APRIL 1969

E&S ENGINEERING AND SCIENCE

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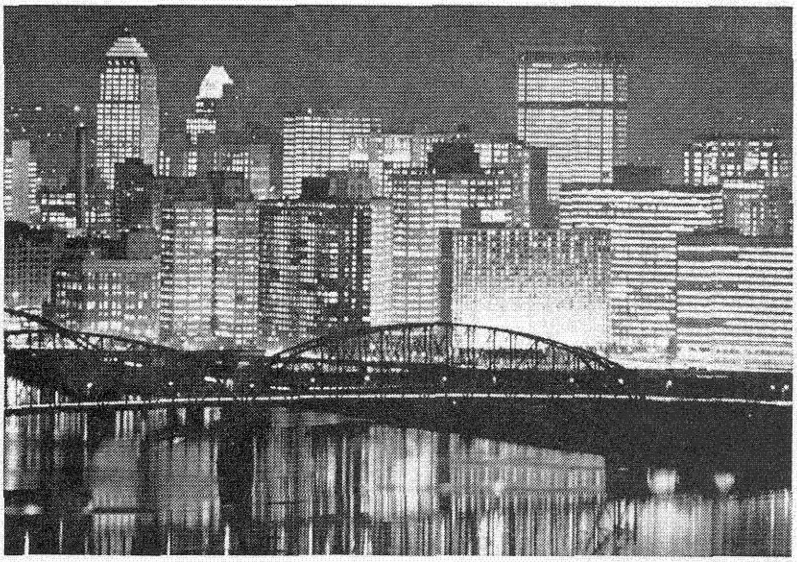
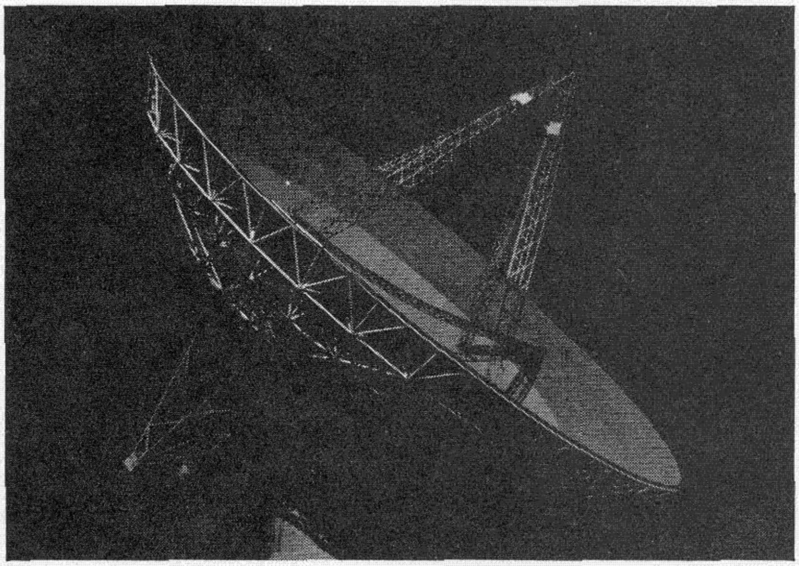


unless they prefer to work in the dark.



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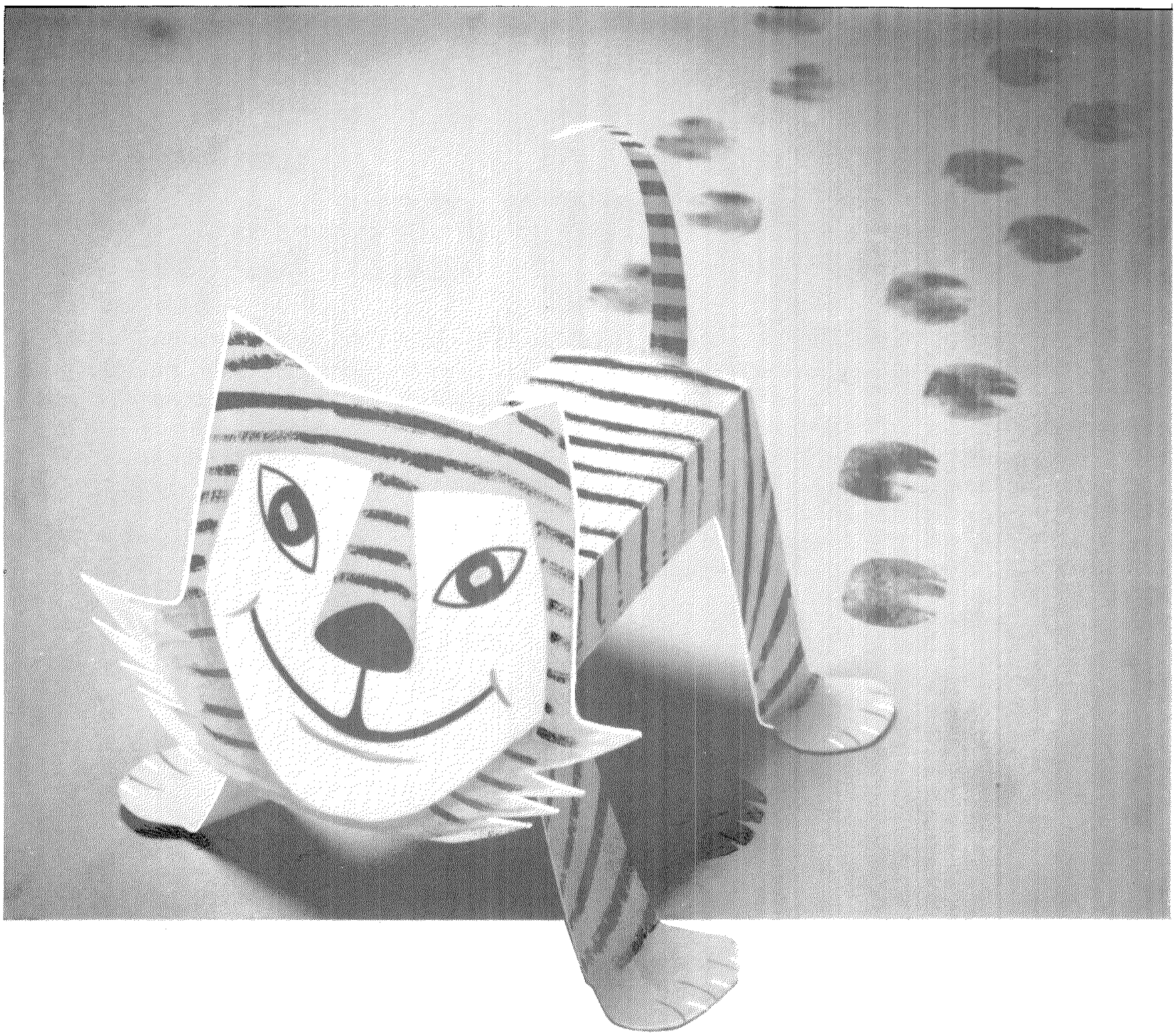

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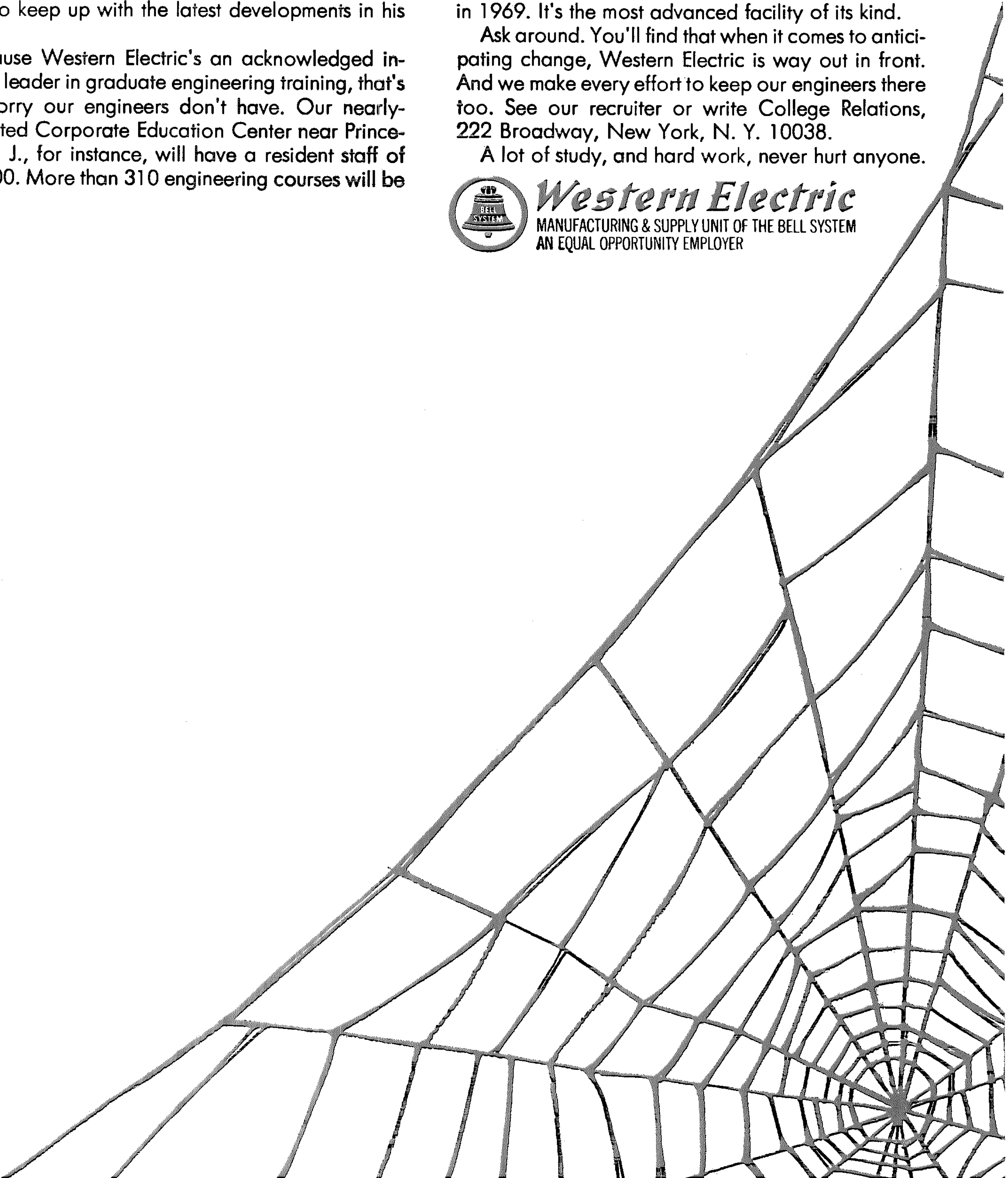

E&S

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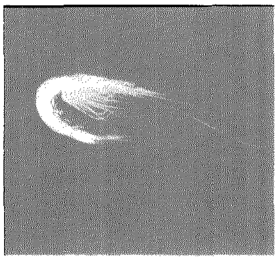

On The Cover

—the opossum shrimp, *Neomysis rayi,* which plays an important part in the continuing study by Caltech's Heinz Lowenstam to determine the impact of biological evolution on the physical chemistry of the oceans. The discovery that this tiny marine creature can make the mineral fluorite—and the significance of this discovery to the ecology of the ocean—is reported on page 28.

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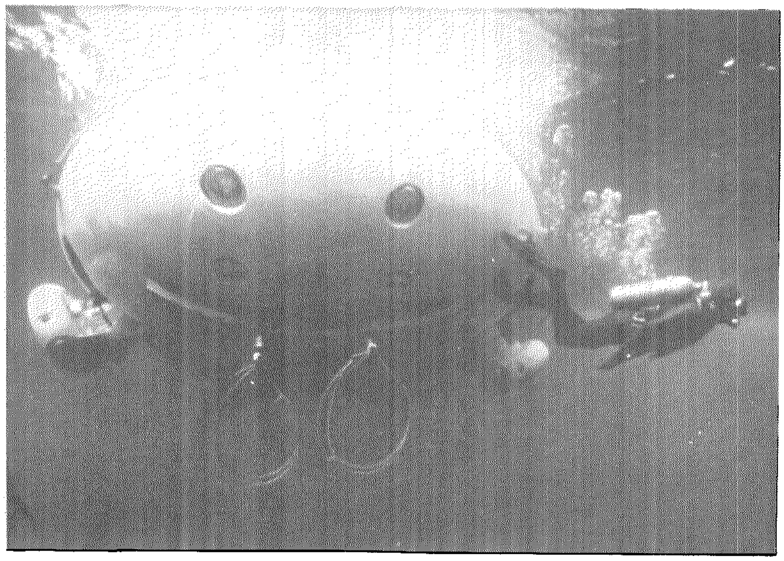
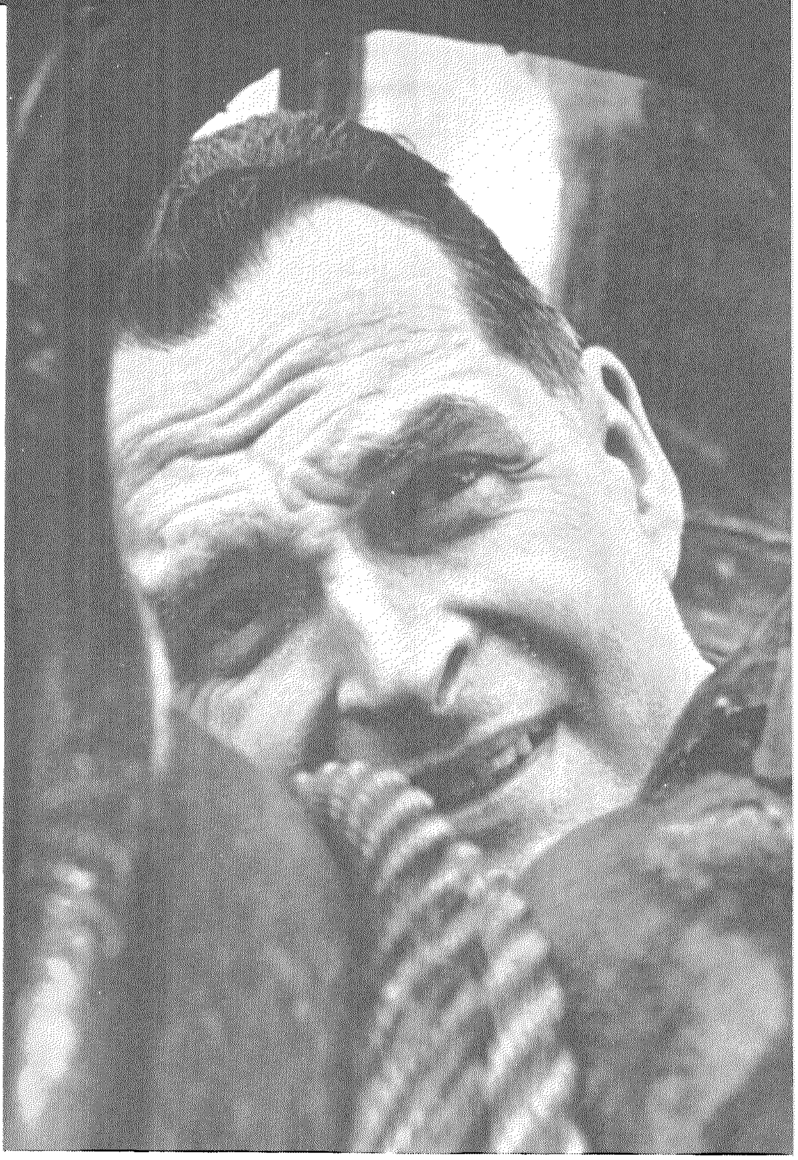
"We've got to stop

"These old fishing boats have had it. We need more floating factories, taking the haul from dozens of trawlers, processing it on board, into frozen fish fillets, canned fish, fish oil, fish flour. This takes materials that can survive the sea."

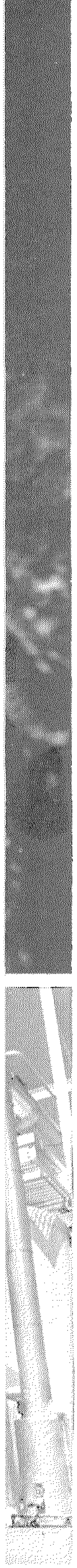


"Take Lockheed's *Deep Quest*. Nickel maraging steel from our research, enabled it to dive to a record depth of 8,310 feet. Whether you start with steel or copper, nickel adds properties that make it work—strength, toughness, resistance to corrosion."

"Drinking water? In desalting the sea, the name of the game is heat transfer. You need a conductor, so you need a metal. But you need one that will stand up to saltwater corrosion. Copper-nickels helped make multi-million-gallon-a-day plants practical."



ishing like St. Peter"



Art Tuthill of International Nickel talks about working the sea—and the role of metallurgy in solving population problems.

"With a world population of roughly three billion, one billion are underfed," says Tuthill. "Yet in the sea there's enough protein for thirty billion. But harvesting it takes more than the will. It takes hardware."

Tuthill covers the marine industry—shipbuilding, ocean engineering, water desalting. He talks like an ecologist, an economist, an engineer. He's a materials expert, on call for any problem in his field.

"And," he says, "we need more from the sea than you can get with a net. Our demand not only for food, but water, and minerals, is growing relentlessly. It takes machines the sea can't destroy.

"One of the primary products of the Nickel company is factual data on performance of materials, their fabricability, their life-cycle economics. We stand with one foot in the laboratory, one in the application. Our aim is to make things happen."

Nickel helps other metals stand up to heat, cold, impact, pressure, abrasion, as well as corrosion. Nickel alloys help advance engineering in vital fields—power, electronics, transportation, aerospace, chemicals, as well as the marine sciences.

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We count our blessings and respect our surroundings. From nickel ores we also recover platinum, palladium, and twelve other commercially useful elements. We make iron pellets for steel. Smoke in our stacks is converted to chemicals for still other industries. And on sand left from processing ore, we grow meadows of hay to control dust storms.

We are 32,500 people in 18 countries—miners, market builders, researchers. We work at the whole job—from geodesy to agronomy, metallurgy to economics, theory to practice. We bring new opportunities to underdeveloped lands, new payrolls, new tax income.

Nickel in the ground is useless. We put it to work.

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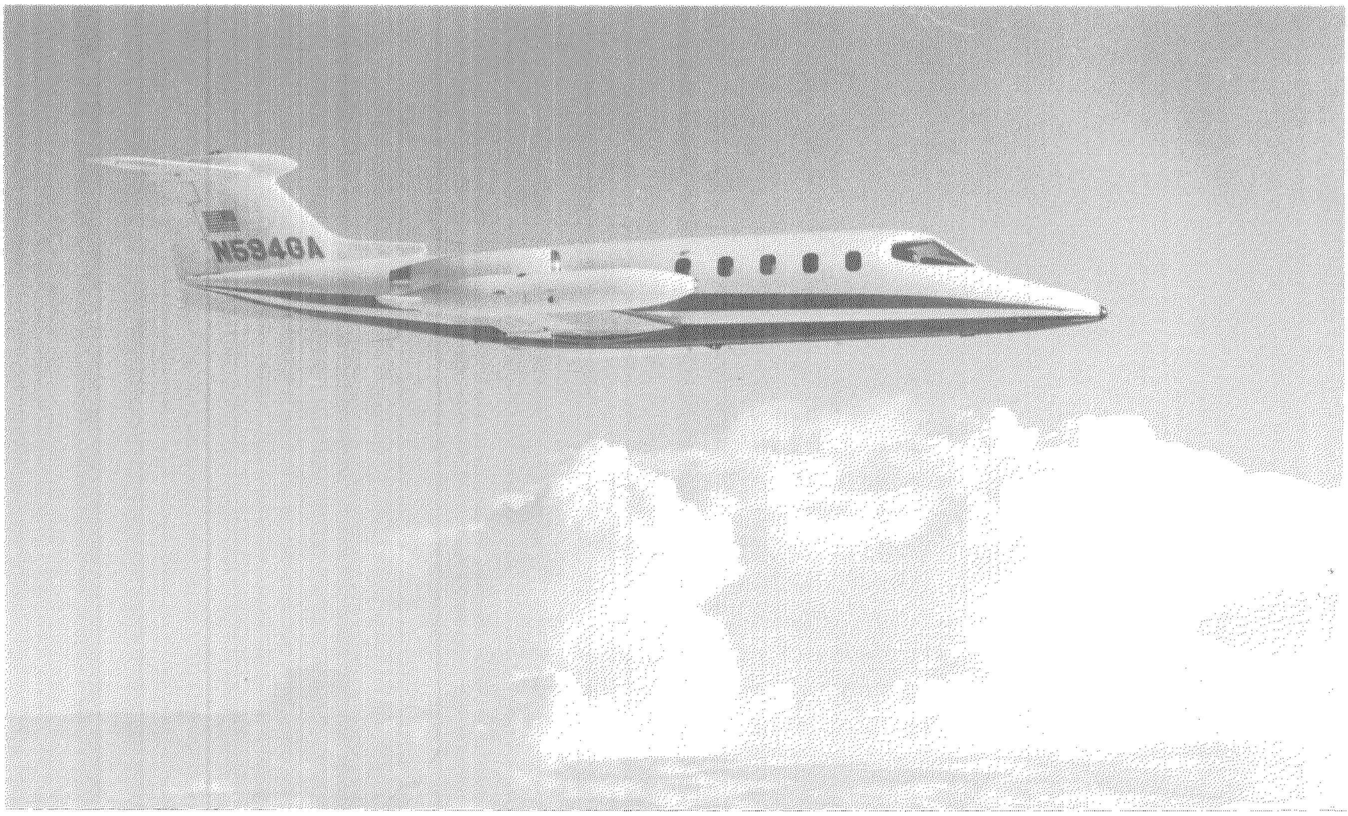
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E&S
April 1969, Vol. XXXII, No. 7

The Prospect of Designed Genetic Change

"It has now become a serious necessity to better the breed of the human race. The average citizen is too base for the everyday work of modern civilization. Civilized man has become possessed of vaster powers than in old times for good or ill but has made no corresponding advance in wits and goodness to enable him to direct his conduct rightly." This was written in 1894 by Sir Francis Galton. The concerns of the present are clearly not new.

It has long been apparent that you and I do not enter this world as unformed clay compliant to any mold; rather, we have in our beginnings some bent of mind, some shade of character. The origin of this structure—of the fiber in this clay—was for centuries mysterious. In earlier times men sought its trace in the conjunction of the stars or perhaps in the momentary combination of the elements at nativity. Today, instead, we know to look within. We seek not in the stars but in our genes for the herald of our fate.

Today there is much talk about the possibility of human genetic modification—of designed genetic change, specifically of mankind. A new eugenics has arisen, based upon the dramatic increase in our understanding of the biochemistry of heredity and our comprehension of the craft and means of evolution. I think this possibility, which we now glimpse only in fragmented outline, is potentially one of the most important concepts to arise in the history of mankind. I can think of none with greater long-range implications for the future of our species. Indeed this concept marks a turning point in the whole evolution of life. For the first time in all time a living creature understands its origin and can undertake to design its future. Even in the ancient myths man was constrained by his essence. He could not rise above his nature to chart his destiny. Today we can envision that chance—and its dark companion of awesome choice and responsibility.

It is all too easy, albeit useful, to let our imagination in these matters roam far beyond our technical base. It is easy, even for modest men given to cautious projection, because in truth all that seems needed is the technology and the resolution to transfer to man what we already know to be feasible in bacteria or carrot cells or frogs. It is easy because there are no known natural laws to repeal or contravene. None of the time warps or hyper-drives or teleportation of science fiction are needed to envision vegetative reproduction, organ regeneration, genetic therapy, or eugenic transformation of our species.

I would like, however, to consider a very specific and possible use of our newer knowledge, relating to a major biomedical problem. This application may well seem of small dimensions as compared to some of the more sweeping prospects, but I believe it will illuminate the state of our knowledge and our technology and will thereby reveal the shape of things to come.

I want to use the phrase "genetic change" in a broad sense, in the sense of altering some physiological or psychological process which at present we believe has been programmed into us through our inheritance. And I will assume that such change might be achieved either in a strictly genetic mode through a change in our inherited characteristics, or in a somatic (non-inheritable) mode—possibly through a change in the time or place or degree of action of our inherited genetic components, or

ROBERT L. SINSHEIMER,

chairman of the division of biology, discusses the possibility of human genetic modification— "potentially one of the most important concepts to arise in the history of mankind."

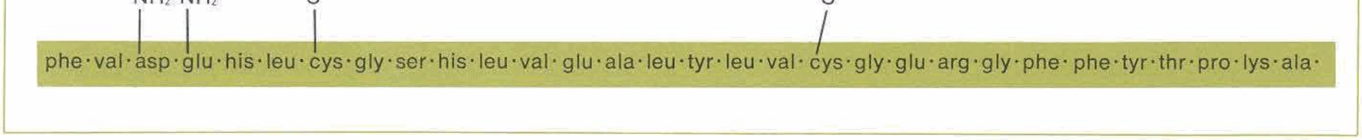
possibly through the somatic addition of genetic components. Obviously changes of the former—the truly genetic type—have the greater ultimate potential; for the very nature of the species seems potentially susceptible to change. Changes of the latter type—somatic genetic modifications—are more limited. Their scope and function are the more restricted, but they are also undoubtedly the more accessible possibilities which we will first achieve.

There are in the United States today some 4,000,000 clinical diabetics. Many of these people are kept alive only by repeated, frequent injections of the hormone insulin. It is believed that there are several million more cases with marginal symptoms. Without recurrent injections of insulin many of these people would perish. While it keeps them alive, the injection of insulin is not the full equivalent of a normal physiological function; diabetics are known to be more susceptible to disease, to heart and circulatory illnesses, and other physical limitations than non-diabetics.

I propose that genetic therapy offers the promise of a much more elegant, and indeed more satisfactory, physiological solution to this ailment. And there are various possible genetic approaches.

To begin we must understand the normal process of insulin formation. Insulin is a protein, composed of two polypeptide chains—one of 21 amino acids and one of 30—joined by two disulphide bonds. There is recent evidence that indicates strongly that the insulin molecule is initially formed as a single polypeptide chain, and an internal segment is subsequently excised by the action of a specific proteolytic enzyme.

The synthesis of this protein, the proinsulin, is accomplished in the usual manner: The hereditary instructions specifying the sequence of amino acids for insulin are encoded in a segment of the DNA

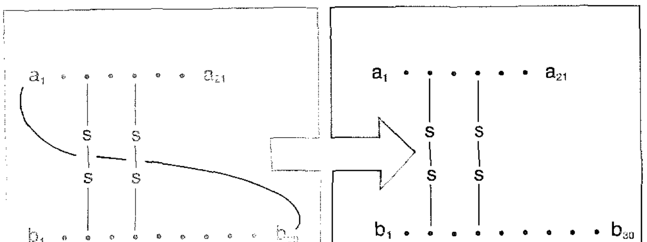


The chemical structure of human insulin consists of two polypeptide chains, one of 21 amino acids and the other of 30, which are joined by two disulphide bonds.

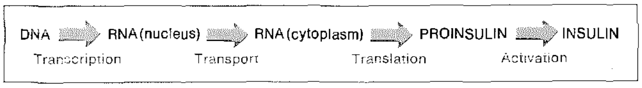
from the cell nucleus. The instructions are copied and transcribed into a messenger RNA molecule which then is transported out of the nucleus to the cytoplasm. There protein synthesis takes place upon the ribosomes. In this process the sequence of nucleotides in the RNA is translated into the corresponding amino acid sequence with the help of the transfer RNA molecules, the activating enzymes, the initiators, the coupling factors, and all the rest of a very complex machinery.

It is well known that this synthesis of insulin normally takes place only in the beta cells in the Islands of Langerhans in the human pancreas. In the diabetic these cells fail to produce an adequate amount of insulin. Now it is believed, and there is good reason for this belief from studies of lower animals, that the *full* DNA content of the genome is present in every somatic cell. And thus we believe that the genetic instructions specifying the sequence of proinsulin are present in all the cells of the body and not only the beta cells of the Islands of Langerhans. Evidently these instructions, though present in other cells, are not in use. Either they are not activated, or, as it is more fashionable to assume these days, they are repressed. Repression could take place at any of several levels.

A typical somatic cell is only called upon to use a small fraction of its genome. There is good evidence that in a liver or a muscle cell no more than 5 percent of the DNA is ever transcribed into RNA, so there is repression at the chromosomal level. Further, it is clear that perhaps half or more of that which *is* transcribed never reaches the cytoplasm to be translated. And even if the RNA reaches the cytoplasm, there is evidence for specific blocks at the translational level. There are clearly many opportunities for the restriction of expression of the inherited genetic instructions.



This schematic drawing of the conversion of proinsulin to insulin illustrates the recent evidence that the insulin molecule is formed as a single polypeptide chain and that an internal segment is subsequently excised by the action of a specific proteolytic enzyme.



Steps in the biosynthesis of insulin. Repression of insulin synthesis could take place at any of these stages.

In the case of insulin we do not know by what means the expression of this gene is limited to a few islands of cells. We do not know at what level the restriction is imposed. However, one approach to the problem of diabetes would be to attempt to turn on the synthesis of insulin in another set of cells.

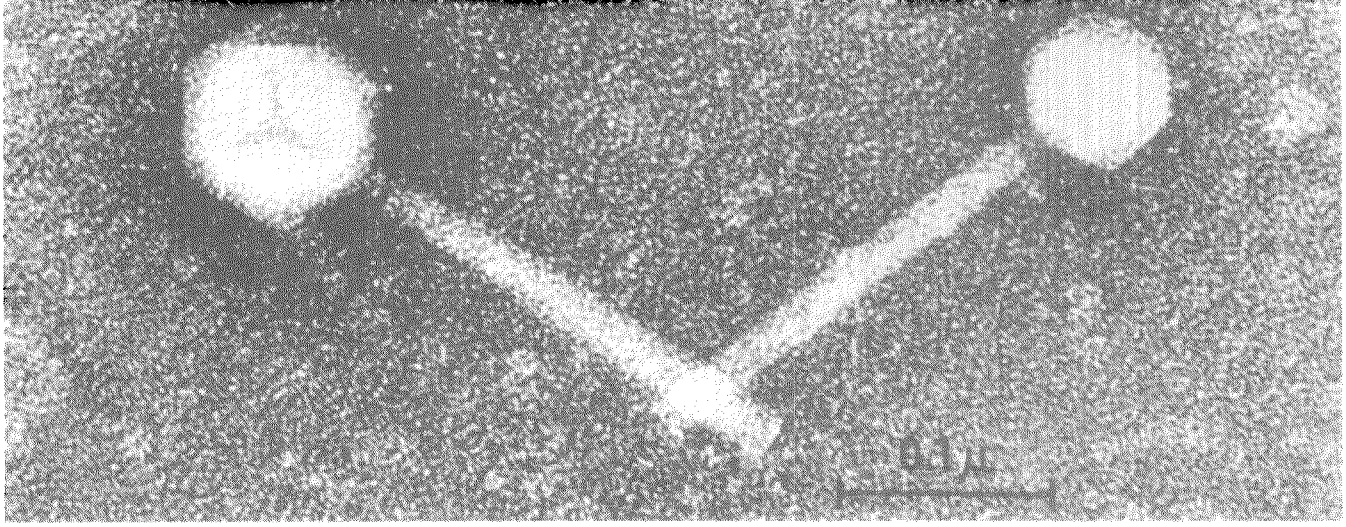
We do know that genes can be turned on by external influence. Hormones do this every day. For example, under the influence of cortisone, liver cells initiate the synthesis of a variety of enzymes including tryptophan pyrrolase and tyrosine-alpha-ketoglutarate transaminase.

In some instances the prior repression appears to be lifted hormonally at the chromosomal level of transcription, in others, at the translational level. We do not now know how we might do this for insulin, but we can see a clear model. And in fact just such an activation or derepression for insulin *must* have occurred through some chain of ontogenetic events during embryonic development to activate—to turn on—the appropriate genes in the beta cells of the Islands of Langerhans.

We should not oversimplify this problem. Obviously if we want a new group of cells to synthesize insulin, we must not only activate the gene for proinsulin but also arrange for its conversion to insulin and for its release from the cells. But, if we were fortunate, these functions might all come as a genetic package.

There is a radically different genetic approach that we might take alternatively. Instead of an attempt to lift this profound repression of the expression of the gene for insulin, we might, in principle, supply to a group of cells a wholly new gene or set of genes which would code for the synthesis of insulin and which might *not* be subject to the normal somatic pattern of repression.

How might we add, in such a specific manner, to the genetic components of a cell? Our models come from studies with bacterial cells. In these organisms a variety of means exist to permit exchange and in so doing to provide small increments of genetic material. These include transformation, contact



Transduction (the transfer of genetic material from one cell to another) can be observed with the bacterial virus P 1. In this electron micrograph of P 1 virus the one-inch line (lower right) represents 1/10 of a micron.

transfer both chromosomal and episomal, and transduction both general and specific. Organelles for contact transfer are not known among mammalian cells, and transformation as such has not yet been convincingly demonstrated in mammalian cells. Therefore, the possible use of transduction as a means to genetic modification of cells of higher organisms should be specifically considered.

Transduction among bacteria involves the transfer of genetic material, DNA, from one cell to another through viral mediation. I would like to present two particular cases.

The first case is that of the bacterial virus P 1, which contains one molecule of DNA of about 60,000,000 in molecular weight. Upon infection of the cell by certain types of P 1, the cell is lysed (broken down) after half an hour to produce a few hundred progeny virus particles. Most of these will contain a DNA identical to that of the virus that initiated the infection. However, a little less than 1 percent of the particles will contain *instead* a piece of the DNA of the chromosome of the host bacterium, a piece also about 60,000,000 in molecular weight. Which piece of DNA—which particular 60,000,000 out of the 3 billion molecular weight of host DNA—is random. The particular virus will contain the piece carrying, say, genes D and E, while another carries a piece with the genes P and Q, etc.

By appropriate means these particles carrying host DNA, called transducing particles, can be separated from those carrying the normal viral DNA. When such transducing particles are added to susceptible bacteria, the DNA inside the virus particle is, in the usual way of bacterial viruses, injected into the cell. But *now* we have added to the cell not a destructive virus genome, but a piece of bacterial DNA which may well carry genetic markers not present in this particular host. This DNA may be transcribed at once to yield new protein.

For this piece of DNA to perpetuate itself, however, it must, in general, become incorporated *into* the host chromosome by a process of genetic recombination. Normal bacterial cells have the enzymatic machinery to do this, and, in the case of the transducing particles of phage P 1, there is about one chance in ten that the particular piece of DNA will be so incorporated and perpetuated.

In bacterial cells there are often small secondary chromosomes—episomes—usually containing 1 or 2 percent as much DNA as the principal chromosome. These are physically separate from the principal chromosome, but usually replicate in synchrony with it. It is possible for a P 1 phage to pick up and transfer an entire episome as well as a piece of bacterial chromosome.

A second case of transduction concerns the temperate (frequently non-lethal) bacteriophage lambda. Upon infection with the bacteriophage lambda, the result in an appreciable percentage of the cells (it can be the majority) is the physical incorporation of either the viral DNA or of one of its descendents into the chromosomes of the host. Following this, the *virus-like* tendencies of this DNA are suppressed. The cell survives and multiplies, and the incorporated viral DNA is replicated into each daughter cell along with the rest of the bacterial chromosome. Such a virus-carrying cell is said to be a lysogen.

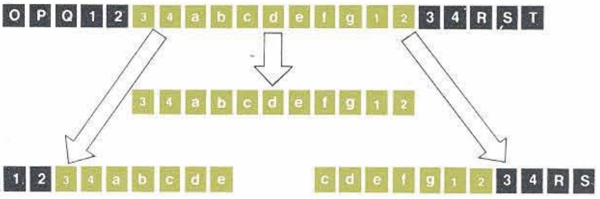
An important feature is that the point of insertion for the lambda DNA into the host DNA is specific, and it is determined by the particular virus which in turn specifies an enzyme—an integrase—which brings about its incorporation at that site. Related strains of lambda-type viruses are known which integrate into *other* chromosomal sites because they have different integrases.

It is possible, however, to induce an activation of this carried viral DNA in the lysogenic cells—to cause it to remember that it really is a virus, to cause it to break out of the bacterial chromosome, to begin to multiply, to produce progeny, and to lyse the cell, producing new virus particles.

Occasionally in such an activation, which is called induction, the piece of DNA which splits out of the chromosome is not strictly the viral genome but may incorporate a piece of the neighboring bacterial chromosome with its genetic material in lieu of a piece of the viral genome. Under certain circumstances viral development can proceed anyway. Such pieces of DNA, partially viral and partially host, can multiply and can be incorporated into virus particles. Particles with this mixed DNA can be isolated from the bulk of the progeny. If they are now added to susceptible cells, this DNA can still integrate into the host chromosome at the same locus but now adding along with the viral genes a specific piece of DNA from the former host—which may carry specific novel genetic traits into the new host.

In both of these cases, then—Pl and lambda—the net result is the introduction, via a particle normally indistinguishable from a virus, of new genetic material into the host cell. In the first instance the new factors added are random relative to the host genome. In the second, they are factors found at specific sites near the normal region of integration of the virus. The region varies in different viruses.

Could a similar transfer be accomplished with a virus in the cells of higher organisms? We have every reason to think that it does occur. Upon infection of mammalian cells with the simian virus 40, or with polyoma virus, or with some strains of adenovirus, in a fraction of such infected cells the viral DNA becomes established within the cell. Whether it is integrated into the chromosomal DNA or is an episome is not known. It is then perpetuated within the clone of cells descended from the original infected cell, as in a lysogenic bacterium. The information carried in the viral DNA is certainly expressed; cells carrying such DNA have altered properties; that messenger RNA derives from this viral DNA can be demonstrated; new protein antigens have been detected within such cells; and in special circumstances the entire genome of the virus can be recovered (and hence must have been present) from remote descendents of such altered cells.



In excision of a lambda DNA from the host chromosome, normal excision (center) yields one complete viral genome, while abnormal excision (left and right) yields mixed genomes, part viral and part host.

Technically and literally the stage is set. If we could obtain a virus analogous to simian virus 40—able to persist within altered cells and carrying an expressible gene for proinsulin in lieu of a normal viral gene—we might indeed be able to provide a genetic alternative to the daily injection of insulin.

The problem then is, where are we to find this virus so propitiously carrying a gene to provide insulin? Such a virus might exist in nature, but I propose that we should quite literally, in time, be able to make it to order. We will have the ability in the not distant future to synthesize a polynucleotide chain capable of coding for insulin and for the other genes necessary to integrate the DNA into a chromosome, or to maintain it as an episome, or whatever. And we will then also be able to package this *de novo* DNA into an appropriate virus coat.

Is this pure fantasy? No, not really. The DNA of simian virus 40 consists of a chain of 5,000 nucleotides. The art of specific polynucleotide synthesis is young but thriving. It is now feasible to construct a specific sequence of 50 deoxyribonucleotides. A sequence of up to 100 seems close at hand, and a thousand or a few thousand is by no means inconceivable.

Furthermore, such a synthesis needs to be done only once. Once the DNA is available, nature provides the means to copy it with the highest fidelity.

Similarly, our understanding of the process of viral self-assembly is growing swiftly, and but a small step behind is the art of viral assembly *in vitro*. The technology needed for such a radically different approach to a major clinical problem is almost in reach.

Though the analogy is not perfect, in describing these prospects I feel strangely akin to the physicists who pointed out in the 1930's that the principles required for the release of the energy locked in the atomic nucleus were understood. All that was needed was a practical breakthrough and the requisite technology. Here, too, the principles seem in hand. All that seems really needed is optimism, sustained effort, and support commensurate with the importance of the problem.

The larger and the deeper challenges—those concerned with the defined genetic improvement of man—perhaps fortunately are not yet in our grasp, but they are etched clear upon the horizon. We should begin to prepare now for their reality.

It is worthwhile to consider specifically wherein the potential of the new genetics exceeds that of the old. To implement the older eugenics of Galton and his successors would have required a massive social program carried out over many generations. Such a program could not have been initiated without the consent and cooperation of a major fraction of the population, and would have been continuously subject to social control. In contrast, the new eugenics could, at least in principle, be implemented on a quite individual basis, in one generation, and subject to no existing social restrictions.

The old eugenics would have required a continual selection for breeding of the fit, and a culling of the unfit. The new eugenics would permit in principle the conversion of all of the unfit to the highest genetic level.

The old eugenics was limited to a numerical enhancement of the best of our existing gene pool. The horizons of the new eugenics are in principle boundless—for we should have the potential to create new genes and new qualities yet undreamed. But of course the ethical dilemma remains. What are the best qualities, and who shall choose?

It is a new horizon in the history of man. Some may smile and may feel that this is but a new version of the old dream of the perfection of man. It is that, but it is something more. The old dreams of the cultural perfection of man were always sharply constrained by his inherent, inherited imperfections and limitations. Man is all too clearly an imperfect and flawed creature. Considering his evolution, it is hardly likely that he could be otherwise. To foster his better traits and to curb his worse by cultural means alone has always been, while clearly not impossible, in many instances most difficult. It has been an Archimedian attempt to move the world, but with short arm of a lever. We now glimpse another route—the chance to ease the internal strains and heal the internal flaws directly, to carry on and consciously perfect far beyond our present vision this remarkable product of two billion years of evolution.

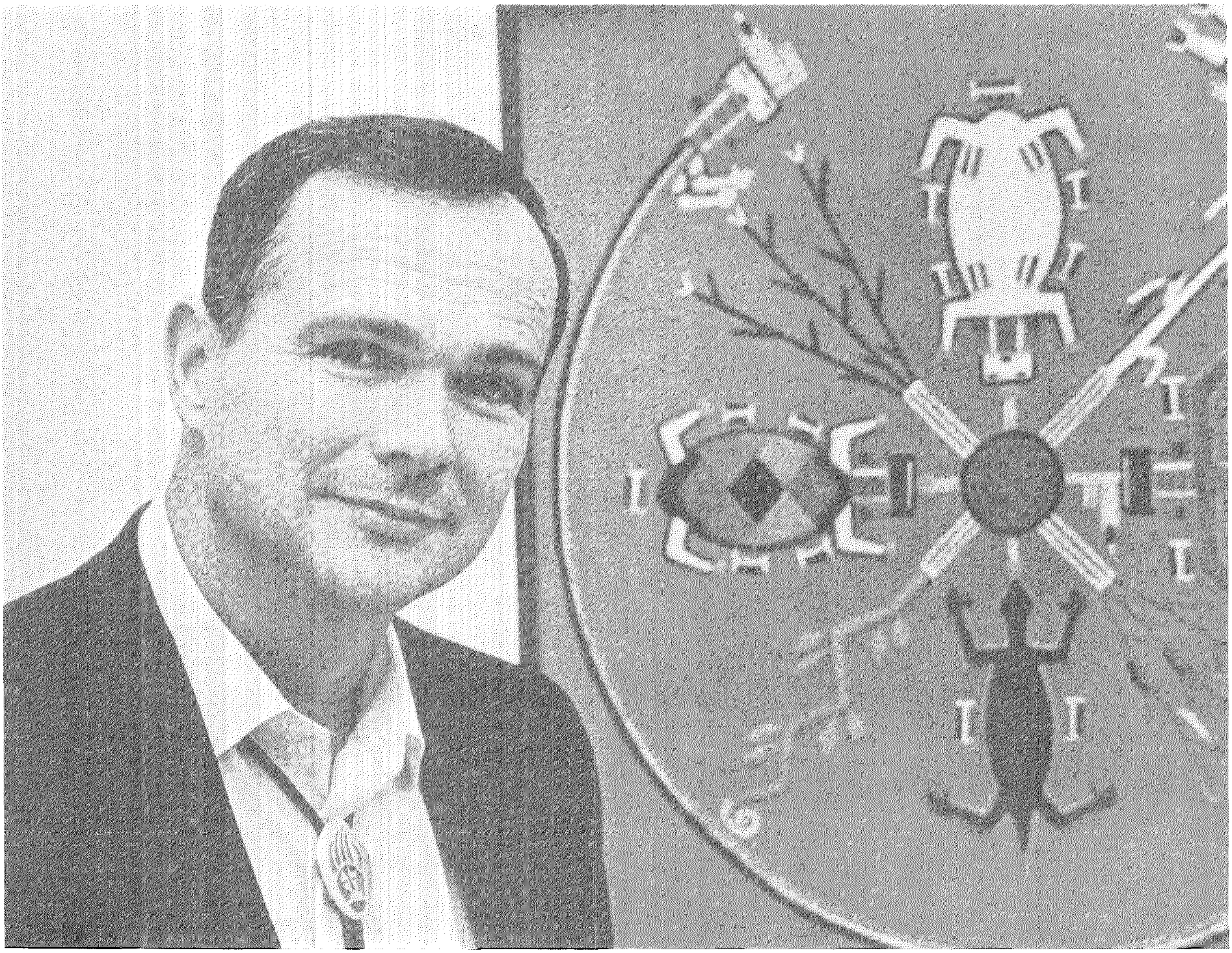
I know there are those who find this concept and this prospect repugnant—who fear, with reason, that we may unleash forces beyond human scale and who recoil from this responsibility. I would suggest to them that they do not see our present situation whole. They are not among the losers in that chromosomal lottery that so firmly channels our human destinies. This response does not come from the 250,000 children born each year in this country with structural or functional defects, of which an estimated 80 percent involve a genetic component. And this figure counts only those with gross evident defects outside those ranges we choose to call natural. It does not include the 50,000,000 "normal" Americans with an IQ of less than 90.

We are among those who were favored in the chromosomal lottery, and, in the nature of things, it will be our very conscious choice whether as a species we will continue to accept the innumerable, individual tragedies inherent in the outcome of this mindless, age-old throw of dice, or instead will shoulder the responsibility for intelligent genetic intervention.

As we enlarge man's freedom, we diminish his constraints and that which he must accept as given. Equality of opportunity is a noble aim given the currently inescapable genetic diversity of man. But what does equality of opportunity mean to the child born with an IQ of 50?

The application of knowledge requires technology, but the impact of knowledge can precede its application. Knowledge brings understanding, and the consequences of understanding can overflow the mind into the heart. It may be that in the near future the most important consequence of our new knowledge of ourselves will be a new sense of the power and responsibility—of the pivotal role—of man in this universe. Copernicus and Darwin demoted man from his bright glory at the focal point of the universe to be merely the current head of the animal line on an insignificant planet. In the mirror of our newer knowledge we can begin to see that in truth we are far more than another ephemeral form in the chain of evolution. Rather we are an historic innovation. We can be the agent of transition to a wholly new path of evolution. This is a cosmic event.

Eugene Shoemaker:



Down to Earth

Eugene Shoemaker, after ten years' work on the United States lunar program, is looking to the time when he can settle down to some important earthly concerns—in particular the chairmanship of Caltech's division of geological sciences.

Shoemaker accepted the job more than a year ago, but it wasn't until January of this year that he found time to move onto the Caltech campus—and even now, after almost five months, the chair behind the chairman's desk is often empty.

Shoemaker is a member of the Lunar Planetary Missions Board and of the Lunar Sample Analysis Planning team. In addition, he heads the Apollo Lunar Geology Field Experiment team of ten scientists, and expects to be spending considerable time on this for the next year.

"I thought I had everything timed so that the manned lunar landing would have been accomplished before I came, full time, to Caltech," he says. "Then the Apollo fire tragedy at Cape Kennedy set us back a good year, so here I am—my first year on campus and the lunar landing still downstream. I'm going to have to play it by ear for a while."

With his particular geological background, Eugene Shoemaker's playing it by ear is almost certain to produce a successful performance. He and geology found each other when he was seven and he started studying rocks and minerals. His father knew just enough about them to satisfy his son's earliest interest and to whet it to the point where Gene was collecting seriously by the time he was eight.

During his early school years the family lived in Buffalo, New York, where the Museum of Science was pioneering a program in science education for youngsters. "I bicycled down on Saturdays and weekday evenings," he says, "and took a whole slug of classes in everything from aquatic biology to geology and mineralogy—so by the time I was ready for high school I knew I wanted to be a geologist."

The family was living in Los Angeles at the outset of World War II. His mother, known in education circles as an able, dedicated teacher, was at the Rosewood Elementary School. (She just retired last year.) His father was working as a "grip" in a movie studio. A man of unusual ability and versatility, he had wanted to go to college enough to put himself through by playing football. He was later, in turn, a teacher, farmer, businessman, and politician. ("He always had to prove himself. He'd see something and wonder whether he could do it or not, then take it on and do it. It would take him about a year to satisfy himself that he could, and then he'd go on to something else.")

"My father always talked up Caltech to me," Shoemaker says. "In those days around here, it was considered the *only* place to go if you were science-minded."

Shoemaker was 16 when he entered Caltech in the fall of 1944. He found the geology department practically defunct. Most of its faculty were off on strategic metals programs or other war work, and its chairman, John P. Buwalda, and Chester Stock were just maintaining a limited number of courses. So Shoemaker met the requirements in everything else until his senior year. Then, when the staff returned at the end of the war, he took nothing but straight geology.

In that era, colleges were one big ebb and flow of students going off into—or returning from—service. Shoemaker was too young to be drafted, and, as a civilian, could also take advantage of the accelerated program given the V12 students being trained for the Navy. He was graduated in three years, in the spring of 1947. Even at this smart clip, though, he found time to be head yell leader, a *Big T* editor, glee club member, and student secretary of the YMCA.

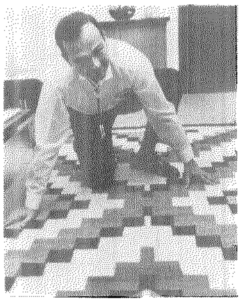
Shoemaker stayed at Caltech for a Master's degree, then got a job offer from the U.S. Geological Survey to work on the uranium exploration program in Grand Junction, Colorado. He was ready to get away from school for a while; he had had a pretty concentrated dose of it. He was still not old enough to vote.

The Colorado Plateau at that time was one of the most exciting places in the world for a geologist to be. The exploration and discovery of uranium there produced the only full-fledged mining boom we have had in the United States in the 20th century—and the most intensive geological effort that had ever been applied in one area in mineral exploration.

Shoemaker was caught up in the excitement of mineral exploration and the unfolding of new geological techniques that were being applied in the uranium program. When his project was temporarily recessed, he applied to Princeton and spent a year there working for his PhD, then came back for another summer in Colorado. He was married that summer to Carolyn Spellmann, the sister of his Caltech roommate, and they were packed to go back to Princeton when he was offered a chance to develop a new research project for the Survey. They unpacked—and for two years Shoemaker worked on the geochemistry of the Colorado Plateau uranium ores and their regional geochemical environment.

Shoemaker finally went back to Princeton with the bulk of the field work done on the problem that he planned to use for a thesis. But the year went by, and he was back in Colorado with the Survey again without having finished it. In fact, *several* years went by—and no thesis.

"It got so I'd see my professors from Princeton at some meeting," says Shoemaker, "and they'd say, 'You know we liked that last paper you wrote; it would make a good thesis.' So finally I asked the department chairman, Harry Hess, how he'd like to have a thesis on Meteor Crater, because that was at the top of my pile at the time, and he said, 'Fine, we'll take it.' And so about ten years after I started at Princeton I finally got my PhD."



Eugene Shoemaker moves into a new office—and a new job—on the Caltech campus

Before joining Caltech this year, Shoemaker was chief scientist of the U.S. Geological Survey Center of Astrogeology at Flagstaff, Arizona. He is still a consultant to the Center, which grew out of the USGS Branch of Astrogeology, which he organized in 1961. He was an investigator for the television camera experiment on the Ranger spacecraft series that took closeup pictures of the moon in 1964 and 1965. And he was principal investigator for the Surveyor spacecraft television camera from 1963 to 1968.

Shoemaker's interest in the moon goes back to the days when it had to be a pretty clandestine affair.

"I got to thinking about the state of rocket development and I figured that, all other things being favorable, it was likely there would be manned expeditions to the moon during my professional career," he says. But he kept fairly quiet about it because sensible people weren't talking about going to the moon then.

He started reading about the moon, though, which led him to study the violently eruptive volcanos of the Navajo and Hopi country. His interest in these volcanos led him to the problems of underground nuclear explosions—which got him into the problems of impact craters. His comparison of nuclear craters with Meteor Crater in Arizona and his subsequent study of impact and shock propagation mechanisms worked directly into an interpretation and understanding of the moon's surface.

His careful study of the best available telescopic photographs of the moon led Shoemaker to the conclusion that its outer part is stratified, and that the distribution of the exposed strata could be mapped. The strata consist primarily of layers of ejecta from large craters. Other deposits of possible volcanic origin appeared to be interlayered with the ejecta. He initiated an extensive program of geological moon mapping, which has been carried on by his colleagues at the U.S. Geological Survey. The primary purpose of this work is to determine the stratographic sequence on the moon in order to unravel the sequence of major events in lunar history. Geology, after all, is a historical science.

By 1962 Shoemaker's work had attracted so much attention that Caltech invited him to come back as a visiting professor. He accepted because it gave him a chance to attempt a synthesis of lunar research—particularly the new problems of understanding and interpreting impact structures on the moon and earth. He started a course he called astrogeology and, as he says, "It seemed to sell."

Caltech suggested he join the Institute on a permanent basis, but he was too much a part of the lunar program to leave it at that point.

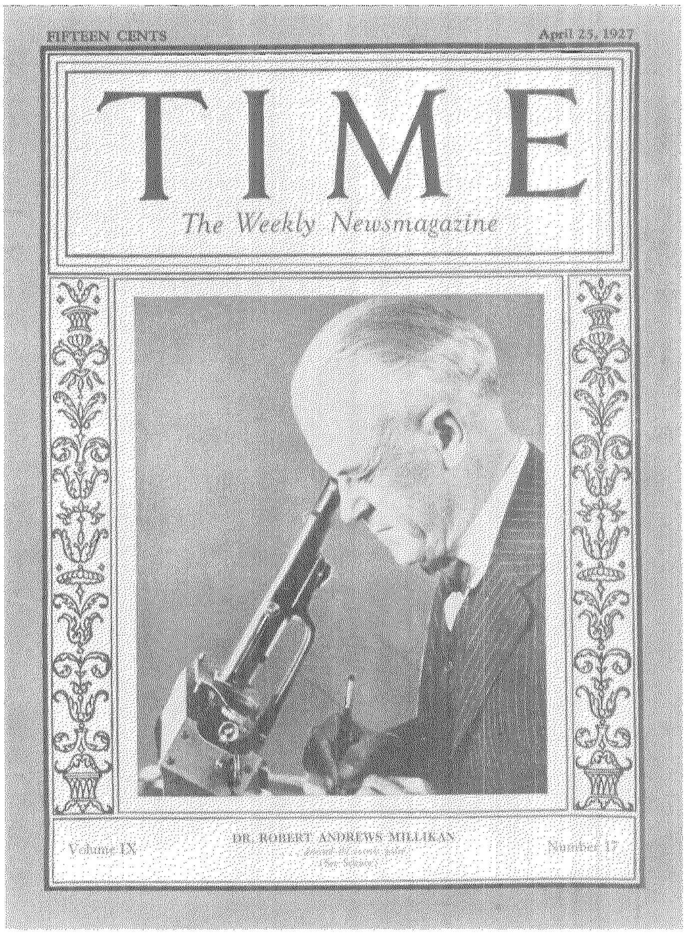
"*Now* I can finally begin to make the break," he says.

He flies every weekend to Flagstaff, and uses this time not only to work with colleagues on the Apollo project, but to see his family. His wife and children—Christine, 16; Patrick, 13; and Linda, 12—will call Flagstaff home until they move to Pasadena in September. On these flights he looks down on one of the geological mysteries he hopes to have a hand in solving: the Basin Range Province, a region of linear mountains and valleys, from about 500 to 1,000 miles across, that lies between the Rocky Mountains and the Colorado Plateau on the east and the Sierra Nevada on the west.

"It's a very unusual part of the earth," he says, "where we have an oceanic rise intersecting the edge of the continent." He believes the solution to its origin is part of the larger set of questions raised by geology's new excitement—global tectonics, the investigation of the forces producing movements of the earth's crust.

"Global tectonics has brought—for the first time—geophysicists and geologists together in an intense dialogue with each other. Part of the story will be worked out by seismology, part by studying the magnetization of the rocks, part by ocean bottom geology, and part by classical techniques of geology on continental parts of the crust. The new global tectonics brings together a whole range of disciplines, and it cuts across discipline lines."

As he anticipates his career reentry into the earth's atmosphere, he suspects he may have a little bit of his father in him. "After a period of time I like to do something different. Now, I'm anxious to put some new irons in the fire down here on earth."



MILLIKAN:
Spokesman for Science in the Twenties

by DANIEL J. KEVLES

One week in 1927 Robert A. Millikan, distinguished physicist and head of the California Institute of Technology, beamed across the country from the cover of *Time*. The story inside gave executive Millikan the face of "a witty and successful banker." It quoted scientist Millikan's reassuring report: "I have never known a thinking man who did not believe in God." In the twenties, when much of the nation held bankers and the Deity in nearly equal reverence, America's second Nobel Prize physicist qualified quite ably as a public pundit of science.

It had been a long and eventful road from Maquoketa, Iowa, to the cover of *Time*. Millikan was a minister's son who got into science by accident. In high school he learned almost nothing about nature's laws. When at Oberlin his Greek professor asked him to teach a physics course in the college's preparatory department, Millikan modestly protested his ignorance. The professor replied: "Anyone who can do well in my Greek can teach physics." Somewhat pinched for funds, Millikan took the job.

Once Millikan started learning physics, he decided to make a career of it. He went on to do graduate work at Columbia, spent a summer at Chicago under Albert A. Michelson, later to become America's first Nobel Prize physicist. Awarded the PhD, he left for advanced study in Europe (his Columbia professor made the trip possible with an obliging loan of $300 at 7 percent). While in Germany Millikan heard of the discovery of x-rays and radioactivity. When a cable arrived with an offer from the

University of Chicago, he hocked his luggage and returned to the States, eager to make his mark in the new physics.

In Chicago, Millikan added a sharp spur to his ambition by falling in love with Greta Blanchard. Well-established Mr. Blanchard, successful manufacturer and elder in his church, considered his daughter's suitor "somewhat hazardous," as Millikan remarked, "because I was not a man of property and had little prospect of ever being such." By paternal insistence, Greta could not marry Robert until he was earning at least $1,500 a year.

Millikan threw his enormous energy into getting ahead. (He needed no more than six hours sleep and often managed a round of golf before morning class.) While frustrated in his research, he did publish a widely acclaimed textbook and develop the teaching side of the department. In 1902 father Blanchard blessed the marriage.

Greta

By 1906 Millikan wanted to do still better. He now had two children and a mortgage. Moreover, Mr. Blanchard's daughter, to whom he was wholly devoted, enjoyed the perquisites of gentility. While his pedagogical accomplishments had just won him an associate professorship, at Chicago the major rewards went for scholarship. Millikan was acutely aware of the controversies reverberating through his science. Eager to join the attack against the atom, he started concentrating on research. By 1910 he had emerged from the laboratory with a precise measure of the electronic charge. Triumphant, he won accolades from the world of science—and from the university a full professorship.

The Millikans prospered. Robert began learning how to administer science. He also started consulting on the development of the vacuum tube at AT&T, which had just established one of the country's first industrial research laboratories. American industry was beginning to recognize that investments in science could yield both dividends in technical progress and the protection of crucial patents. As a result, a mutually advantageous connection was developing between academic science and business. Millikan, the able Chicago physicist and administrator, was an early link in the chain.

When the First World War erupted, Millikan went to Washington as the Chief Executive Officer of the National Research Council. There he worked closely with George Ellery Hale and Arthur A. Noyes; both had a special interest in the Throop College of Technology, the small school in Pasadena from which Caltech would germinate. There also he got to know many of the dollar-a-year industrialists who had come to the capital to help run the defense effort. There Major Millikan of the U.S. Army learned how, as a friend put it, to "sell science" to a wide variety of people, military and civilian alike.

Robert

The war pointed up a highly effective sales argument. Airplanes, submarines, poison gas—all were revolutionizing the face of combat. At the same time, physics was detecting air fighters and U-boats, chemistry protecting against noxious attacks; in short, science was proving a defense against its own martial offspring. Moreover, the war was driving home the economic reasoning of firms like AT&T (if planes could carry bombs in war, they could transport passengers in peace). Salesman Millikan drew his conclusions. One way to drum for science was to stress its powerful utilitarian potential for both the nation's defense and its economy.

Millikan came out of the war eager to get back to atomic physics. In 1919 Ernest Rutherford, the British Nobel Laureate, made the mysteries of the atom all the more tantalizing by reporting an experiment in which nuclear mass had been transformed into energy. But to press the attack on the atom would take money. Millikan returned to Chicago insisting on far more funds for research. Despite his threat to accept an offer from Throop, the university, squeezed in the burst of postwar inflation, refused his demands. Millikan left for Pasadena, where he had been promised a munificence for physics. ("Just imagine," a German scientist goggled, "Millikan is said to have a hundred thousand dollars *a year* for his researches!")

Millikan arrived as full-time head of the newly named California Institute of Technology in 1921. He brought scientific acumen and zeal to the post. Both these traits, fused with Hale's vision, Noyes's wisdom, and all that money, made Caltech virtually an overnight success. As chairman of the executive council—he preferred the title to president even though, it is said, he ran the Institute autocratically—Millikan found himself standing on an increasingly prestigious institutional platform. The award of the Nobel Prize in 1923 added to his public clout.

Salesman Millikan made effective use of it over the decade. Privately, he raised money for Caltech (so persuasively that executives at the Rockefeller Foundation would virtually lock the cash box when he came around). Publicly, he spoke for science in general. With the country in the clutches of isolationism, science for defense had become an untimely argument. But in the twenties, Americans were eager to hear about science for science, science for God, and science for industry.

Science had never enjoyed such wide publicity in the United States. Einstein paid his first visit to America in 1921 and charmed newspaper readers all over the country. Year after year, the merest utterance of the wild-haired, absentminded genius of relativity found its way onto the front pages. Einstein's idiosyncracies aside, his ideas fascinated the public. Arthur S. Eddington's book-length expositions of relativity sold well through the decade.

The flow of scientific news was unprecedented. Major newspapers all over the country hired science editors. Mass-circulation magazines carried stories on the most abstruse developments and glossed them with technological promise (harness the energy in a glass of water and you could power a steamship clear across the Atlantic). The American Association for the Advancement of Science happily contributed to the stream of news by inaugurating symposia for the press. Science Service, created in 1921, sent out authoritative copy.

Apart from the publicists, science gained enormous prestige from its identification with enormously prestigious business. The war had ratified industry's commitment to research. During the twenties, radio and rayon, along with all the other gadgets taking their place in the pantheon of American technology, supported the utilitarian argument for science spectacularly. In the era of Warren Harding's normalcy, business was good for America, science good for business, and, completing the chain, science good for the nation's prosperity.

Friendly journalists added to the Chamber of Commerce image by letting the public know that not every scientist was offbeat like Einstein. Scientific conventioneers, a reporter wrote, were "as clean-shaven, as youthful, and as jazzy as a foregathering of Rotarians." Today's scientist, he elaborated, "is fully as much a man of the world as his brother, the businessman." Hadn't his research given America the automobile, the radio, even the saxophone? In this reporter's opinion, scientists had in fact made the jazz age possible.

Millikan believed in science, in God,
in private enterprise,
and in all with equal faith.

In the twenties, the scientist as giver of good things spoke with high authority. The new advertising industry, already a litmus paper of American tastes, understood the potential. There was the squib for Palmolive soap: "The blend of palm and olive oils has produced the mildest cleanser science can produce;" and the pitch for Pebeco toothpaste which quoted the well-considered opinion of an "Eminent scientist: 'Use a dentifrice that polishes without scratching and one which mildly stimulates the salivary glands.' " The *Nation* remarked, "A sentence which begins with 'Science says' will generally be found to settle any argument in a social gathering or sell any article from tooth-paste to refrigerators."

Many Americans were particularly eager to hear what science had to say about God and religion. The decade swirled with defensive religiosity. In the post-Civil War years Thomas Huxley had hurled Darwinism against the certitudes of the Bible. Now science was once again assaulting religious and moral pieties. At the carnival in Dayton, Tennessee, Clarence Darrow, counsel for young John Scopes, ridiculed William Jennings Bryan's fundamentalist conviction that the Lord had literally created the universe in six days of labor. Behavioral psychol-

Next to Albert Einstein, Robert Millikan was the most famous scientist of the twenties.

ogists worried millions who were scarcely fundamentalist with the assertion that you could not find a soul in a test tube. College students, taking Freud as their text, scoffed at traditional standards of virtue. The Reverend Harry Emerson Fosdick could say: "When a prominent scientist comes out strongly for religion, all the churches thank Heaven and take courage as though it were the highest possible compliment to God to have Eddington believe in Him."

Millikan believed—in science, in God, in private enterprise, and in all with equal faith. Americans of the twenties could happily respond to his authoritative voice.

Religiously troubled people could find comfort in the way that Millikan the physicist touted the intellectual adventure of research. He extolled the rich harvest of ideas emerging from the study of matter. But God's universe, he assured, defied complete understanding. Moreover, the revolution of relativity and quanta had stripped science of certainty and taught the physicist "a wholesome lesson of humility." In Millikan's somewhat labored extrapolation, no scientist who admitted the tortuous complexities of the atom could assail religious truths with the insistence of his 19th-century predecessors.

For Millikan the minister's son, science without religion could be a curse to mankind. In fact, he insisted: *"The most important thing in the world* *is the reality of moral and spiritual values.*" At the same time, religion benefited from the open-minded tolerance of the scientific spirit. Churches without it had fostered "dogmatism, bigotry, persecution, religious wars, and all the other disasters which in the past have been heaped upon mankind in the name of religion."

No contest of science and religion concerned Americans of the twenties more than the battles symbolized by the Scopes trial. On that issue Millikan was no adamantine atheist like Thomas Huxley a half century before. A reconciler, he repeatedly testified to the "complete lack of antagonism between the fields of science and religion." Why, the dozen leading scientists of America, Millikan exclaimed, saw absolutely no conflict between the two. More important, most of them were willing to line up in support of a higher being, and Millikan had testimonials to prove it.

No less an apostle of business than of God, Millikan made science an ally of the economy of normalcy. The world's economic problems, he said more than once, could not be solved by government intervention. Caltech itself exemplified the effectiveness of relying on private enterprise instead of the state. The resolution of economic want lay in more abundant production by more abundant industry. "No efforts toward social readjustments or toward the redistribution of wealth," he asserted, "have one-thousandth as large a chance of contributing to human well-being as have the efforts of the physicist, the chemist, and the biologist toward the better understanding and the better control of nature." New science led to new technology, to new industry, to new and higher paying jobs. Not revolution, but research, Millikan insisted, was the best bet for American labor.

Millikan's expositions of science, his testimonials to God, his sonorous accolades to private enterprise —all contributed to win him a wide public in the twenties. Apart from Einstein, he was the most famous scientist of the decade. But fame also won Millikan his critics. Some scientists considered him a platitudinous bore; others, sneering at his emphasis on utility, a desecration of the temple of pure science. Most scoffed when on occasion his religious convictions interfered with his physics. Einstein, a confirmed agnostic, reportedly once said of Millikan's views on cosmic rays: "He's not dishonest, just ignorant."

His public critics were not quite so generous. Today, one remarked, a Millikan "sits in the seats of the mighty. He is the president of great universities, the chairman of semi-official governmental councils, the trusted adviser of states and even of corporations." With responsibilities like these, the Millikans owed a greater loyalty to civilization than to science. But Millikan himself, this critic asserted, had discarded the salutary inconoclasm of a Huxley for the custodianship of the status quo.

Not all the critics matched Millikan against Huxley. Many were humanists who, like Huxley's enemies a half century before, considered science dominant a threat to the balance and texture of civilized society.

The most penetrating of Millikan's humanist critics was Christian Gauss of Princeton. Gauss wholly admired the Nobel Prize winner for his triumphs in physics. Professor of modern languages and one-time president of the Dante League of America, he disagreed quite emphatically with the social pundit's "confidence in the future of our civilization under science." Gauss chastized Millikan for arguing that morality progressed with the progress of research. Science merely described nature; it could not—and did not—speak to timeless questions of values. Surely one would not want to discard the teachings of Christ and Confucius because they were "hopelessly unscientific." Surely while modern man knew more than Socrates, he was demonstrably neither wiser nor more decent. Industry wrote checks against the sciences, but the sciences did not in turn check the rapacious industrialist. And if science had beneficently enlarged peaceful man's mastery over nature, it had multiplied warlike man's power to kill and destroy.

Gauss articulated what a good many Americans outside the academy apparently felt in a more marrow-of-the-bones way. When in 1927 an English bishop proposed a ten-year moratorium on research to allow civilization time to cope with its creations, he provoked a widespread stir in the United States. As the *Chicago Evening Post* explained its sympathetic response: "Science has been leading us rather a giddy chase for the last two or three decades."

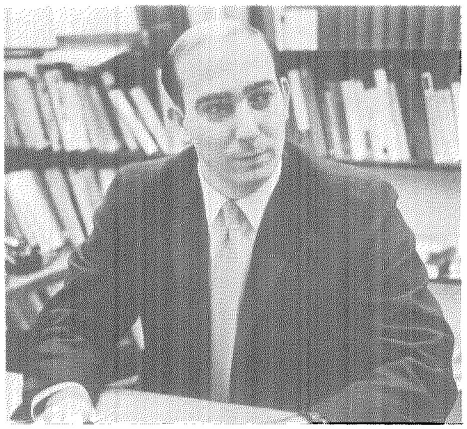
In the context of the criticism, Millikan emerged not only as exponent but as staunch defender of science. With good sense, he attacked the bishop's proposal as "impossible and foolish." With something less than tolerance, he charged the dissidents with being misguided completely. Had the machine, instead of liberating civilization, enslaved it? In Millikan's opinion, the automobile had not smothered cities in exhaust fumes and congestion. It had created a "new race of men." "Contrast the clear-eyed, sober, skillful, intelligent-looking taxi driver of today with the red-nosed wreck of a human being who used to be the London cabby a quarter of a century ago . . ." For Millikan, a teetotaler, the new London cabby proved irrefutably how "responsibility and power" born of the machine could alter human nature.

Millikan found scarcely a sin to be credited to science. "It is literature and art," he insisted, ". . . which have been the prey of those influences through which the chief menace to our civilization comes . . . Today literature is infested here and there with unbridled license, with emotional, destructive, over-sexed, neurotic influences. . ." The bishop need not worry about science, or even about the absurd possibility that mankind, armed with the energy of the atom, might blow itself to kingdom come. That energy, Millikan predicted, was destined to stay locked in the atom. Most scientists agreed, but Millikan's way of assurance was characteristically his own. "The Creator has put some foolproof elements into his handiwork and . . . man is powerless to do it any titanic damage anyway."

In the long run, Millikan was of course wrong about that. In the short run, after 1929, to much of the public he appeared wrong about a lot of other things. An adamant opponent of the New Deal, he kept on touting the economic boon of science and private enterprise, kept on attacking plans to redistribute wealth through government action, kept on preaching the importance of spiritual values to a people anxious about unemployment, economic collapse, and dictatorships abroad. Americans of the thirties still respected Millikan the scientist. They ignored, indeed some ridiculed, Millikan the social and economic pundit.

Amid the brutality of the Depression, Millikan's social vision of science was considered irrelevant at best. New Dealers preferred to use the scientific method as a weapon of reform. They wrestled with the economic role of science so as to save private enterprise by learning how to eliminate its inequities. They ignored the reconciliation of science and religion and concentrated on assuring Americans a chance to face God on a full stomach. The public rationale of science in one era does not necessarily fit the urgencies of the next. So scientists of the fifties are discovering today. So Millikan failed to recognize after 1929. By clinging tenaciously to the orthodoxies of the twenties, in the thirties he found himself publicly beside the point.

But in the twenties, Millikan, prickly toward state aid even for science, did enjoy a luminescent hour. Your industries, he told the New York State Chamber of Commerce in 1928, are the "offspring of pure science. If you believe in private initiative, you will keep pure science going strong in the universities . . . and applied science going strong in the private industrial laboratories." In the era of normalcy, Robert A. Millikan, widely respected physicist, sage of morals and religion, apostle of business, could speak for science very well indeed.



DANIEL J. KEVLES, associate professor of history at Caltech whose specialty is the development of science in the United States, is also a knowledgeable student of the career of Robert A. Millikan, Nobel Laureate in physics and head of the Institute from 1921 to 1946. In 1966 Dr. Kevles supervised the organization and cataloguing of Dr. Millikan's personal papers for the Caltech Archives. He has also explored this collection for his own book, a social and political history of physics in modern America, which will be published by Knopf. To gather information for his study, Kevles, who first majored in physics as an undergraduate at Princeton and then took his PhD in history there, recently spent a year in Washington, D.C., as an Old Dominion Fellow of Caltech, doing research in the National Archives and the Library of Congress. "Millikan: Spokesman for Science in the Twenties" has been adapted from a talk given at Caltech's 1968 Alumni Seminar.

CONFESSIONS OF A GENIAL ABBOT—III

By ROBERT A. HUTTENBACK

It has always been a source of wonder to me that some Caltech students can become terribly exercised over the number of times tunaburgers are served to them, or the hardness of the pillows on their beds, but have generally been unconcerned about the bomb, war, or the state of society. In 1961 they were not yet prepared to enter the arena of political and social action. They were still addicted to the complex practical joke.

I remember one night standing out by the parking lot next to the Keck engineering building and watching a long line of cars dutifully following a carefully marked, tortuous course. It turned out that some boys in Page House had placed detour signs on Del Mar Boulevard, routing the traffic on one of Pasadena's major arteries through the dark parking area. When a police car inadvertently got caught in the maze, the jig was up.

One memorable incident from those years began with an irate phone call to Dr. DuBridge from the latest husband of a famous movie star. The luminary's daughter, the gentleman averred, had been rendered pregnant by a Caltech student. I called the expectant father to my office. Had he perpetrated the awful deed? He responded that it seemed quite likely. He was in the habit of foregoing many of his afternoon physical education periods, and the young lady, knowing this, would make her escape from a local girls' school and seduce him in his room. I asked him why he had taken no precautions, to which he answered that he had volunteered as a sperm donor for the UCLA medical school and they had informed him that he was sterile. He went on to explain that neither he nor the girl wanted to get married, but a few days previously he had been confronted by the girl's mother, several daddies, the girl's psychiatrist, and the family lawyer—all of whom urged him to make an honorable woman of the unfortunate young lady. He had demurred, but that night had phoned his mother and confessed all She moved the lad to tears by pointing out that the expected baby would be his father's grandchild—a rather obvious conclusion but one the boy had somehow overlooked. Apparently it was a telling argument, for he immediately determined to marry the girl after all. Too late—the next morning's paper announced her wedding to someone else. Perhaps he was sterile after all!

Be that as it may, we were still faced with a sticky disciplinary problem. Technically the boy had violated no house rule. He had entertained his lady friend well within the legal hours. (On the other hand, we had no specific rules against murder and arson; it was understood that they did not constitute proper conduct.) We finally arrived at a rather Solomon-like decision. As the young man had really been the passive partner and as the flesh is weak, we limited ourselves to asking him to move off campus where he might pursue his interests undisturbed. The last time I saw him, he had just lost a considerable amount of money to a man whom he had discovered playing a game involving three walnut shells and a pea! Not all Caltech students are geniuses.

In 1963-64 a disturbing percentage of the freshman class determined to leave Caltech and pursue their studies elsewhere. The actual number was really not much higher than in previous years, but the quality of those intending to depart was. They were among our best students, and what was particularly disquieting was that most of them were not disenchanted with science but with the Institute. I asked some of these boys to put their thoughts into writing, and the results were revealing. One student with a grade point average of 3.7 wrote:

> Among the greatest virtues of the Institute is the personal freedom its students enjoy. This freedom implies a philosophy of education that encourages independence and responsibility, that shuns restrictions on the life of an individual. It is highly ironic that at an educational institution characterized by such a philosophy the students' lives should be narrowly restricted, that their primary interests and aspirations should conform so closely to a single pattern . . .
>
> Two factors contribute to the deficiencies I have just suggested. The first of these is the limited range of the students. No humanities majors are available for discussions on campus . . . Nevertheless the atmosphere is not scientifically enthusiastic nor is it, in general, scientifically stimulating . . . Concern with science, when it exists, is suppressed by a self-defensive cynicism. At the same time, an indifference to the humanities, politics, religion, and philosophy is present . . .
>
> Compounding the tendency toward intellectual indifference is the volume and intensity of work required by the Institute . . . I am not bitter about the work here; I am, however, bitter about the sacrifice of a full educational experience and the sacrifice of an intellectual spirit that has been made at the altar of work . . .

The effect of this statement and others like it was sufficiently disturbing to the faculty that a committee on the freshman year was appointed. Out of their many months of deliberation some important changes arose. At the same time the newly appointed and forward-looking provost, Robert Bacher, took advantage of the presence of Carl Rogers—the father of non-directive, client-centered therapy—in southern California to appoint him an educational consultant to the Institute. A number of the faculty met with Rogers almost every month for the best part of two years, and these meetings provided a significant catalyst for change at the California Institute of Technology.

We are blessed annually with the most talented group of entering students in the country.

To start with, the division of humanities formulated a program which offered freshmen some choice of courses in their first year. This moderate reform led to a proposal to offer undergraduate majors in nonscientific fields in which we were well staffed, i.e., history, English, and economics. It was argued that we would thus keep at the Institute some attractive boys who might otherwise leave. It was also argued that, given the amount of required science, the Institute might produce a truly unique product—a humanist or social scientist with a high level of sophistication in the sciences. Eventually the faculty and trustees approved these majors.

The committee on the freshman year was also wrestling with how to make the first year at the Institute less traumatic. Though no one considered it a panacea, there was general enthusiasm among the committee members for the concept of giving freshman grades of "Pass" or "Fail" only. After endless discussions, the scheme was voted into operation for a two-year experimental period. It turned out to be a great success and is now a permanent part of the Caltech system.

But are they really being educated and turned into creative human beings?

To understand the level of the responsibility of the trustees, faculty, and administration of the Institute for the welfare of students, it must be remembered that we are blessed annually with the most talented group of entering students in the country. In a sense they are unspoilable, and faculty bosoms swell with pride as they progress through the BS and to a PhD. But are they really being educated and turned into creative human beings? Would they have been more creative and exciting had they undertaken their studies elsewhere? That the California Institute offers an excellent formal education seems incontrovertible. But what about the area of informal education? Is the sacrifice the undergraduate makes to attend the Institute worth it? Admittedly these are imponderables, but they are worthy of deep thought.

What will the future hold? I am inclined to think that the answer to many of the Institute's problems must lie in increased diversification. An active committee on aims and goals has come into operation. The question of the admission of members of disadvantaged communities has gripped the faculty's attention, and an *ad hoc* committee presented a report to the president which resulted in a unanimous resolution by the admissions committee that an additional full-time admissions officer be appointed to deal with the problem.

Urged on by students, the faculty also grappled with the notion of admitting women undergraduates. The usual arguments were trotted out—that a Caltech education would be wasted on most women, who would only get married, raise children, and never make use of their special training; or, conversely, that it was positively medieval to discriminate against women. To its credit, the faculty voted overwhelmingly to urge the trustees to admit women to the undergraduate school with "all deliberate speed." The trustees eventually voted to admit girl undergraduates as freshmen and transfer students in the fall of 1970.

The public is probably willing to admit that students drink (providing they do so discreetly); it is not willing to make the same concession in regard to marijuana. And Caltech shares a drug problem with every other college in the country. Until early 1967, the Institute was inclined to draw a veil over the whole question. But then the editor of the *California Tech* determined to publish an exposé of drug usage on the campus. He claimed to know that almost 30 percent of the undergraduates used marijuana and to have access to figures on the use of LSD. Before he published his story, however, he came to ask my advice. I pointed out that he hardly had very reliable evidence and that we were all aware of drug usage on the campus. I thought that in view of the fact that the newspaper received extensive attention beyond the confines of the campus it would be wiser not to publish the article. But I was unequivocal in declaring that it was not my decision but his in his capacity as editor of the paper. In retrospect I rather think the lad wanted to be forbidden to publish the story, for, after seeing me, he visited the director of publications, the dean of students, and finally the president—all of whom gave the young man much the same answer to his inquiries.

The article was published eventually, and a mild form of hell broke loose. The student government seized all copies of the *Tech*, and a recall campaign was mounted against the editor. Happily it failed. Through its board of directors the student body did establish a policy of greater control over the paper.

The administration was in something of a quandary. In the past we had tended to deal with drugs on an *ad hoc* and strictly *sub rosa* basis. Now, regardless of the accuracy of the article, it was publicly known that Caltech students smoked pot. It was decided to try to determine accurately what the situation was. A questionnaire was prepared, and the students were asked to complete it. Absolute anonymity was assured, and 90 percent of the combined graduate and undergraduate student body responded. The final tabulation of the results indicated that 86.3 percent of the entire student body had never used marijuana; 5 percent had tried it one or two times; and 8.7 percent had used it on three or more occasions. The undergraduate on-campus usage was less than half of the off-campus figure. Almost 91 percent of the undergraduates and 98.1 percent of the graduates had never used LSD. And even these figures, I would think, have now been reduced after the publicity concerning the adverse effects of LSD on the mind—the Caltech student's most valued asset.

The results of the survey prompted the president to appoint a faculty-student committee, ably chaired by the Institute psychologist, Dr. Kenneth Eells, to recommend an Institute policy on the use of drugs. The committee attempted to face the problem realistically and sympathetically, and the final report, *Drugs and the Caltech Student,* emphasized education rather than draconian retribution. That it was officially adopted by the board of trustees is a tribute to the good sense and patience of the committee, the president of the Institute, and of the trustees themselves.

I am inclined to think that the answer to many of the Institute's problems must lie in increased diversification.

In 1967 a most remarkable student was elected president of the student body. Joe Rhodes had emerged from a slum high school in Pittsburgh and had been admitted not only to Caltech but to MIT, Harvard, and the Juilliard School of Music (on a violin scholarship). Joe had real charisma and the ability to lead and influence others without being aggressive or particularly self-assertive. To make it possible for Joe to be their leader while he was only a sophomore, the Associated Students had to amend their constitution. This they did, and, having cleared the decks, they elected him president by an overwhelming margin. He was not only the first sophomore to become student body president, but he was also the first Negro in the history of Caltech to hold that office.

Joe immediately called a meeting of the entire student body, and faculty members were also invited. The assembled students passed several resolutions calling for reforms such as the presence of students on faculty committees. So discreetly did Joe and his colleagues handle this affair that the faculty welcomed their proposals, and no ill-feeling or any sort of confrontation developed.

During this period Joe and some of his friends were experiencing the kind of disillusionment and restless exasperation so common to modern American youth. They began to doubt the relevance of what they were learning and decided that they were unwilling, as the modern cliche puts it, "to postpone gratification." Out of their concern emerged the most responsible and innovative expression of student disenchantment and disaffection to be seen on any campus in the United States. Led by Joe, the Associated Students determined to undertake a research project on air pollution and to invite students from other campuses, both boys and girls, to work with them.

The faculty and administration of the Institute did not welcome the proposal with open arms. There was a tendency to judge the idea purely as a research proposal, when really it was both more and less than that. Happily, the faculty at least tolerated the plan, and, much to everyone's surprise, Joe was able to raise $70,000 from the Department of Health, Education and Welfare to finance the project. The Institute added about $30,000 in waiving overhead costs.

Throughout the summer of 1968, Caltech students and those from other campuses worked on the project, and, with an additional $50,000 that Joe gleaned from the Ford Foundation, some of the boys and girls have continued into the school year of 1968-69. Although the trustees had voted to bring girls to the Caltech campus in September 1970, the students had, in effect, already accomplished the task. Whether the research project makes significant technical contributions to solving the problem of air pollution may or may not be important, but the program is a great success regardless. It has been a triumph of student-directed self-education and interaction.

I have tried to keep pace with Joe Rhodes and his colleagues. Through a Master's Fund my office has attempted to make students more critical of their environment and to expand their horizons. As a first step I sent one student from each house on a tour of several other campuses—Harvard, Yale, Bowdoin, Rice, Wesleyan, Swarthmore, Amherst, and Williams. The students were impressed with what they saw, and their report, *Reflections on Several Worlds*, is already in its second printing. The report and influence of these seven students have helped chip away at the fortress of Institute complacency.

The Master's Fund has been put to many other uses. My office has sponsored sensitivity conferences, art classes, speed-reading instruction, faculty-student dinners, and theater performances, and has reimbursed students who attended concerts, plays, and other forms of entertainment. It has been the single greatest asset I have had for the proper execution of my duties.

Progress is slowly being made in many facets of undergraduate education at the Institute. The demands placed on students in their first two years at Caltech have been considerably reduced, and a real measure of flexibility and free choice has been introduced. Initiation in its old negative form is almost dead. This year (1968-69) for the first time we have introduced graduate students into all of the student houses, and a married couple now has charge of Dabney House.

But much yet remains to be done. The homogeneity of Caltech must be broken down. Plans for the increased admission of the disadvantaged must be formulated and implemented. Various different kinds of housing schemes should be available. Probably an experimental college should be created to deal with subjects other than science and engineering. Caltech is no longer so far ahead of other schools in the level of its undergraduate science education—it no longer so uniquely fills the kind of need it once did—that it is fair to ask a singularly attractive group of young people to make the kind of sacrifice demanded of them when they enter the cloisters of the California Institute of Technology.

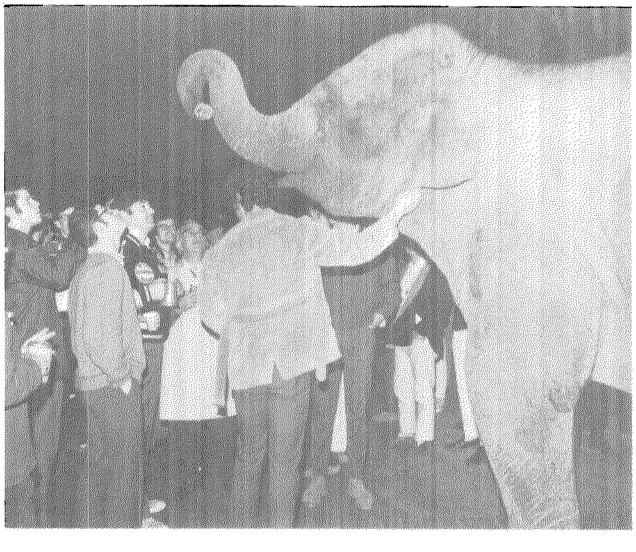
I hope my words do not offend. If they do, let me make it perfectly clear that I write in this vein not because I don't think Caltech is a wonderful institution but rather because I am very much a part of the place and want to see it grow and prosper more than it ever has in its already illustrious history.

I cherish a great number of memories of my years as Master of the Student Houses, but above all I remember with warmth and deep affection several generations of Caltech undergraduates with whom it has been a great joy to work. They made it all worthwhile.

Epilogue

Robert Huttenback got a rousing farewell in February as he set off on a six-month leave to do historical research in England and ended his 11-year career as Caltech's Master of Student Houses. He returns next fall as Dean of Students.

Hundreds of undergraduates and other Genial Abbot fans paraded around the campus with their former Master, three bagpipers, Mrs. Huttenback, and an elephant named Margie. On the steps of Millikan Library, amid a shower of balloons, speeches, and gifts, the marchers toasted the old master with tankards of ale, while Margie drank hers from the Millikan fountain.



RESEARCH NOTES

THE CHEMISTRY OF THE OCEANS

In the course of study to determine the impact of biological processes on the chemistry of the oceans, a Caltech scientist has found sea animals that can make the mineral fluorite (CaF_2). Heinz Lowenstam, professor of paleoecology, has established conclusively that at least three small animals —the opossum shrimp, deep sea snails, and a sea slug called a nudibranch—deposit fluorite, a crystalline compound, in their mineralized tissues.

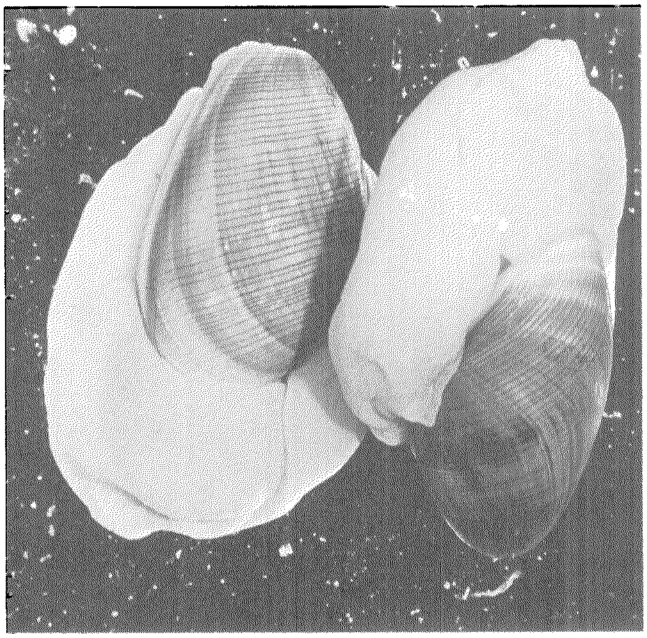
The shrimp extract fluorine from the sea to make tiny button-shaped stones (statoliths) that function as balancing organs, enabling the shrimp to sense direction. The animal sheds its exoskeleton, with the statolith, as many as 40 times in a lifetime. The opossum shrimp are a major food source for fish, whales, and other marine animals; and the statolith, which is not digested, is excreted to become part of the sediment on the ocean floor.

The molted and excreted statoliths are commonly ingested by bottom-feeding creatures that can digest some of the organic framework and excrete free crystals of fluorite. Dr. Lowenstam suggests that this process may account for much of the fluorite accumulation on the sea floor.

Observations also suggest that where huge populations of the shrimp exist, their large-scale fluorite precipitation seems to influence the fluorine content of the surrounding seawater during certain seasons. For many years geologists believed that fluorine-bearing sediments were derived only from weathering products of rocks on land and from submarine volcanos.

Dr. Lowenstam's work was conducted in cooperation with Duncan McConnell of Ohio State University. Specimens for their experiments were gathered in coastal waters of Denmark and near Puget Sound in Washington. The researchers also

Heinz A. Lowenstam, Caltech professor of paleoecology.



The deep sea snail is one of three marine animals in which deposits of fluorite have been discovered.

investigated deep sea snails and nudibranches. Fluorite was found in the gizzard plates of snails, collected in water from 180 to 200 feet deep near Denmark's Faeroe Island in the Norwegian Sea. Nudibranches were found to have a very high fluorine content—as much as 18 and 19 percent—in their spicules, small mineralized spines formed by the tissues.

The researchers used two methods—x-ray diffraction and electron probe analysis—to identify the presence of fluorite in the shrimp and snails. First they mechanically removed the statoliths and gizzard plates from the specimens. One portion was examined with the electron probe microanalyzer to determine the major chemical constituents; the other portion, finely ground into a powder, was studied for its x-ray diffraction patterns.

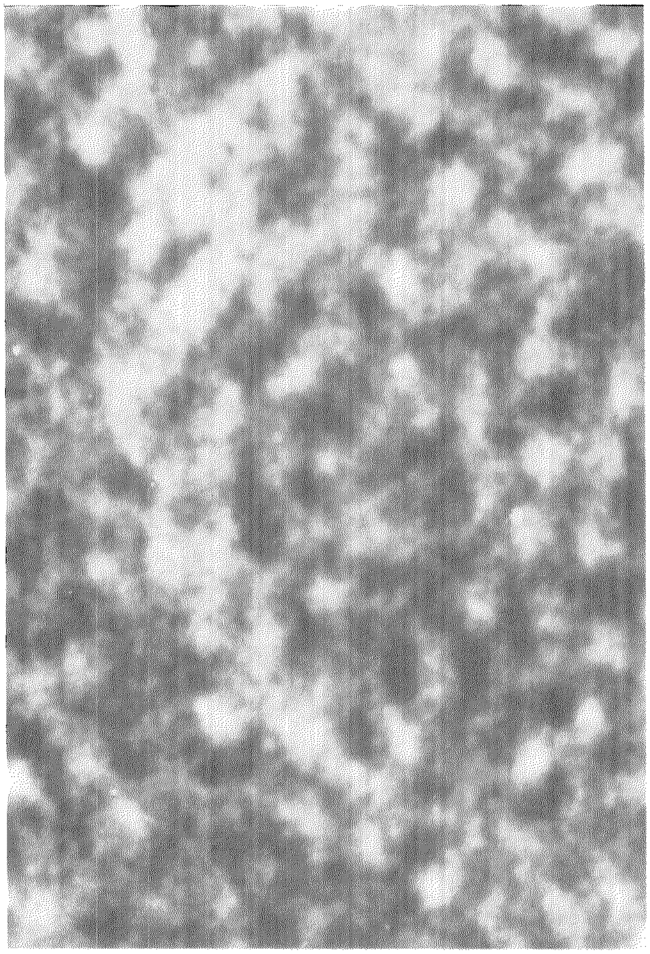
A RARE GLIMPSE INTO MOLECULAR STRUCTURE

An electron micrograph of a short length of a DNA molecule, recently presented by Caltech graduate student Jack Griffith as part of a scientific paper, has now been snapped up by the press and identified dramatically as "the first picture of the coiled structure of life's basic genetic material."

Griffith is unwilling to attach quite this much significance to the photograph. He will go so far as to agree that the micrograph *may* contain visual evidence of the 16-year-old theory of the double-stranded helical structure of DNA, proposed by Crick and Watson in 1953. The faint outlines of two intertwined strands can be discerned in his picture, which has been magnified 7,300,000 times. And when measurements were done on the micrograph, all the relevant dimensions were in good agreement with the known parameters of DNA.

Still, with characteristic scientific reluctance, Griffith describes his picture as "Exciting—yes! Beautiful—yes! Proof—no!"

"We cannot be certain," he says, "that the helical structure observed in this micrograph is real and not an artifact or a fortuitous structure that *looks* like the Watson-Crick structure. Such rare glimpses into molecular structure may be hints of statistically sound micrograph data to come, or they may be erroneous artifacts created by the machines that visualize them."



Electron micrograph of a portion of a DNA molecule, taken by Caltech graduate student Jack Griffith. The crescent-shaped helical structure which runs top to bottom, slightly left of center, may be the first visual evidence of the structure of DNA.

David Marsh, Caltech research fellow in chemistry.

ALLERGENS AND ALLERGOIDS

A Caltech chemist has found a way to take most of the side effects out of medication now used to relieve allergic reactions. Drugs now in use employ an allergen, a form of the same natural irritant found in pollens and other sources of plant and animal materials. Present medication employs the native allergen. When injected as medication, the allergen causes formation of a specific blocking antibody to neutralize the natural irritant. However, the amounts of native allergen which can be administered are severely limited by the risk of general allergic reaction.

Research fellow David Marsh has been able to alter the allergen to make what he calls an "allergoid"; by doing so he can almost eliminate the unwanted allergic reaction (by over 99.8 percent), as tested on human white blood cells, bronchial tubes, and skin. However, the desirable blocking antibody production, as measured in guinea pigs, is maintained at up to 60 percent of that of the allergen. Immunization of humans has yet to be performed.

To make allergoids, natural allergens, such as may be extracted from ragweed pollen, are incubated in a diluted form with formaldehyde and the amino acid lysine for 32 days at 90 degrees Fahrenheit. The resulting altered configuration is an allergoid.

It seems likely that allergoids will be superior to native allergenic materials in therapeutic usage since, potentially, much higher doses could be given to allergy patients with greater safety. Higher doses would mean greater amounts of protective blocking antibodies with fewer injections than current practice dictates. Marsh hopes the material will become clinically available within three years.

Marsh, who began the research in England, is working at Caltech with Dan Campbell, professor of immunochemistry. They have also collaborated with Lawrence Lichtenstein, M.D., associate professor of medicine at Johns Hopkins, and with Zack Haddad, M.D., director of pediatric allergy and immunology at the Los Angeles County—USC Medical Center. The research is supported by the U.S. Public Health Service and Hollister-Stier Laboratories in Spokane.

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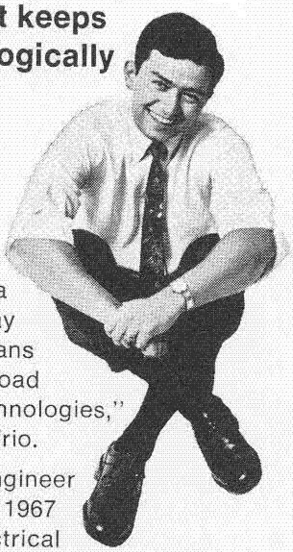

"Working in data processing today pretty much means you work in a broad spectrum of technologies," says Nick Donofrio.

An Associate Engineer at IBM, Nick is a 1967 graduate in Electrical Engineering. He's using his technical background to design circuits for computer memory systems.

Nick says, "Your specialty at IBM can take you into the front yard of half a dozen different fields. In my job, for example, I work with systems design engineers, chemists, physicists, metallurgists, and programmers."

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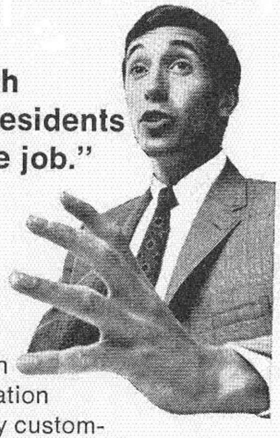

"I'm pretty much the IBM Corporation in the eyes of my customers," says Andy Moran. "I consider that fairly good for an engineer who graduated only two years ago."

Andy earned his B.S.E.E. in 1966. Today, he's a Marketing Representative with IBM, involved in the planning, selling and installation of data processing systems.

Andy's customers include companies with annual sales ranging from 20 million to 120 million dollars. He often works with executive vice-presidents and presidents.

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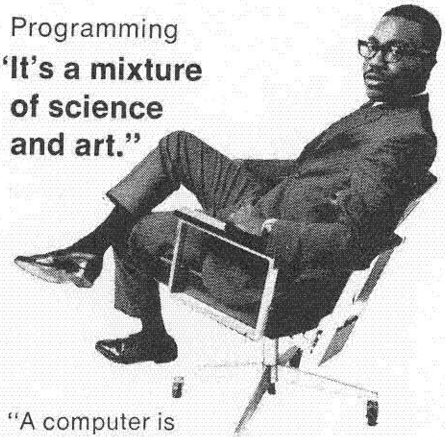

"A computer is practically useless until somebody writes a program for it," says Earl Wilson.

Earl got a B.A. in Modern Languages in June, 1967. He's now an IBM programmer working on a new teleprocessing system linking IBM divisions.

Earl defines a "program" as a set of instructions that enables a computer to do a specific job. "Programming involves science," says Earl, "because you have to analyze problems logically and objectively. But then you have an infinite variety of ways to write your program."

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