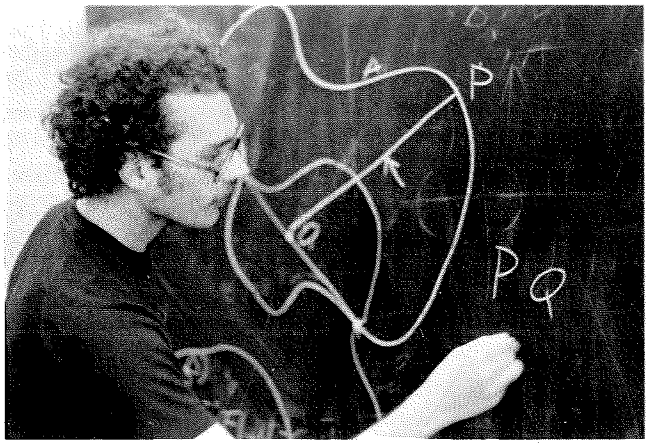

You want to find two points on any closed curve, says Peter Shor, so that a given point inside that curve is exactly in the middle of the line between those two points.

Although the answers to the Putnam problems are published annually in the American Mathematical Monthly, those answers are terse, to say the least, and not much help to the fairly casual observer, unless he happens to already know how to do it anyway. But Shor has picked out for *E&S* some of his favorite Putnam problems and explained how he arrived at the solutions. His explanations may not exactly make it all look easy, but at least they bring the Putnam Competition into range — like instant replay in slow motion — where the skills can be observed and appreciated by the great majority of us whose ingenuity falls somewhat short. Following is Shor's play-by-play description.

The first is problem B-4 from 1977: Let C be a continuous closed curve in the plane which does not cross itself and let Q be a point inside C. Show that there exist points P_1 and P_2 on C such that Q is the midpoint of the line segment P_1P_2.



If the curve rotated around the point crosses the original curve at one point, then it's also going to cross it at another point.



Some point P (designated R_1 in the text) on the original curve is going to be the farthest away from Q, so that PQ is a maximum distance.

You want to find two points on any closed curve so that a given point inside that curve is exactly in the middle of the line between those two points. Suppose you just rotate the entire curve 180 degrees in the same plane around the point Q. (Moving things around in a plane is a nice way of solving some problems. The Mathematics Olympiad had a lot of this sort of thing because we weren't supposed to know calculus yet.) Now, once you've done that, let's suppose that the rotated curve crosses, or intersects, the original curve. And if it crosses the original curve at one point, P_1, then it's also going to cross it at another point, P_2, on the other side of point Q (because you rotated it 180 degrees around Q). Since P_1 on the original curve corresponds to the position of P_2 on the rotated curve and P_2 on the original curve corresponds to P_1 on the rotated curve, then P_1Q and P_2Q have to be the same length. So Q is the midpoint of the line segment P_1P_2.

But that's not quite all. This is true *if* the curve crosses itself. You have to show that that *does* indeed happen; you have to show that when you rotate it 180 degrees around itself, there is some point where the rotated curve intersects the original one. That's not hard to do because you know that some point — let's call it R_1 — on this original curve is going to be the farthest point away from Q, so that R_1Q is the maximum distance; there's another point, R_2, such that R_2Q is the minimum distance from Q to the curve. If you want to be technical about it, this comes from something called compactness, which we don't want to worry about now. So this point R_2 — the closest one — must be inside the rotated curve, because the rotated curve can't have passed between this point and Q. If it did, there would have to be a point closer to Q, and there isn't because we defined R_2 as the closest. By the same reasoning, R_1 must be outside the rotated curve. And a curve connecting something inside and something outside has to cross somewhere. So we have shown that the original curve and the rotated curve do intersect. And we have already shown that if they do intersect, there must be two points that are equidistant from Q.

This next one is from the latest Putnam contest, problem A-4 from 1980:
(a) Prove that there exist integers a, b, c, not all zero and each of absolute value less than one million, such that

$$|a + b\sqrt{2} + c\sqrt{3}| < 10^{-11}$$

(b) Let a, b, c be integers, not all zero and each of absolute value less than one million. Prove that

$$|a + b\sqrt{2} + c\sqrt{3}| > 10^{-21}$$

When you first look at it, it looks rather unusual — fairly bizarre really. You start looking at all these numbers — a, b, and c can be positive or negative, but each has to be less than 1 million, or 10^6, and $a + b\sqrt{2} + c\sqrt{3}$ has to be less than 10^{-11}, or .00000000001. Since this is very close to zero, one way to approach the problem is to find two numbers of this form that are very close together, so that when you subtract them, their difference is close to zero.

So let's say we have besides a, b, and c, a_1 and b_1 and c_1; if we subtract $a_1 + b_1\sqrt{2} + c_1\sqrt{3}$ from $a + b\sqrt{2} + c\sqrt{3}$, we end up with $(a-a_1) + (b-b_1)\sqrt{2} + (c-c_1)\sqrt{3}$, which is to be small. Just how small? If we can show that it's smaller than 10^{-11} we will have solved the first part of the problem. If a, b, and c, and a_1, b_1, and c_1 are positive integers less than 10^6, then $a-a_1$ and $b-b_1$ and so on are less than a million, because if you subtract two ordinary positive integers less than 1 million, your answer can't be larger than a million; it could be negative but can't be bigger than 10^6 or less than -10^6. Since $1 + \sqrt{2} + \sqrt{3}$ is less than 10, then the number $a + b\sqrt{2} + c\sqrt{3}$ must be less than 10^7. And since these numbers are positive they lie between 0 and 10^7.

We have a million choices for a, a million choices for b, and a million choices for c. If we plug them all in for $a + b\sqrt{2} + c\sqrt{3}$, we get 10^6 times 10^6 times 10^6, or 10^{18} numbers of that form squeezed between 0 and 10^7. If we space them all equally, then they would all be $10^7/10^{18}$, or 10^{-11}, distance apart. And if we don't space them all equally, they're going to have to be closer than 10^{-11}. There have to be at least two points in there that are closer together than 10^{-11} so that the difference when you subtract them is less than 10^{-11}. Strictly speaking, even if they *were* all equally spaced, they would be closer than 10^{-11} because $1 + \sqrt{2} + \sqrt{3}$ is *less* than 10, which makes the interval actually less than 10^7 and an average distance of less than 10^{-11}.

And what if they're the same number? In this part of the problem that doesn't matter because subtracting them would give you zero, which is certainly less than 10^{-11}. However, that brings us to the second part of the problem, which asks us to show that $|a + b\sqrt{2} + c\sqrt{3}|$ is greater than 10^{-21}.

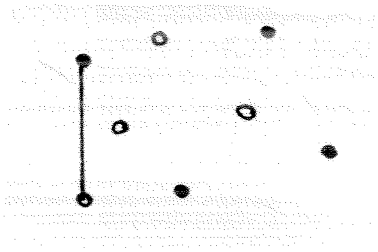
(The first part of this problem was supposed to be relatively easy. We're going to leave the second part for interested readers to tackle; it's more difficult and involves an entirely different idea and technique. It's not that Shor didn't figure it out; he did, and we will publish the solution in the next issue.)

And finally, problem A-4 from 1979: Let A be a set of 2n points in the plane, no three of which are collinear. Suppose that n of them are colored red and the remaining n blue. Prove or disprove: there are n closed straight line segments, no two with a point in common, such that the endpoints of each segment are points of A having different colors.

This is a connect-the-dots problem essentially. You've got n blue dots and n red dots and you have to connect blue ones to red ones with straight lines so that no lines cross. You have to show that this can be done no matter where the points are.

A good way to start is to put down a bunch of points and try to get a good idea. You look at them and think: Which points can you connect without interfering with any of the other possible connections? Which ones can you connect so that they won't come back to haunt you?

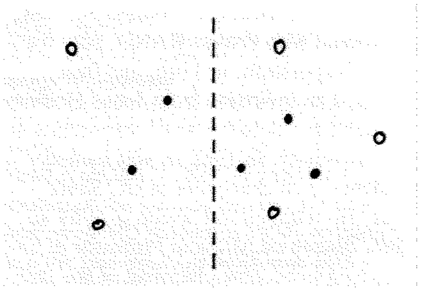
The obvious ones to connect are the ones on the outside. How do we define what "outside" means? If you connect all the points — every point with every other point regardless of color, there will be some connections located so that all the rest of the points will lie on one side of that line. (It won't work if there are three points on the same line, but the conditions state that no two lines have a point in common.) Now, if there are a red point and a blue point on the outside, then there has to be a red one next to a blue one someplace on the outside. So you connect these two, and that line will be on the outside and will not interfere with any subsequent lines that you draw. If you can connect two points on the outside, then you only have to work with the rest, which is a smaller number of each color, so you can just do the same thing again — reduced to a smaller problem — and so on. If you can do it with four of each color, then by induction you can do it with three, with two, and with one of each color.



However, to make this work we assumed there were a red point and a blue point somewhere on the outside to start with. The solution doesn't work if all the points on the outside are the same color — let's say blue. Since we can't connect two of the outside dots in this case, we have to think of something else. I remember staring at this for awhile before getting an idea — some way of measuring the points from left to right. We can put in a coordinate axis (and rotate things around if we have to so that this axis doesn't make any point directly above another, that is, the same left-to-rightness) and then label the points 1, 2, 3, and so on, starting from the leftmost point and numbering them to the rightmost point. No matter how we draw the axis, each of these extreme points has to be the same color, because all of the outside points are the same color in this case.

If you can draw a vertical line separating two consecutively numbered points somewhere in between, so that there is an equal number of red points and blue points on the left side (they will be the lower-numbered ones), then you can solve the problem for each side by the proof just mentioned. If the colors are equal on the left side, they will also be equal on the right side of such a line; the two sides don't have to be equal to each other.

We can take our coordinate axis and start moving it across the field of dots from left to right. The first dot we hit will have to be blue because we have stated that all the outside dots are blue. If we "keep score" of the blue points minus the red points as we go across, then we would start out with 1 blue point and it would go up or down 1 with each point passed. For example, if the next point were blue, the "score" would go to 2. By the time we get to just before the last dot, we would have to be at −1, because the last dot will also have to be blue and we have to end up with zero. In going from 1 to −1, somewhere in between we would have had to cross zero, giving equal numbers of each color on either side.



It's not unusual in mathematics to have a hodgepodge of arguments like this. It may not be elegant, but it's legitimate, and sometimes the only way, to say that either this argument will work or, if it doesn't, then this other argument will work, and then prove that there aren't any cases where none of the arguments will work.

Now for all the Sunday-morning quarterbacks who have been saying, "Why, that's not so hard," throughout this article, here are some more Putnam problems to try out yourselves. Shor's answers will be published in the next issue of *E&S*.

From 1980:

Problem B-3

For which real numbers a does the sequence defined by the initial condition $u_0 = a$ and the recursion $u_{n+1} = 2u_n - n^2$ have $u_n > 0$ for all $n \geqslant 0$?

(Express the answer in the simplest form.)

Problem B-4

Let $A_1, A_2, \ldots, A_{1066}$ be subsets of a finite set X such that $|A_i| > \frac{1}{2}|X|$ for $1 \leqslant i \leqslant 1066$. Prove there exist ten elements $x_1, \ldots, x_{10}$ of X such that every A_i contains at least one of $x_1, \ldots, x_{10}$.

(Here $|S|$ means the number of elements in the set S.)

From 1978:

Problem A-6

Let n distinct points in the plane be given. Prove that fewer than $2n^{\frac{3}{2}}$ pairs of them are unit distance apart. (This may look simple, but it was one of the hardest problems that year.) □

Retirements

1980-1981

Francis Clauser

James Bonner

James F. Bonner
Professor of Biology

James Bonner came to Caltech as a junior in chemistry in 1929 — just one year after the founding of the Biology Division. In that one year he discovered biology. After a brief return to finish his AB at the University of Utah, he became a graduate student in biology at Caltech in 1931. Bonner will become professor of biology emeritus this July.

Bonner has been internationally known for his work on how living cells specialize. He earned Caltech's first PhD in plant physiology in 1934. In his earlier research he continued in this field and in plant biochemistry, making substantial contributions in the discovery of hormones that regulate growth and that induce plants to flower and bear fruit. One of the successful applications of his research was a great increase in the yield of Malaysian rubber trees. In the early 1960s Bonner turned toward more fundamental biochemical research into the molecular basis of the control of gene activity in higher organisms. His insights into the biochemical signals that switch genes on and off opened a whole new research area in the structure and functioning of the genetic apparatus. These studies led him into genetic engineering.

Bonner has had a continuing interest in population growth, world food production, and in the future of industrial society. He was one of the originators of the "Next 100 Years" conference and book and of the subsequent Next 90 and 80 Years.

In 1973 he received the American Chemical Society's Richard C. Tolman Medal, and he is a member of this and many other professional societies, including the National Academy of Sciences, the American Academy of Arts and Sciences, and the American Association for the Advancement of Science. His books, *Plant Biochemistry* (three editions), *Principles of Plant Physiology, Molecular Biology of Development, The Nucleohistones,* and others, have been widely used in university courses. He has traveled extensively, both in a professional capacity and an avocational one — mountain climbing and skiing.

Bonner is not retiring. He and his wife, Ingelore, are the principal officers of their genetic engineering firm, Phytogen.

Francis H. Clauser
Clark Blanchard Millikan Professor of Engineering, Emeritus

Francis Clauser became professor emeritus last summer and now finds more time to travel — an interest that in the past has led the Clausers to drive by car to such far reaches of the globe as the Australian Outback, the length of South America, through the Middle East to Persia, and across the Sahara to old Timbuktu. They have most recently visited Mongolia and western China and are currently cruising the canals of France.

Clauser earned all his degrees at Caltech — BS in physics (1934), MS in mechanical engineering (1935), and PhD in aeronautics (1937). After leading a design research section at Douglas Aircraft, he worked with Project RAND, now the Rand Corporation, in 1946, and then accepted an invitation from Johns Hopkins to establish a department of aeronautics at that university. In 1965 he was named academic vice chancellor at UC Santa Cruz. He returned to Caltech in 1969 as Clark Blanchard Millikan Professor of Engineering to become chairman of the Division of Engineering and Applied Science, a job he relinquished in 1974.

In 1966 Clauser received one of the first Caltech Distinguished Alumni Awards. He is a fellow of the American Academy of Arts and Sciences and the American Institute of Aeronautics and Astronautics and a council member of the National Academy of Engineering. He has published widely in the fields of aerodynamics, space flight, fluid dynamics, applied mechanics, and nonlinear mechanics and is currently conducting research on the reduction of combustion engine emissions.

It was Clauser's idea to establish at Caltech what has become the Sherman Fairchild Distinguished Scholars program. This was not the only one of his ideas to bear fruit. In 1946 he was in charge of RAND's first report — *Preliminary Design of an Experimental World-Circling Spaceship* — in which he predicted the imminent feasibility of such a satellite and its emergence as one of the most potent scientific tools of the century. And in 1968 as a member of the NASA Science and Technology Advisory Committee for Manned Space Flight, he advanced the concept of reusability of space vehicles as the logical way to reduce the costs of space flight — the genesis of the Space Shuttle.

Leverett Davis, Jr.
Professor of Theoretical Physics

Arriving at Caltech as a graduate student in 1936 with a BS from Oregon State College, Leverett Davis stayed on after earning an MS (1938) and PhD (1941) to join the research staff. During the war years he worked on the exterior ballistics of rockets, receiving the President's Certificate of Merit in 1948. At the end of World War II he joined the Caltech faculty, becoming

Leverett Davis, Jr.

Marshall Hall, Jr.

George Housner

professor of theoretical physics in 1956. He will become professor emeritus in July of this year. During many of those years he acted as the unofficial faculty parliamentarian, providing the last word on proper procedures.

A National Science Foundation grant took him to Germany in 1957-58 for research at the Max Planck Institute for Physics in Göttingen. Davis's work has been concerned with the polarization of star light by interstellar dust grains that are aligned by the galactic magnetic field, with the acceleration of cosmic rays, with the characteristics of the magnetic field and plasma in interplanetary space, and with the properties of the solar wind. He helped plan, design, and carry out the magnetometer experiments on the Mariner and Pioneer spacecraft. In 1970 Davis received the Exceptional Scientific Achievement Award from the National Aeronautics and Space Administration for using spacecraft data to measure the magnetic fields near Mars, Venus, and Earth, as well as the high frequency waves in the Earth's magnetosheath.

Davis has participated in summer study groups on interplanetary and planetary exploration for the National Academy of Sciences, NASA, and the European Space Research Institute. He has been president of the Commission on Plasmas and Magnetohydrodynamics in Astrophysics, and a councillor of the American Physical Society. He is also a fellow of that society as well as the American Astronomical Society, and the American Geophysical Union.

Marshall Hall, Jr.

Professor of Mathematics

Marshall Hall, Jr. will become professor of mathematics emeritus on July 1. A member of the faculty since 1959, he was named Caltech's first IBM Professor of Mathematics in 1973 and was executive officer for mathematics between 1966 and 1969. His BS (1932) and PhD (1936) are from Yale, where he was assistant professor from 1941 to 1946; he served on the faculty of Ohio State University from 1946 until 1959, as full professor after 1949.

An internationally known mathematician, Hall has worked extensively in combinatorial analysis (the study of the arrangement of objects) and group theory (in particular, finite simple groups) and discovered important relationships between the two fields. He is also known for his research on theory of numbers and projective geometry.

Hall was elected a member of the American Academy of Arts and Sciences in 1975 and a fellow the following year. He is also a member of the Mathematical Association of America and the American Mathematical Society and has chaired symposia and summer institutes for the latter group. Hall had studied at Trinity College, Cambridge, as a Henry Fellow in 1932-33 and returned there on Guggenheim Fellowships in 1956 and 1971. In 1977 he was a visiting fellow at Merton College, Oxford, and in 1980 was the Lady Davis Visiting Professor at the Technion in Haifa, Israel. The Yale Graduate School awarded him the Wilbur Lucius Cross Medal in 1973 for his achievements.

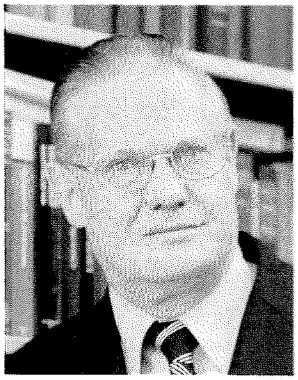
George W. Housner

Carl F Braun Professor of Engineering

George W. Housner becomes professor emeritus on July 1 after 36 years as a member of the Caltech faculty. He first came as an MS student in 1934, arriving from the University of Michigan with a BS degree, and after working as a structural engineer he returned in 1939 and received his PhD in 1941. In 1941-42 he was with the U.S. Army Engineer Corps, and from 1942 to 1945 he served with the Operations Analysis Section of the 15th Air Force in North Africa and Italy. He has been on the staff of the Institute since 1945.

Housner is internationally known for his research on strong earthquakes and their effects on structures. His work has been an important contribution to the development of earthquake-resistant design procedures now in worldwide use. He has frequently been consulted on the seismic design of major projects, including the San Francisco Bay Area Rapid Transit System, the long-span suspension bridge over the Tagus River in Lisbon, Portugal, high-rise buildings in Los Angeles, the California Feather River Water Project, the Trans Arabian Pipe Line, and nuclear power plants in the U.S., Japan, and Europe.

Housner is a member of the National Academy of Engineering and of the National Academy of Sciences. He is currently chairman of the Earthquake Advisory Board of the California Department of Water Resources and has served on the Governor's Council on Earthquake Hazards and the Los Angeles County Earthquake Investigation Commission. His awards include the Distinguished Service Award of the U.S. War Department, the Vincent Bendix Research Award of the American Society for Engineering Education and the von Karman Medal for Research of the American Society of Civil Engineers. He has served as president of the Seismological Society of America and the International Association for Earthquake Engineering. At Caltech he has been chairman and secretary of the faculty and is a member of The Associates.

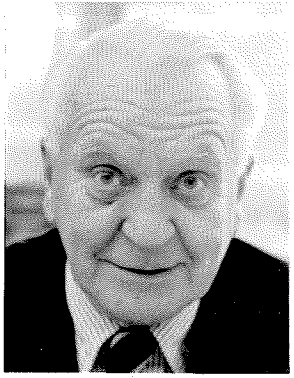

Donald Hudson

Donald E. Hudson
Professor of Mechanical Engineering and Applied Mechanics

After earning his BS (1938), MS (1939), and PhD (1942) at Caltech, Donald E. Hudson joined the faculty, becoming professor of mechanical engineering and applied mechanics in 1955. He will become professor emeritus next month.

A pioneer in developing analytical and experimental methods in earthquake engineering, he has made basic contributions to experimental techniques in structural dynamics, developing some of the first practicable devices for determining the response of full-scale structures to dynamic loads. He made the first of many visits to India in 1958, when he assisted in establishing the School of Earthquake Engineering at the University of Roorkee. At present he directs a joint Indo-U.S.A. program for the deployment of special instrumentation for the measurement of strong earthquake ground motions in the Himalayan regions. He has been active in similar instrumentation projects in California, Yugoslavia, and Argentina. Earthquake engineering interests have taken him on extended trips to South America in connection with UNESCO programs, to New Zealand, and to Japan.

Hudson was elected to membership in the National Academy of Engineering in 1974. He is a Fellow of the American Society of Mechanical Engineers, and past president of the Seismological Society of America. Other memberships include the Society for Experimental Stress Analysis, the American Society for Engineering Education, the American Geophysical Union, and the Earthquake Engineering Research Institute. In 1980 he was elected to a four-year term as president of the International Association for Earthquake Engineering.


Paco Lagerstrom

Paco A. Lagerstrom
Professor of Applied Mathematics

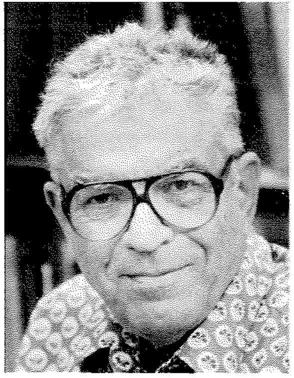
After 35 years on the Caltech faculty Paco A. Lagerstrom will become professor emeritus this summer. He first came to the Institute as a research associate in aeronautics, becoming professor of aeronautics in 1952. He has been professor of applied mathematics since 1967.

Born in Sweden, he studied at the University of Stockholm and earned a graduate degree in philosophy in 1939. In 1942 he received a PhD in mathematics from Princeton, where he remained as an instructor until early 1944. He then joined Bell Aircraft as an aerodynamicist and subsequently Douglas Aircraft where he stayed on as a consultant till 1966. Lagerstrom has been at Caltech since 1946 except for a year (1960-61) when he taught at the University of Paris on Guggenheim and Fulbright fellowships.

His early applied work was concerned mainly with aerodynamics and space dynamics (he was a contributor to Douglas's pioneering 1946 report on the feasibility of placing a satellite in space). Later on he turned to theoretical studies of viscous flow. This led to investigations of the basic ideas underlying singular perturbation techniques, the extension of those techniques, and their applications outside fluid dynamics, and he is currently finishing a book in this field. His early work on supersonic wings involved use of Lie's classical methods of applying group theory to differential equations, and recent research has focused on the extension of Lie's ideas to higher-order symmetries.

Lagerstrom has played a very active role in the local arts community. He has been a board member of the Coleman Chamber Music Association since 1950 and its president (1958-60). Other board memberships included the Southern California Chamber Music Society and several other music organizations, and he is a former trustee of the Pasadena Art Museum. In 1974 the Pasadena Arts Council presented him its Patron of the Arts award.

Lagerstrom also was named Chevalier (dans l'ordre des) Palmes Académiques (1963) and is a founding member of the international committee of Journal de Mécanique (Paris). He is a member of the American Mathematical Society and SIAM.


Robert Langmuir

Robert V. Langmuir
Professor of Electrical Engineering, Emeritus

Robert V. Langmuir came to Caltech for the second time in 1948. The first time was as a graduate student in 1935, and he received his PhD in 1943. Between 1942 and 1948 he worked for General Electric — on radar countermeasures until 1945 and then on the construction of a 70 MEV synchrotron. When he came back to the Institute it was as a senior research fellow, and his research was on the construction of a large electron synchrotron in the energy range between 600 million and one billion volts. One way or another, that project kept him and several other faculty members busy for the next 12 years. He became a full professor in 1957 and professor emeritus in 1980. He acted as head of electrical engineering from 1960 to 1970.

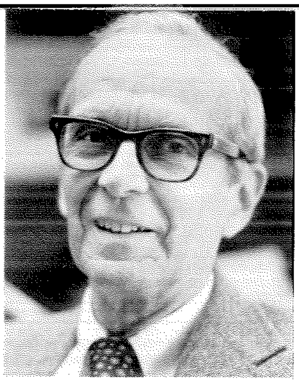
Langmuir was a consultant to TRW Inc. from 1956 to 1964 and to Consolidated Engineering Corporation from 1952 to 1954. He is a Fellow of the American Physical Society and a member of the Institute of Electrical and Electronic Engineers. His book, *Electromagnetic Fields and Waves,* was published by McGraw-Hill in 1960.

Gilbert D. McCann
Professor of Applied Science, Emeritus

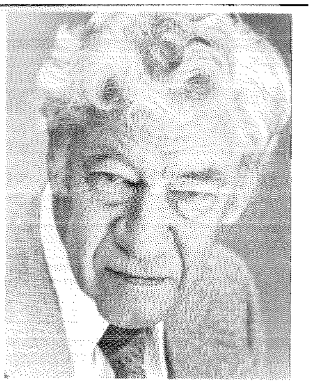
Gilbert D. McCann became professor of applied science emeritus last summer after 34 years on the Caltech faculty. He also received all three degrees from the Institute, his BS in 1934, MS in 1935, and PhD in 1939.

After several years at Westinghouse, where he initiated the company's large-scale computer program, he returned to

Gilbert McCann



Rodman Paul



Duncan Rannie

Caltech in 1946 to begin a research program in the development of analog and digital computers and their applications to engineering and numerical analysis. From 1947 to 1966 he was professor of electrical engineering and thereafter until retirement was professor of applied science. He was named director of the new computing center (later known as the Willis H. Booth Computing Center) in 1966, a post he held until 1971.

McCann presided over the initial expansion of Caltech's campus-wide, interactive computer system, making it accessible to students as well as faculty, and for teaching purposes as well as research. His own early research was directed toward miniaturization of electronic components for computers, while subsequent work took him into the application of computer technology to biology. He was particularly concerned with basic processes of sight perception and how animals can translate what they see into thought and action. His most recent research has involved combining x-ray and computer techniques applied to such diverse fields as archaeology and studies of the brain.

In 1942 McCann received the Eta Kappa Nu Award for Outstanding Engineer. He is a fellow of the Institute of Electrical and Electronics Engineers and a member of several other technical societies, as well as the Caltech Associates.

An avocation for McCann over the last few years has been raising Arabian horses. He hopes in retirement to have more time to devote to this.

Rodman W. Paul

Edward S. Harkness Professor of History

Rodman Paul, a leading authority on the history of the American West, will become professor emeritus next month after 34 years at Caltech. During that time he also served as acting chairman of the Division of the Humanities and Social Sciences (1978) and was twice cited by the student body for excellence in teaching (1977 and 1980).

Paul received his AB, AM, and PhD degrees from Harvard, where he was also an instructor and assistant dean. After service in the Navy during the war, he taught briefly at Yale before coming to Caltech in 1947. He has been here ever since with the exception of a year at Oxford (1955-56) on a Ford Foundation grant.

In addition to his numerous professional memberships, Paul is currently president of the American Historical Association's Pacific Coast branch and has been elected a fellow of the Society of American Historians and the California Historical Society. He was president of the Western History Association in 1978 and from 1968 to 1977 was a member of the Advisory Council of the National Archives in Washington and also served as its chairman. Last year he was elected to membership in the famous old American Antiquarian Society, founded during the War of 1812. He has also been a member of the board of editors of half a dozen major historical journals.

Paul has published several books and numerous articles on the American West, particularly the mining era. His book *California Gold: The Beginning of Mining in the Far West* (1947) won the annual prize of the American Historical Association's Pacific Coast branch, and an essay by him won the annual prize as the best article published in the *Pacific Historical Review* in 1959. He has also written extensively on California history and is currently doing research for a book on the Far West and the Great Plains between 1859 and 1900.

W. Duncan Rannie

Robert H. Goddard Professor of Jet Propulsion and Professor of Mechanical Engineering

Duncan Rannie, who came to Caltech to study under Theodore von Kármán, will become professor emeritus this summer. With a BA (1936) and MA (1937) from the University of Toronto, Rannie was appointed a graduate assistant in mathematics and aeronautics at Caltech in 1938. For the next three years he worked with von Kármán on a number of projects, including the aerodynamic design of the Smith-Putnam windmill and the investigation of the failure of the Tacoma Narrows Bridge. From 1941 to 1946 he headed the aerodynamics group responsible for gas turbine development at the Northrop-Hendy Company. In 1946 he went to the Jet Propulsion Laboratory as chief of the Ramjet Section. He was appointed assistant professor of mechanical engineering in 1949, associate professor in 1951, the same year he completed his PhD, and professor in 1955. The following year he was named the second Robert H. Goddard Professor of Jet Propulsion and the title of Professor of Mechanical Engineering was added in 1978.

Rannie is known for his work in several branches of fluid mechanics, in particular the aerodynamics of turbomachines and of heat exchangers. He has done extensive research on the design of compressor blading, on stalling of compressors and on the flow of particulates in rocket engines. He is a Fellow of the American Institute of Aeronautics and Astronautics, a corresponding member of the International Academy of Astronautics and a member of the National Academy of Engineering.

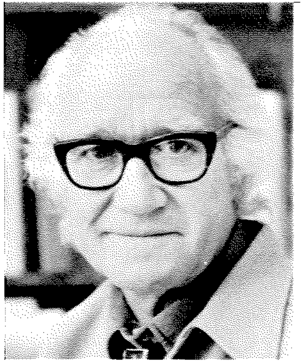
John Todd

Professor of Mathematics

John Todd, who joined the Caltech faculty in 1957, will become professor emeritus on July 1. An expert in numerical mathematics, he has devoted most of his research over the past 40 years to exploitation of the computer in various fields of mathematics, science, and engineering.

Educated in his native Northern Ireland and at Cambridge, England (where he studied under the legendary J. E. Littlewood), he received his BSc in 1931 from Queen's University, Belfast, where he was a lecturer from 1933 to 1937. Between 1937 and 1949 he taught at King's College, University of London, and it was there that he met and married Olga Taussky who also became a noted mathemati-

John Todd



Robert Walker

cian. During the war he was a scientific officer with the British Admiralty. In 1945 he was instrumental in preventing the dissolution of the Research Institute at Oberwolfach, an organization that has since made unique contributions to the mathematical sciences. In 1947 he was invited to work on high-speed computing at the National Bureau of Standards in Washington, D.C., and California. Except for some time with John Von Neumann's group at Princeton's Institute for Advanced Study, and a brief return to London, Todd remained at the Bureau for ten years, first as chief of its Computation Laboratory (one of the first such to be equipped with an electronic computer, the SEAC) and then as chief of Numerical Analysis. In 1957 he became professor of mathematics at Caltech.

Todd was also a Fulbright Professor at the University of Vienna in 1965. He has been active in various professional societies, in particular the Mathematical Association of America, of which he is at present a governor.

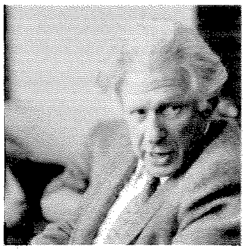
Robert L. Walker
Professor of Physics

Robert L. Walker, who has also been executive officer for physics since 1976, will become professor emeritus this July. He has elected early retirement and plans to move to New Mexico.

Walker earned his BS at the University of Chicago in 1941 and during the war worked on the Manhattan Project at both the University of Chicago and Los Alamos. After a year as research associate at Cornell University, where he earned his PhD in 1948, he came to Caltech as an assistant professor. He became associate professor in 1953, spent a year in Italy on a Fulbright Fellowship in 1955-56, and has been full professor since 1959.

Experimental high energy physics is Walker's field, and he has been particularly concerned with the design of detectors for high energy physics. During his early years at Caltech he was involved in the construction and operation of the billion-volt electron synchrotron, which at the time was the most powerful machine of its type. Much of his research was devoted to the experimental study of pion photoproduction reactions and to the theoretical interpretation of photoproduction data. After 1970 he worked on pion charge exchange and related reactions at the Fermi National Accelerator Laboratory.

His book, *Mathematical Methods of Physics,* written with Jon Mathews, was published in 1964. Walker is a fellow of the American Physical Society and a member of the American Association for the Advancement of Science. ☐



An Interview with Marvin Goldberger

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GOLDBERGER: That is the case, and that is being changed by a program now in its formative stages. We expect to emphasize in our fund-raising activities over the next three or four years those things that will contribute heavily to our unrestricted funds. The mechanism for doing that is to try to greatly increase the number of endowed professorships, which will serve the purpose of giving appropriate recognition to professors on the campus. It will also relieve the general budget effectively and therefore turn even highly restricted grants instantly into unrestricted funds. And it gives us an excellent lever for attracting outstanding people from the outside.

TIM BRAZY: Tuition has been raised by $1,000 for next year. Do you see this kind of increase continuing for the next few years?

GOLDBERGER: Well, the tuition is going up very rapidly all over the country. We're still $1,000 or so behind the Ivy League schools. I think our tuition will continue to climb. I worry about this seriously, of course, because when tuition climbs, we have to find adequate funds for student aid. You know we don't deny people entrance on financial grounds. So you sort of get caught coming and going.

WASSERBURG: What is your view about faculty salaries, in particular for junior faculty? A small study has shown that these people are suffering some substantial economic jeopardy, and that means that the institutions are in danger of not being able to attract really outstanding young people on which the future of their institutions must clearly depend.

DANIEL KEVLES: Particularly in very high demand fields like engineering.

GOLDBERGER: Well the problem in engineering is really acute in all of the fashionable fields — computer science, electrical engineering, solid state physics. With current industrial engineering salaries, students in those fields have little incentive to take a job at half the salary at a university. But there are only a certain number of things that one can do to alleviate this situation. One thing we're trying to interest donors in is making prize junior appointments — like the Noyes instructorship in chemistry — jobs that have some perks such as certain amounts of funds available for research, for travel, or we might even want to allow a half year off during the first three or six years appointment for a sabbatical. As far as competing directly with salaries, I don't see how we can ever do that.

LIST: How do medical and dental schools do it, and law schools? They have exactly the same kind of problem — competing with a professional income that may run into six figures. Yet they seem to be able to find very good people to teach.

GOLDBERGER: If I'm not mistaken, in these medical, dental, and law schools, professors are allowed very generous moonlight privileges. We have, of course, a somewhat restrictive policy about outside consulting, and at least for us I think by and large it's a good policy because of the opportunities for abuses under a liberal consulting policy.

ALBERT LIN: Do you think that Caltech will start doing classified research if it becomes apparent that fiscally it is necessary to do so?

GOLDBERGER: I doubt it. I would be strongly opposed to doing classified research at Caltech unless it met two conditions: first, that it was clearly perceived by all of us here as being absolutely necessary in the national interest and, second, that we had some truly unique capability to do so. I certainly would not flee to the classified research coffers just to keep going with business as usual.

It is necessary to distinguish classified research from research with Department of Defense funding. There was a time when a tremendous amount of basic research without any strings whatsoever was supported by the DOD. Recently, there has been a desire on the part of DOD to get back into the support of basic research in universities, but in two successive years Congress cut the funds. Now it may be with the current enthusiasm for DOD spending — and these being trivial amounts of money by comparison with most of their outlays — that basic research money might come through. I have no objections whatsoever to taking money from DOD under those circumstances, but it wouldn't be for classified research.

KEVLES: Would you say the same for proprietary research in regard to industry as well?

GOLDBERGER: Yes. I think that setting foot on that particular slippery slope can completely distort the whole Institute, and I would be strongly opposed to it. The recent Harvard farrago of trying to set up a private corporation in the university provides a very stern lesson.

WASSERBURG: What do you envision the role of Caltech should be over the next decade, in particular with regard to the function and role, first, of private institutions and, second, of those whose dominant effort is directed toward excellence in science, engineering, and technology — as distinct from humanities, economics, or any other field?

GOLDBERGER: I think the continuing role of the Institute is to train excellent students and to produce excellent research. The fact that the preponderance of effort at Caltech has historically been in the physical and biological sciences doesn't in my mind preclude our having a selective and excellent humanities and social sciences activity.

I'm not concerned that the Institute will lose its leadership position in the physical and biological sciences if it should acquire excellence in humanities and social science. In other words, it's not a zero-sum game, and there is no need to dilute the historic strengths of the Institute.

WASSERBURG: You mean that it is still the major goal of the institution to maintain absolute excellence in science, engineering, and technology as the primary goal and these other things as ancillary features of the education program, or are you saying that the balance of the educational goals should shift? The facts are that historically the California Institute of Technology has always had an extremely strong training for undergraduates in the humanities, and that training has much distinguished its scientific and engineering product. I did not intend to demean these other fields but to ask what is your sense of the balance and primary goal of the institution.

GOLDBERGER: I don't like saying that one thing is primary and another thing is secondary. I want us to be the best in everything that we choose to do. Of course, 80 percent of the activity here is in the physical and biological sciences, so that excellence in those fields by definition becomes a primary goal. I won't compromise with any of those, I won't dilute those, and I won't in any way deflect us from trying to be the very best in the world in all of those subjects, while at the same time doing the absolute best job I can in the humanities and social sciences.

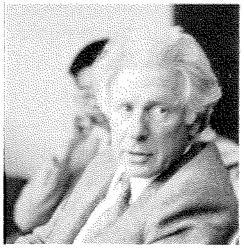
KEVLES: When you came here to discuss the possibility of becoming president of Caltech, you said that you had ambitions for the improvement of undergraduate education. How do you feel about the issue of undergraduate education at this point?

GOLDBERGER: Well, I feel a great deal better about it than I did, largely as a result of the efforts of David Goodstein in his reorganization of the physics program. I think the fact that the core physics curriculum has as instructors senior professors from all over the Institute is a very positive sign and clearly one that the students have noticed and appreciated. I don't know whether it's because I want to see it, but I find a little bit less of a sort of Marine Corps boot camp attitude on the part of some of the faculty with respect to the students, and I think that's positive.

One thing that has happened the past two summers that I think has had a very therapeutic effect on undergraduate education and the attitude of the students is the Summer Undergraduate Research Fellowship (SURF) program. So far it has touched only a minority of the students, but it will be expanding. This program provides an opportunity for students to spend a really meaningful period at an early stage in their lives learning that science is not just working a set of problems at the end of a chapter, with answers that all come out to be rational fractions.

LIST: An aspect of this that I'd really like to follow up on is that 50 percent of the undergraduates are now opting as upperclassmen to become engineers. Along the same lines, my friends on the freshman admissions committee tell me that the high school students they see are all computer mad. They all want to come here because they see a future associated with computers. What's the Institute's position going to be if it gets to the point where 65 percent or 70 percent of our undergraduate students want to do engineering, and of those, half or more want to do computer science? As it now stands, for the last 10 or 15 years the relative contribution to the engineering division's budget for instruction and research has actually gone down although the number of students has gone up, and it seems to me that the problem is only going to get worse as more and more students become enraptured with the whole business of chasing binary digits.

NORMAN DAVIDSON: To put the question more charitably — supposing you can see that one particular field is a very valuable intellectual enterprise and also one which is pretty marketable right now, so we have quite a few of our students choosing this particular option, but maybe ten years from now they'll all be going into recombinant DNA. How do you reconcile this with the general Institute goal of maintaining excellence in a number of



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fields irrespective of student enthusiasms at a particular time?

GOLDBERGER: That's a very important consideration. We can't afford to overreact to whatever is the current interest of the student body, but I think John has touched on a very important point. The engineering and applied science enterprise here is in the process of rebuilding now. The rate at which it rebuilds is necessarily slow because what's happening here is not an isolated phenomenon; it's happening all over the country. MIT is experiencing the same shift from the pure sciences to engineering — in fact, maybe by an even greater percentage than we have experienced. We're pushing forward in engineering and applied science from two standpoints: one, from the standpoint of encouraging additional faculty appointments, and second, designating funds to provide the appropriate facilities, the start-up costs, for the people who will come.

LIST: That's one aspect of it; the other aspect of it is how to control what the undergraduates want to do.

GOLDBERGER: I don't think that's controllable. You know, undergraduates come in with particular interests that we respond to as best we can. We try to present them with the very best shopping list that we have available, knowing that a lot of their interests will change over a period of a couple of years. It's also true, however, that these same computer interests are going to play a more and more important role across the board in all of the sciences. I don't think you can undo that particular fascination.

LIST: I'm not particularly wanting to undo it; I'm just wondering how the Institute is going to respond to it.

GOLDBERGER: We have to respond. We have to have people to teach courses, obviously. At the same time we have to be careful not to unbalance the size of the faculty in response to it. It's a delicate line that we have to draw.

BRUCE SAMS: You said you were interested in improving some of the humanities, and I want to ask why it is virtually impossible to get credit for the performing arts. Is that making art any less valuable than studying about it? What is your position on that?

GOLDBERGER: Course credit is a faculty decision, and every time the issue arises, as far as I can tell from studying the faculty minutes, there is an intense and passionate discussion. The comparison with credit for Phys Ed is raised, invidious comparisons are made . . .

WASSERBURG: Fly-casting 1? Ping Pong 2?

GOLDBERGER: Right. All kinds of problems arise, but I'm quite open to a recommendation from the faculty on this.

WORKMAN: I'd like to pursue this one a little further. I think Caltech has a history — its administration — of listening to what students have to say. The meeting among students, faculty, and alumni a year ago was a recent example of that. I have the impression as an alumnus that the vocal students claim to have a very important role in the decision-making process both academically and administratively. Do you see a growth in student influence on campus?

GOLDBERGER: It's my feeling that although students have access to the decision-making process in the form of potential membership on a variety of committees, they don't really take advantage of it, but this is something David may be in a better position to comment on than I am.

GOODSTEIN: Students are duly appointed to all committees; whether they exercise influence on the committees or not, one would have to ask the students.

WASSERBURG: This question goes together with the other discussion of changes in the undergraduate curriculum. There are specific items like course changes which are very positive and I applaud them. On the other hand, I think there's danger in citing these items as a reflection of actual improvement of circumstances in undergraduate and graduate education. There's a problem of student participation in these things, and I think there really has to be some sort of reassessment of what should happen in this general area, taking into account the very small size of Caltech. Where do you think revitalization of the total undergraduate endeavor might come, with an increase in student vigor and participation?

WORKMAN: It's really a question of administrative attitude relative to the students.

GOLDBERGER: Well, I think it's more than administrative attitude. The differences that I see in undergraduate education here as compared to that in other institutions are not anything terribly concrete — not course content or structure or anything like that. It is a question of attitudes. And changing the minds and hearts of the people, as we have learned from sorry experience, is sometimes a very slow process. But I have a feeling that it is changing, albeit slower than one would like to see. Whether it could be enhanced by improvement of communication between students and faculty and students and administration I don't know. I'm quite open to any mechanisms that might be suggested to speed up the process.

KEVLES: In the old days, before you came here, there was intermittent but persistent talk about changing attitudes by changing the environment for undergraduate education. The idea was to try to bring to the Institute a different kind of student; that is, to diversify the undergraduate body somewhat. Do you have any such notions in your mind?

GOLDBERGER: I think probably one would like ideally that the students not only be as incredibly bright as they are but that they be socially adept and mature and so on. But we are an unusual, special institution, and I don't think that without dramatically changing the character of the place we could have an impact by evolutionary changes in the admissions procedure.

BRAZY: There's been a lot of talk about attrition in the past. A report was made at the faculty-student conference last year that attrition was primarily due to innate characteristics of the students, not characteristics of the Institute. How do you feel about that?

GOLDBERGER: I think there's some truth and some falseness to it. The best we

can do is to make sure if students drop out of Caltech that it's not because we are turning them off or that we're presenting them with a social structure which is so hostile that they can't make an appropriate adjustment to the situation.

I was very concerned about the dropout rate when I first came here, but the more I thought about it, the less worried I became. When students first come here, they're often almost monomaniacal about their interests, and then they suddenly discover a lot of different things are available in the world that they didn't appreciate before. So they may not be as configured for a dedicated life of binary digits as at first they thought. But there is very little room to move laterally within Caltech, in contrast to major liberal arts colleges. And so they go elsewhere, and we just don't have very much control over that.

WASSERBURG: Don't you think something like a senior thesis would be a major step in alleviating some of the interaction problems between the students and the faculty? It might decrease the dropouts and transfers and increase the level, frequency, and quality of interaction between students and faculty and between the students themselves.

GOLDBERGER: Well, I think it would increase the quality of interaction appreciably, but unfortunately a senior thesis carries the word senior, so the students could well have dropped out before they had this wonderful enriching experience. But I think the SURF program has the capability of having a very therapeutic effect on students, increasing their interaction with the faculty.

NORMAN DAVIDSON: Are you thinking that that could become large enough to really include a significant fraction of the students? If so, it would be marvelous.

GOLDBERGER: It would obviously have to be enlarged because it would require an enormous commitment on the part of the faculty. You know, it's not easy to think up things for these students to do in a two-month period — things that will really attract their attention and not be totally mechanical. One of our Associates who was a major contributor last year to the SURF program has undertaken, almost as a personal crusade, to greatly increase the funds that will be made available for that program in the future.

ERIC DAVIDSON: When I was in college, for better or worse I spent a lot of time — all four years, not just summer — doing research in the laboratory. And I find that surprisingly uncommon here, at least in biology. A relatively small number of the undergraduates take advantage of the existing opportunities, mostly because of the course pressure. I wonder if something like the SURF program, which is for 2 months out of the 12, could actually substitute for more serious encouragement to regard research as part of an education in the sciences.

GOLDBERGER: I don't know, Eric. It's a hard question. Reducing the course load to a certain extent, so that people would feel freer to become involved earlier on in the laboratories, would require a serious change in attitude on the part of the faculty, who are very intense about the material that they want to transmit in a given period of time. When you have very bright students, the temptation to teach them absolutely everything is almost irresistible. You get so caught up in that that you don't really give the students enough free time to think. Now people learn in different ways; some people probably flourish in this kind of intense pressure of course work. Others learn more slowly and have to have time to contemplate. There probably is no ideal system.

WASSERBURG: To what extent do you think this high-pressure environment is really a reflection of the quarter system rather than the semester system?

GOLDBERGER: Well, I know from my own experience that there is a dangerous tendency to regard quarters as semesters and to become compulsive about covering a certain amount of material. I think we may be trying to teach too much to undergraduates.

WORKMAN: Is there something you personally can do to change the attitude of the faculty to include more things like SURF?

GOLDBERGER: I don't know what I can do except to implore, . . . I mean, I don't have any powerful tools.

KEVLES: Command?

GOLDBERGER: Commanding doesn't get very far on this campus. Maybe certain kinds of blandishments . . .

KEVLES: Apropos command and blandishments, would you compare your expectations of being president of Caltech and the actual experience? And, also, would you care to comment on what it's like to be a university president these days?

GOLDBERGER: Before I became president, I didn't have much idea of what the job entailed. When I came here to be interviewed, the first person I saw was Bob Christy, who was acting president, and I asked him two questions. The first thing I said to him, because he is a very old friend, was, "Bob, am I really nuts to think about this at all?" And he said, "No, you're not nuts to think about it." And then my second question was, "What do you do when you come in your office in the morning?" He just laughed hysterically, and at that time I didn't know why. Now I know.

I had talked with some elder statesmen before I came, and they informed me that one of the problems is that you have very many bosses — students, faculty, trustees, staff. And I sort of understood that, but I guess the thing that I was least prepared for was how hard you have to work by comparison with being a professor. It's a more high pressure job, in which your time is really not your own. Someone once likened the position of department chairman to being a half-time job; it's five minutes off and five minutes on. And that's what makes the pressure. It's a continuing series of interruptions of any kind of coherent thought.

Usually, by the time people come to see me, they have something fairly serious on their minds. So I have to think very hard about something for half an hour, and

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then I have to turn that off somehow and think very hard about something else. And that is so different from what you do when you are actually doing research, when you think very hard about something for three hours, three days, three weeks, and you don't think about anything else. It's physically tiring too.

Of course there are certain kinds of mandatory social activities, to which I had never been exposed. They are different and in many cases a tremendous amount of fun. I've enjoyed a great deal of that side of the job. I like to talk to people, and that's what I do most of the day, but sometimes when I come home, all I want to do is pick up the newspaper and read it. I don't want to talk then because I've been talking for eight continuous hours, and what I would like more than anything else is just to keep quiet for a few minutes. I generally recover from that.

KEVLES: You regard this as a pretty doable job, not personally doable only, but institutionally and structurally, despite all the constraints, even the many bosses? (Incidentally, you left out the federal government, which is a very important boss.) One finds presidents of other institutions saying after some years that the job is not doable . . .

NORMAN DAVIDSON: But the institutions are functioning.

GOLDBERGER: Institutions, of course, have a tremendous amount of inertia, and if there were no one in this office, much the same thing would go on, I'm afraid. Sometimes my influence is much less than I would like it to be. At least the response time is much slower. I feel it's manageable, Dan. I don't think I could realistically be president of an institution that didn't have such a large component of scientific activity which, if I don't understand in detail, I'm at least not terrified by.

WASSERBURG: Do you think you receive enough support in terms of carrying out your functions as president of Caltech — enough support from the students, the faculty, your administrative associates?

GOLDBERGER: I feel the support from my administrative associates is superb. I think I probably don't go to the faculty with as many requests for support as I might. And that isn't because I don't trust them, but I somehow haven't reached out to them in the way that I think I could. Under circumstances in which I have asked for help, I have found total support. As far as support from the students is concerned, I don't quite see how that fits into the scheme of things, but I try to be as responsive and open to the students as I possibly can.

GOODSTEIN: We have not had a Nobel Prize at Caltech since 1969. Ought we to be worried about that?

GOLDBERGER: I worry about it every October.

GOODSTEIN: I don't mean to be facetious. A place like Caltech either improves or gets worse; it doesn't stay the same.

GOLDBERGER: I agree. I think we must continue to work very hard to bring to Caltech absolute top, first-rate people, but a Nobel Prize is a very capricious measure of success. It has a profound effect on institutions. It gives them a visbility to students that is off-scale. It offers an almost irrational attractiveness to students, who somehow equate the presence of Nobel laureates with excellence in graduate education. But there are a few people around here who I feel are logical potential recipients of the Nobel Prize, and I always grieve when they don't get it. I'm sure that they do too.

There's a question I would like to ask if I may. Are there areas of science or knowledge more generally, including the humanities and social sciences, that Caltech is not now doing that it ought to be doing? Are we overlooking some opportunity that we might seize on? I want to put a boundary condition on this question, namely that I don't want to play catch-up. I don't want to start doing something just because other places are doing it. What I really want to do is the equivalent of hiring Thomas Hunt Morgan and starting biology. That's what I would like to do. It's not easy to think of such things.

LIST: Well, there are two paths which are obviously converging in the future — biology and computer science, the whole business of memory storage. Nobody seems to have approached the biological memory aspect of it, which is orders of magnitude higher than the things that people in the solid state area do. It's clear that somewhere out there they are going to converge.

GOLDBERGER: If I had to pick a particular area, that generalized information-system area is one that I would like to try to emphasize. That's the most obvious one.

LIN: Dr. Goldberger, you are in a very interesting position because you are regarded as an authority in science and technology by virtue of your personal credentials and by your position as president of Caltech. Furthermore, you have a personal interest in some of the applications of technology as evidenced by your current series of talks here at Caltech regarding war and arms control. You have also spoken to other groups in Los Angeles about this and other topics. What do you see as the effect of such talks in areas where you are an authority and where you are trying to influence people to do something?

GOLDBERGER: As for the effect, I believe we are beginning to see a growing sophistication among people about the real implications and dangers associated with the arms race and with nuclear war. How much of an effect this will have on national policy depends upon how much this grass roots movement continues to grow.

I'm concerned somewhat about my going around giving these speeches because although I always try to emphasize that I'm speaking as an individual and not necessarily representing the views of the institution, it's hard for me to divorce myself from the institution. And I suppose that people come to hear me at least in part because I am the president of Caltech. But I feel strongly about the international security situation and that it's important for those people who have experience in it to speak out. I can at least tell the facts and make it more reasonable for people to be in a position to advise their representatives in the government what to do — or to throw out those representatives who seem to be insensitive to what they feel are important issues. So I've got to do it. □

ALUMNI FLIGHTS ABROAD
1980~1981

This program of tours, originally planned for alumni of Harvard, Yale, Princeton, and M.I.T., is now open to alumni of California Institute of Technology as well as certain other distinguished colleges and universities. Begun in 1965 and now in its sixteenth year, it is designed for educated and intelligent travelers and planned for persons who might normally prefer to travel independently, visiting distant lands and regions where it is advantageous to travel as a group.

The program offers a wide choice of journeys to some of the most interesting and unusual parts of the world, including Japan and the Far East; Central Asia, from the Khyber Pass to the Taj Mahal and the Himalayas of Nepal; the surprising world of South India; the islands of the East, from Java and Sumatra to Borneo and Ceylon; the treasures of ancient Egypt, the world of antiquity in Greece and Asia Minor; East Africa and Islands of the Seychelles; New Guinea; the South Pacific; the Galapagos and South America; and more.

REALMS OF ANTIQUITY: A newly- expanded program of itineraries, ranging from 15 to 35 days, offers an even wider range of the archaeological treasures of classical antiquity in Greece, Asia Minor and the Aegean, as well as the ancient Greek cities on the island of Sicily, the ruins of Carthage and Roman cities of North Africa, and a comprehensive and authoritative survey of the civilization of ancient Egypt, along the Nile Valley from Cairo and Meidum as far as Abu Simbel near the border of the Sudan. This is one of the most complete and far-ranging programs ever offered to the civilizations and cities of the ancient world, including sites such as Aphrodisias, Didyma, Aspendos, Miletus and the Hittite citadel of Hattusas, as well as Athens, Troy, Mycenae, Pergamum, Crete and a host of other cities and islands of classical antiquity. The programs in Egypt offer an unusually comprehensive and perceptive view of the civilization of ancient Egypt and the antiquities of the Nile Valley, and include as well a visit to the collection of Egyptian antiquities in the British Museum in London, with the Rosetta Stone.

SOUTH AMERICA and THE GALAPAGOS: A choice of itineraries of from 12 to 29 days, including a cruise among the islands of the Galapagos, the jungle of the Amazon, the Nazca Lines and the desert of southern Peru, the ancient civilizations of the Andes from Machu Picchu to Tiahuanaco near Lake Titicaca, the great colonial cities of the conquistadores, the futuristic city of Brasilia, Iguassu Falls, the snow-capped peaks of the Andes and other sights of unusual interest.

EAST AFRICA—KENYA, TANZANIA AND THE SEYCHELLES: A distinctive program of 5 outstanding safaris, ranging in length from 16 to 32 days, to the great wilderness areas of Kenya and Tanzania and to the beautiful islands of the Seychelles. The safari programs are carefully planned and comprehensive and are led by experts on East African wildlife, offering an exceptional opportunity to see and photograph the wildlife of Africa.

THE SOUTH PACIFIC and NEW GUINEA: A primitive and beautiful land unfolds in the 22-day EXPEDITION TO NEW GUINEA, a rare glimpse into a vanishing world of Stone Age tribes and customs. Includes the famous Highlands of New Guinea, with Sing Sings and tribal cultures and customs, and an exploration of the remote tribal villages of the Sepik and Karawari Rivers and the vast Sepik Plain, as well as the North Coast at Madang and Wewak and the beautiful volcanic island of New Britain with the Baining Fire Dancers. To the south, the island continent of Australia and the islands of New Zealand are covered by the SOUTH PACIFIC, 28 days, unfolding a world of Maori villages, boiling geysers, fiords and snow-capped mountains, ski plane flights over glacier snows, jet boat rides, sheep ranches, penguins, the Australian "outback," historic convict settlements from the days of Charles Dickens, and the Great Barrier Reef. Optional visits can also be made to other islands of the southern Pacific, such as Fiji and Tahiti.

CENTRAL ASIA and THE HIMALAYAS: An expanded program of three itineraries, from 24 to 29 days, explores north and central India and the romantic world of the Moghul Empire, the interesting and surprising world of south India, the remote mountain kingdom of Nepal, and the untamed Northwest Frontier at Peshawar and the Punjab in Pakistan. Includes the Khyber Pass, towering Moghul forts, intricately sculptured temples, lavish palaces, historic gardens, the teeming banks of the Ganges, holy cities and picturesque villages, and the splendor of the Taj Mahal, as well as tropical lagoons and canals, ancient Portuguese churches, the snow-capped peaks of the Himalayas along the roof of the world, and hotels which once were palaces of maharajas.

THE FAR EAST: Itineraries which offer a penetrating insight into the lands and islands of the East. THE ORIENT, 30 days, surveys the treasures of ancient and modern Japan, with Kyoto, Nara, Ise-Shima, Kamakura, Nikko, the Fuji-Hakone National Park, and Tokyo. Also included are the important cities of Southeast Asia, from Singapore and Hong Kong to the temples of Bangkok and the island of Bali. A different and unusual perspective is offered in BEYOND THE JAVA SEA, 34 days, a journey through the tropics of the Far East from Manila and the island fortress of Corregidor to headhunter villages in the jungle of Borneo, the ancient civilizations of Ceylon, Batak tribal villages in Sumatra, the tropical island of Penang, and ancient temples in Java and Bali.

Prices range from $2,350 to $4,500 from U.S. points of departure. Air travel is on regularly scheduled flights of major airlines, utilizing reduced fares which save up to $600.00 and more over normal fares. Fully descriptive brochures are available, giving itineraries in detail and listing departure dates, hotels, individual tour rates and other information. For full details contact:

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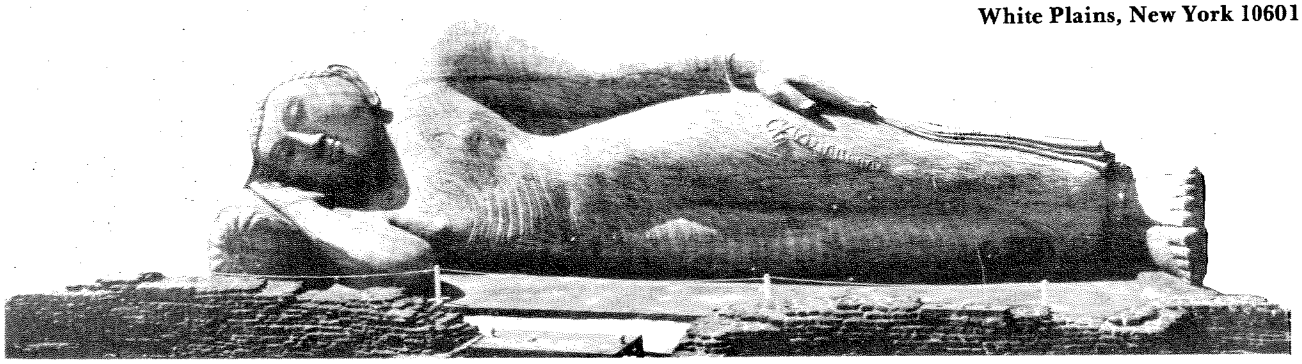
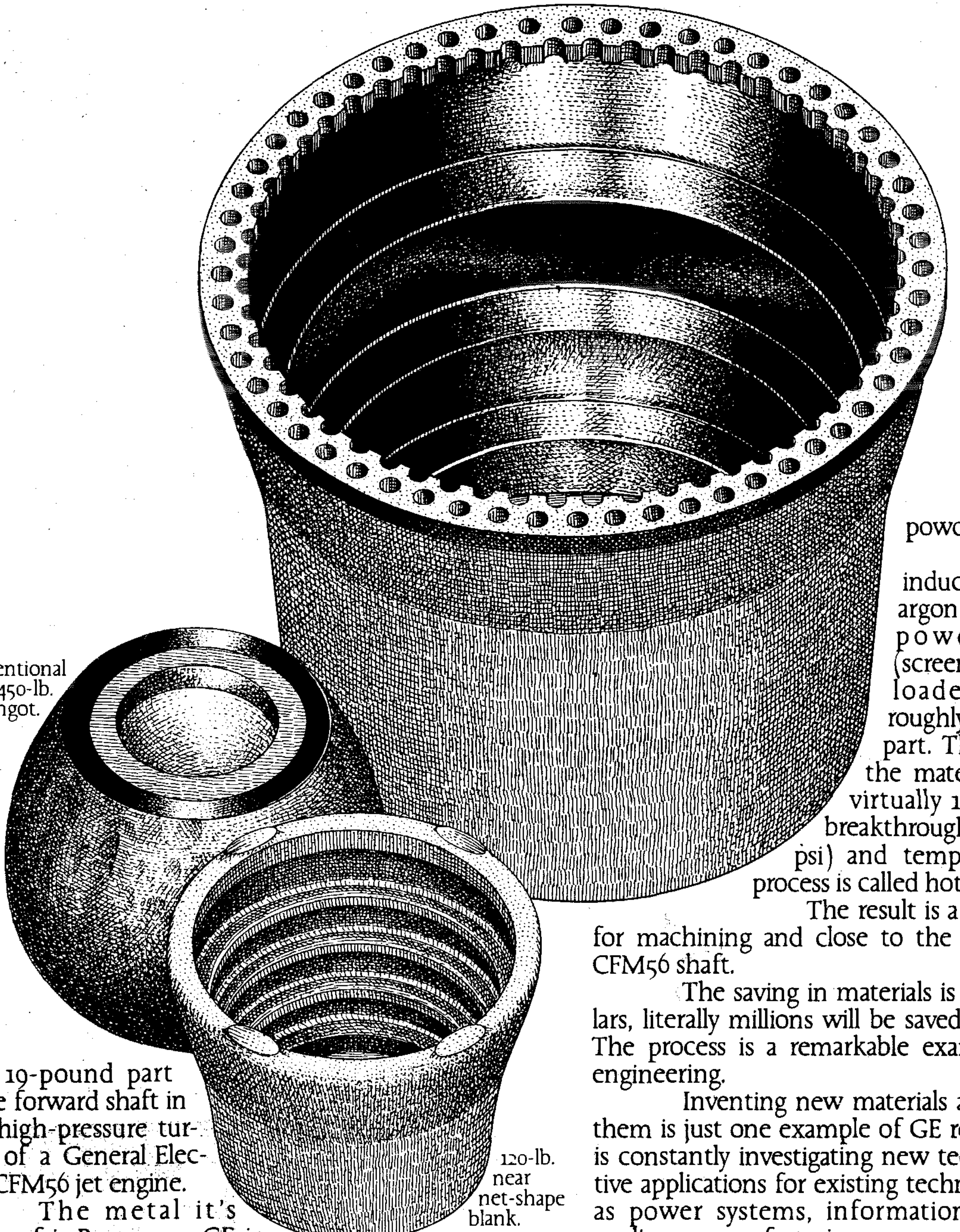


Why should it take 450 pounds of metal to make a 19-pound part?



Conventional 450-lb. ingot.

120-lb. near net-shape blank.

The 19-pound part is the forward shaft in the high-pressure turbine of a General Electric CFM56 jet engine.

The metal it's made of is Rene 95, a GE invention. Rene 95 is an exotic superalloy of nickel, cobalt, columbium, tungsten and 17 other elements. To fabricate a forward shaft from Rene 95 by conventional methods, you start with a 450-pound ingot. After forging, pressing and machining, you end up with a single 19-pound shaft...and more than 400 pounds of expensive scrap.

That's a distressing waste of critical raw materials and of the energy it takes to mine and refine them.

So GE engineers turned to near net-shape forming: fabricating the finished part from a blank shaped as closely as possible to the shape of the finished part.

But how could such a blank be created without starting with a 450-pound ingot? To solve that problem, GE engineers developed a truly unique application of powdered metallurgy.

Virgin or vacuum induction-melted Rene 95 is argon-atomized to create a powder. The powder (screened for particle size) is loaded into containers roughly shaped like the final part. Then, in an autoclave, the material is consolidated to virtually 100% density (that's a breakthrough) at high pressure (15K psi) and temperature (2000°F.). The process is called hot isostatic pressing.

The result is a 120-pound ingot ready for machining and close to the shape of the finished CFM56 shaft.

The saving in materials is more than 70%. In dollars, literally millions will be saved over the next decade. The process is a remarkable example of cost-effective engineering.

Inventing new materials and better ways to use them is just one example of GE research in progress. GE is constantly investigating new technologies and innovative applications for existing technologies—in such areas as power systems, information services and major appliance manufacturing.

This takes talent—engineering talent—not just in research and development, but in design and manufacturing, application and sales.

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