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FEBRUARY 1970

Engineering and Science

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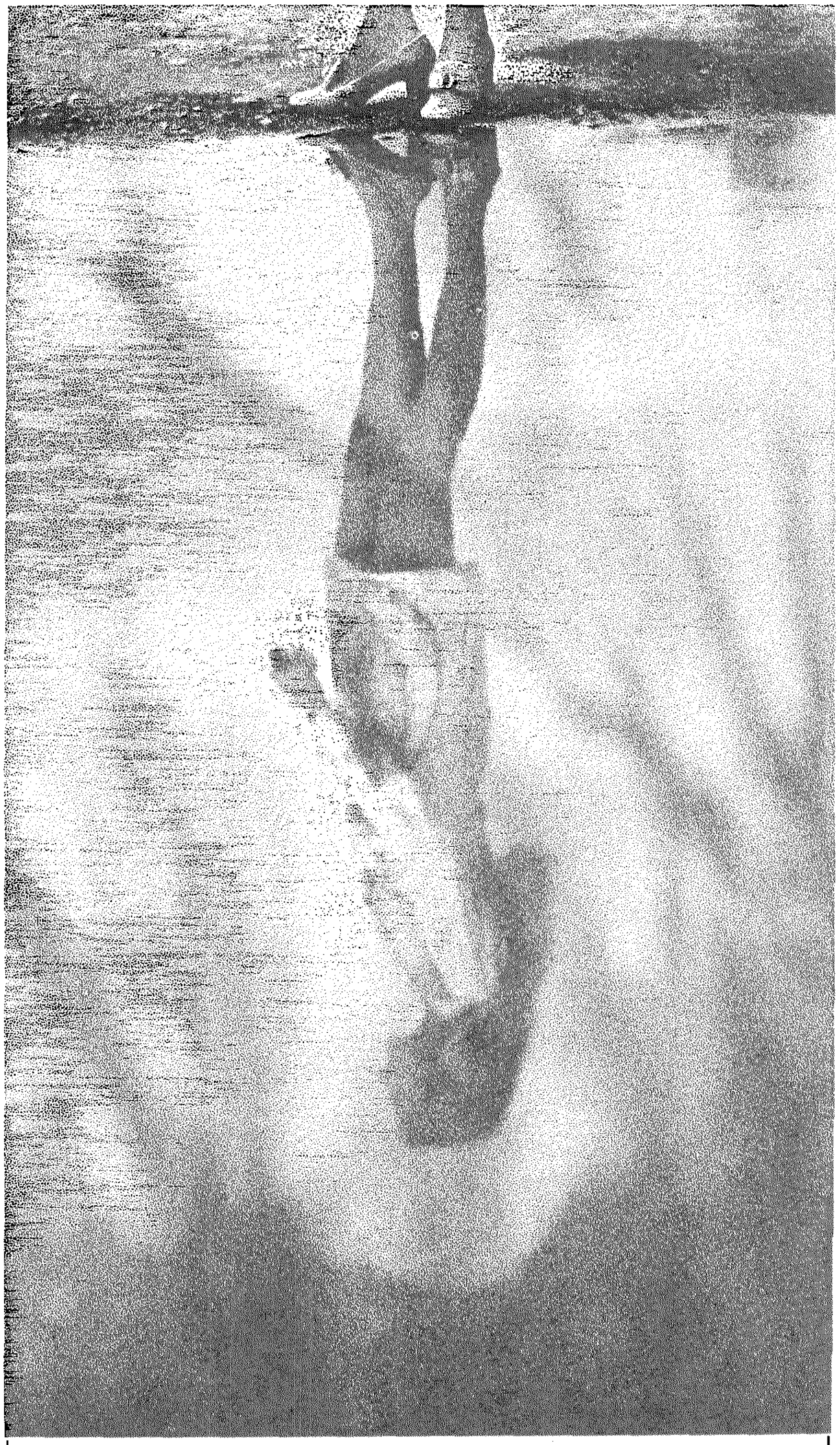

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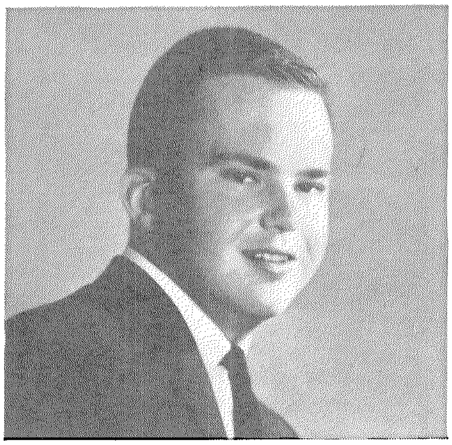

Doug Taylor got his B.S. degree in Electronics Engineering in 1967.

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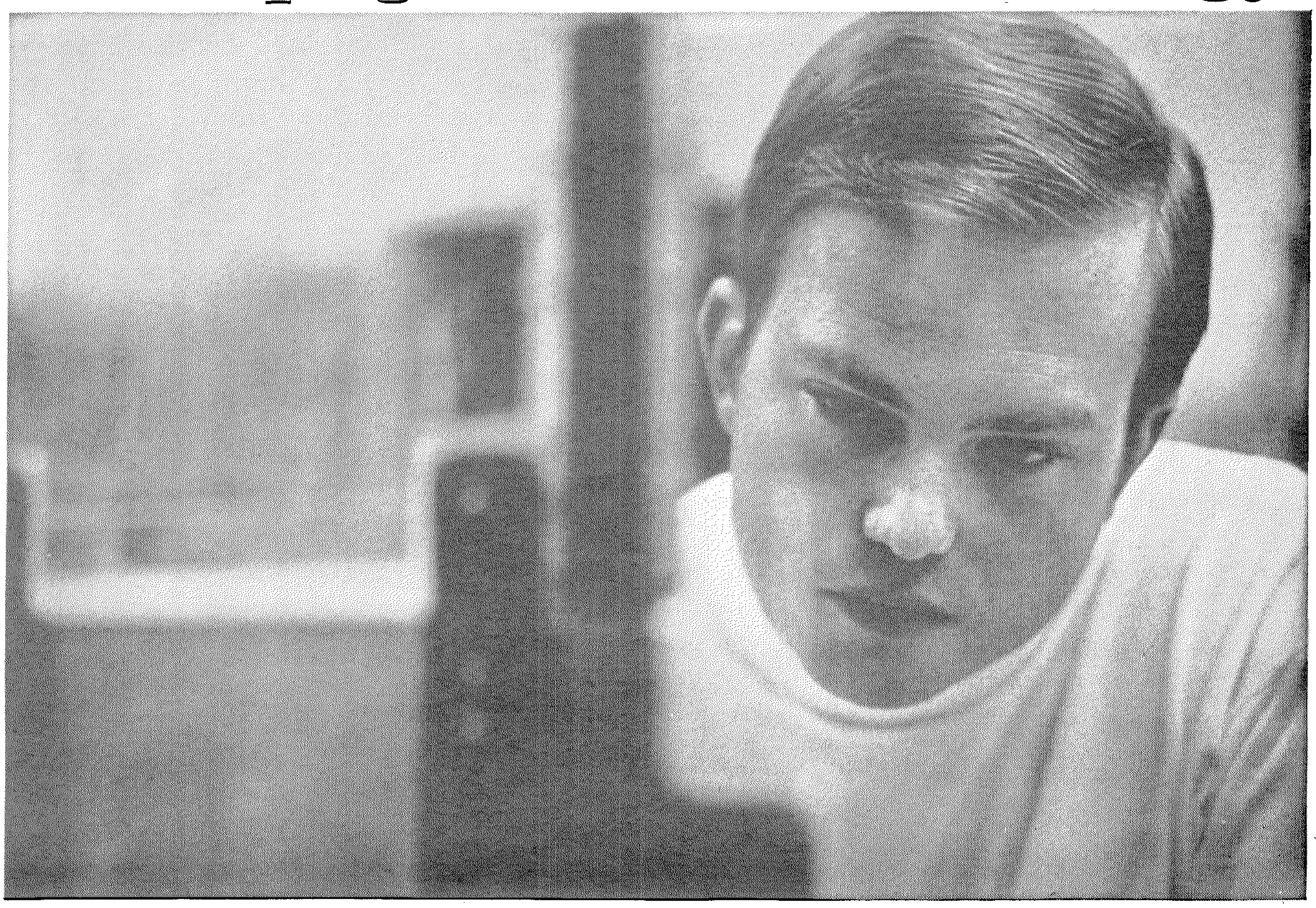

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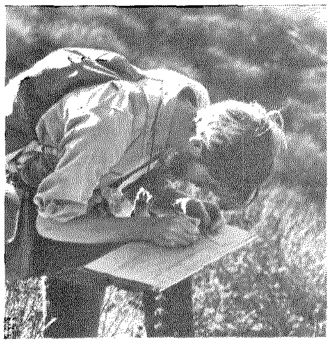

In this issue

Geologists at Work

On the cover, graduate student Jim Powell is engrossed in recording a geologic detail on his map during a field trip in southern California. More pictures of Powell and his geology classmates, who have almost forgotten what it's like to have a Saturday *without* a field trip, are on pages 14-17.

Sponsorship of Basic Research

Arthur Laufer, deputy director and chief scientist for the Office of Naval Research in Pasadena, gave the talk from which the article on page 8 is adapted to the annual initiation meeting of the society of Sigma Xi at Caltech last year. Laufer's experience with research financing is considerable. Each year his office lets and monitors about 550 basic research contracts valued at $37 million in the nine western states; about 60 percent of those contracts are with universities.

An Old Friend

In June 1967 Ronald Scott, professor of civil engineering, wrote a firsthand account for *E&S* magazine of his work with the soil scoop on Surveyor III. He thought it only fitting that, with the return of the scoop to earth by the Apollo 12 astronauts, he should complete the story—which he does on pages 20-23.

Engineering and Science

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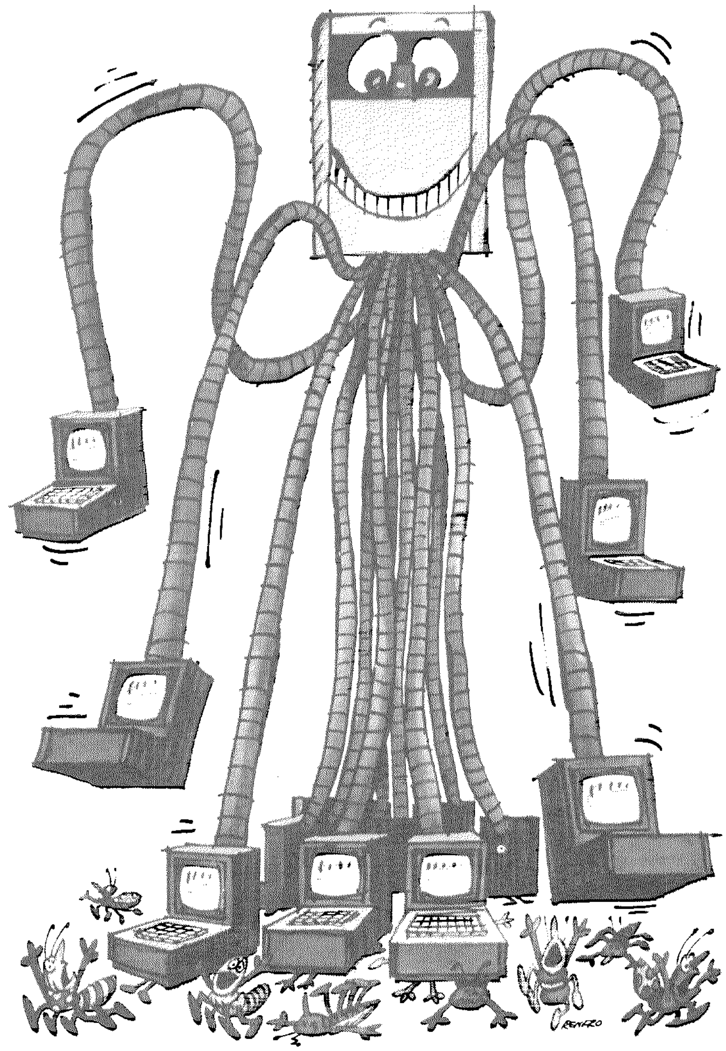

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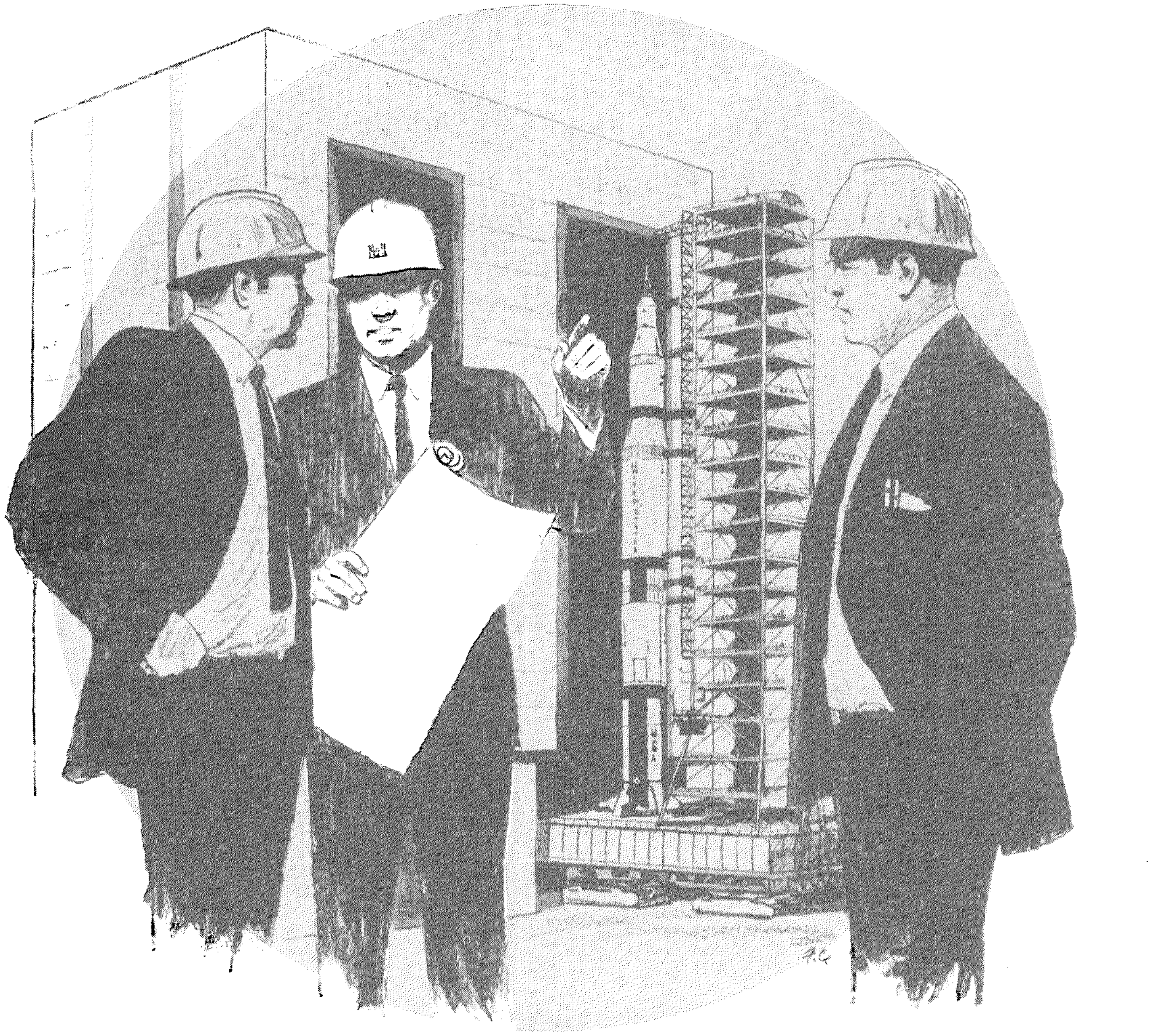

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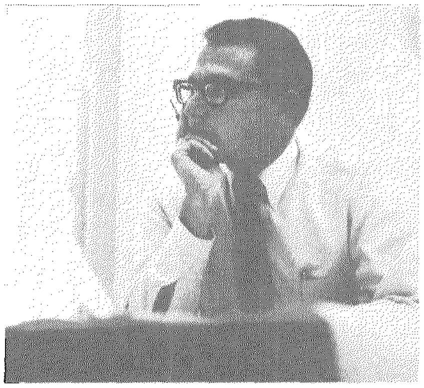

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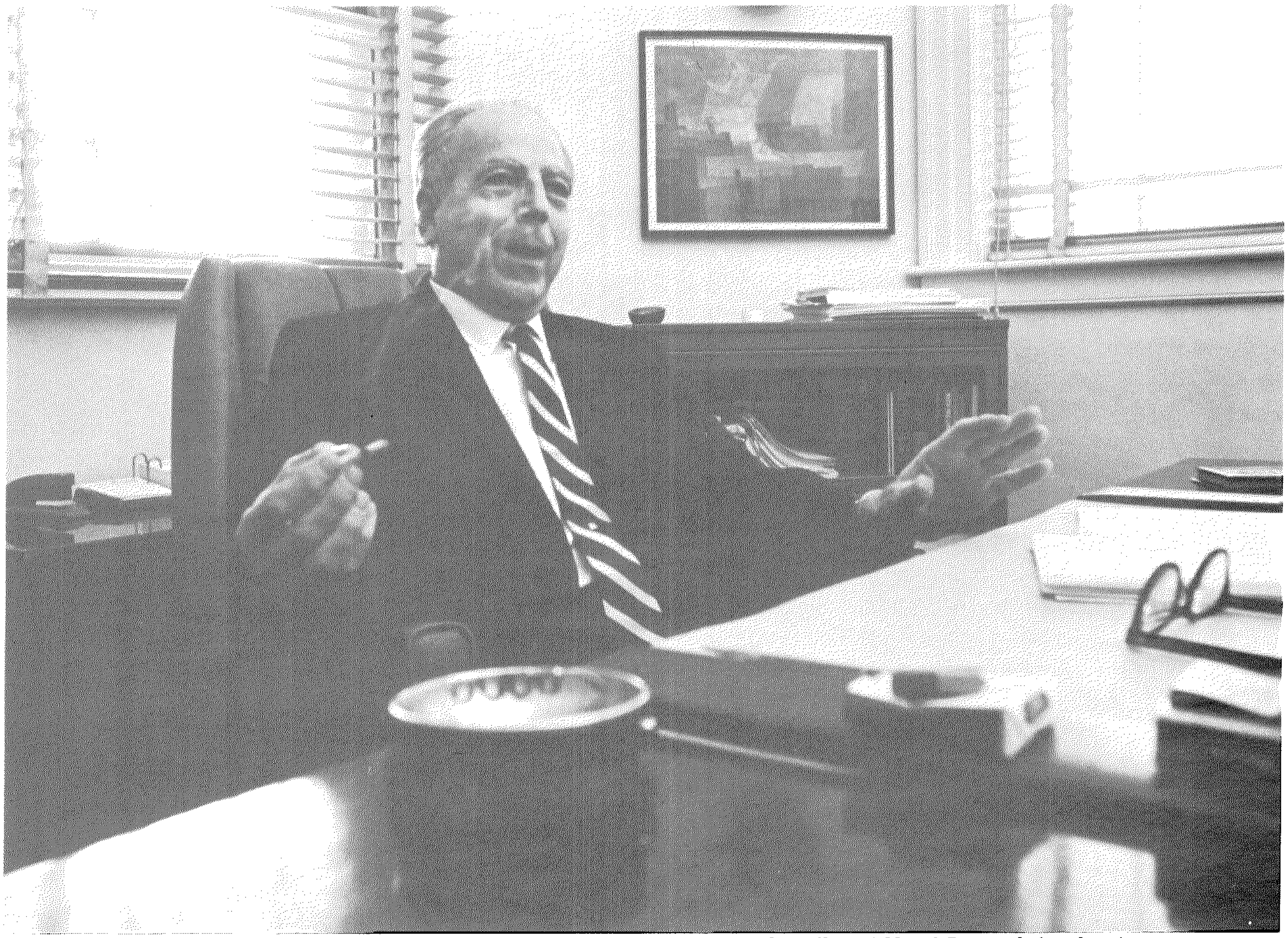


Arthur Laufer, deputy director and chief scientist of the Office of Naval Research for the nine western states.

"From World War II through 1957, federal support was sustained by faith in science; from the time of Sputnik in 1957 into the 1960's, it was sustained by the fear of Soviet competition. These have now been exhausted as justifications . . . "

The Sponsorship of Basic Research

by Arthur Laufer

The search for new technology is often best approached by indirection.

The late Charles F. Kettering, vice president of General Motors and inventor of the automobile self-starter, defined basic research ungrammatically as "something that if you don't do it until you have to, it's too late." Some academicians call it "pure" science, thereby making a value judgment; and some cynics call it "useless" science —another value judgment. The National Science Foundation defines basic research as "the search for an understanding of the laws of nature without regard to the ultimate application of the results." The Department of Defense defines it as "that type of research which is directed toward an increase of knowledge in science. In such research, the primary aim of the investigator is a fuller knowledge or understanding of the subject under study, rather than any practical application thereof." A typical industrial definition is "original investigations for the advancement of scientific knowledge that do not have specific commercial objectives."

Presumably, research which is not "basic" is "applied," or directed toward a practical application. In point of fact, whether a given research project is basic or applied is in the eye of the beholder. It often may be either, depending on the motives of those conducting the work and those sponsoring the work, and, furthermore, it may be basic for one and applied for the other. For example, a university scientist received Navy support for what in his eyes was a "basic" research project in biology entitled "Sweat Glands of the Australian Aborigines." A U. S. senator questioned why such work should be supported by the Navy. He was satisfied when he was informed that the aborigines perspire very little, and that if we could learn why, the knowledge might help us in our undersea programs where men have to live in confined spaces, and water vapor removal is a substantial problem.

Whether a research project is termed basic or applied is primarily a matter of semantics and viewpoint. However, in the hierarchy of science, basic research is claimed to stand higher than applied research. This attitude is unfortunate both because there is much excitement and intellectual satisfaction in good applied science, and because science has always ultimately been justified by its contributions to the welfare of mankind.

But, be it viewed as basic or applied, most of the research done in universities is funded by the federal government. The mutual dependence of the government and the universities in basic research is one of the most significant developments of our time. Last year about $26 billion was spent on research and development in this country; about $17 billion of that came from the federal government. The amount devoted to basic research in universities is impressive: Last year the government provided about $1.4 billion for university research.

This overwhelming involvement of the federal government in academic science is clearly a modern phenomenon. Before World War II the federal government supported some science, but, outside of agriculture and some geology, very little in the universities. In those days, funds for university research came from a number of highly selective philanthropic foundations and from the meager operating funds of the schools themselves.

When war came to Europe, the question arose as to whether science in this country could be mobilized in our own defense. Most of the nation's best scientists at that time were on university faculties, a situation different from that in most European nations. It was therefore necessary to try to develop a mechanism for using the scientific talent in the universities, even though no tradition of substantial government support of university research had previously been established.

In 1940 President Roosevelt established the National Defense Research Committee (NDRC) to "conduct research for the creation and improvement of instruments, methods, and materials of warfare." A year later it was superseded by the Office of Scientific Research and Development (OSRD), headed by Vannevar Bush, which provided support for university scientists doing research judged important to the national security. Where large concentrations of scientists were needed for large problems, organizations were created—such as the Radiation Laboratory at MIT, headed by Lee DuBridge, where very successful work on radar was done.

Most of this research was, of course, applied research and war-related. A large number of university scientists were involved, great technical advances were made, and the results of organizing the scientific potential of the nation were dramatic. However, OSRD was a temporary wartime organization and went out of existence automatically at the end of the war in 1945.

Even before the end of the war, many influential people felt that it would be unwise to allow science, after the

"In the 1890's a Bishop Wright said that God did not mean for us to fly—if He had, He would have given us wings. Bishop Wright had two sons, named Wilbur and Orville."

war, to slip back to the level of prewar scientific activity, These general feelings were given coherent expression in 1945 in the report of a special presidential committee of distinguished scientists, educators, and industrialists, headed by Vannevar Bush. This report, entitled *Science—The Endless Frontier,* provided a blueprint and a timetable for the postwar expansion of the federal support of science.

The committee proposed creation of a National Research Foundation to support basic research, on a large and increasing scale, primarily at universities. But it was not until 1950, five years after the end of the war and the end of OSRD, that a bill was finally passed and signed by President Truman, establishing the National Science Foundation. Its initial appropriation was only $225,000 for 1951, a far cry from the high hopes expressed during the war. This funding was not nearly enough to prevent science from slipping back into the prewar "sealing wax and string" days. Not until 1957, following Sputnik, was the NSF budget raised to the level proposed back in 1945.

However, the United States Navy was standing in the wings, ready, willing, and able. For many generations the Navy had had a strong interest in science, and during the war a group of young, scientifically trained Naval officers began worrying about what would happen to research when OSRD went out of existence. With the support of a number of eminent scientists, they persuaded the Navy in 1945 to establish the Office of Research and Inventions, which merged several Navy research organizations into a single agency.

At this time, with Congress embroiled in the bitter arguments concerning the establishment of the National Science Foundation, it was apparent that a civilian research-supporting agency would not be established by the time it was needed. The Navy, determined not to allow OSRD's research momentum to be dissipated, arranged for the submission of a bill to Congress for the establishment of an Office of Naval Research, which was to absorb the Office of Research and Inventions. Congress established the Office of Naval Research in 1946 and gave it authority for conducting a broad program of scientific research under contracts with civilian organizations.

Thus, the Navy found itself the sole government agency with the power to move into the void created by the phasing out of the OSRD. Ironically, although the civilian OSRD had been concerned primarily with applied, war-related, classified research, the military ONR was to be concerned for many years primarily with basic, non-war-related, unclassified academic research.

As the first permanent federal agency charged with the primary mission of supporting basic research in universities, ONR had to develop a new type of contract which would be acceptable to the universities and would still protect the government interest. Some universities were fearful that federal support would mean federal control and that onerous restrictions would be imposed. ONR developed a system that invited the submission of unsolicited proposals, in lieu of the time-honored system of competitive bidding. The principal product of the contract was acknowledged to be a report or preferably a paper in a scientific journal, rather than hardware. The contract was to be monitored with official restraint and a minimum of reporting.

These features, which seem natural now, were revolutionary in 1946. This display of understanding of the nature of research and of the latitude necessary in the contractual relationship won over the scientific community, and ONR was deluged by a flood of proposals.

By 1949 ONR had 1,200 contracts in 200 institutions, engaging the efforts of 3,000 scientists and 2,500 graduate students. The provision for the support of graduate students as research assistants to the principal investigators was a significant innovation. When the AEC and the NIH began contracting for research, and when the NSF and later the Air Force Office of Scientific Research, the Army Research Office, and NASA were established and undertook their research-supporting efforts, the ONR policies served as their model.

Over the years ONR has sponsored a broad spectrum of scientific research in all the sciences. Much significant work has been supported at Caltech, including Carl Anderson's work in cosmic rays, the Lauritsen-Fowler work on low-energy nuclear physics and nuclear astrophysics, the Owens Valley Radio Observatory, and even much of Linus Pauling's work on proteins.

Today, although the Office of Naval Research still has a budget of over $160 million and more than 2,000 contracts, it supports only a small fraction of the ongoing research in the country. Also, even though many ONR-supported investigators believe their research to be "basic" in the sense discussed earlier, ONR now restricts its support to projects which it views as clearly relevant to its mission. Nevertheless, the results of its early influence are still being felt.

On the occasion of the dedication of the Owens Valley

Radio Observatory in 1961, Lee DuBridge said: "To ONR, the scientific world owes an enormous debt of gratitude for pioneering the way in which the government could assist the universities in the prosecution of the search for basic knowledge and the training of graduate students in scientific and engineering pursuits, and these techniques have been widely copied in other agencies of the government today."

In the year 1966, ONR's twentieth anniversary, Professor Harvey Brooks of Harvard said: "As one reviews the history of American science and technology in the last 20 years, one cannot fail but be struck by the strategic role which ONR-sponsored work has played. In fact, when one considers its present minor fiscal role in research support compared with what it was in the early days, one is surprised at its still major importance and influence. Wherever the most important advances are being made, one still seems to find ONR present with at least some support. A catalog of areas in which ONR-sponsored scientists have pioneered shows how frequently ONR has been there with the right science at the right time, even though few foresaw the usefulness and relevance when ONR first began to sponsor it."

These statements are quoted here in order to make a point: that the existence of a number of different mission-oriented research-sponsoring agencies, of which ONR is only one, each with its own motivation, is good for science.

Still, the existence now of so many similar research-sponsoring agencies leads, from time to time, to serious proposals that basic research ought to be supported through one super agency, in part to achieve administrative neatness.

Such a system would be fraught with danger.

The various mission-oriented agencies of the government have urgent need for the scientific talent of the universities to assist in the solution of our many national problems, ranging from the conquest of disease to the provision of an adequate national defense. A single monolithic agency cannot be relied upon to have the wisdom needed to support in sufficient depth the various areas which are of overriding importance to the missions of the other agencies.

The diversity of our present system for the support of academic research is probably the greatest source of our scientific and technical strength. There is no single best way to support science. Our scientific choices are now governed by a wide range of priorities, environments, and motivations, leading to a strong and flexible system. The fears of federal control coming with federal funds have proved unfounded perhaps largely because the funds have been injected through a variety of agencies for a variety of purposes. The multiplicity of alternative sources of support is, I believe, one of the most important safeguards for the independence of the individual scientist.

A monolithic science-support agency would be highly vulnerable to Congressional action. From World War II through 1957, federal support of science was sustained by faith in science; from the time of Sputnik in 1957 into the 1960's, it was sustained by the fear of Soviet competition. These have now been exhausted as justifications, and the Congress demands that the present emphasis be on utility. The Congress recently has revealed an apparent loss of confidence in the worth of basic research. The mission-oriented agencies can often provide justifications for the support of science which are more readily endorsed by our legislators than are the justifications for "pure" science.

Consider another aspect of this problem. Every agency must plan its research program to match the available funds and to attempt to place its support in areas which are likely to prove productive. Such planning must inevitably be based on predictions of the future, and, unfortunately, our crystal balls are very cloudy. This difficulty would be compounded many times over if all the planning of science support were in the hands of a single agency. Let me give a few examples of our lack of foresight.

A technical forecast in 1937 missed computers, atomic energy, antibiotics, radar, and jet propulsion. Yet all of these were incorporated in successful systems within a few years after the forecast.

Another example. Vannevar Bush, in testimony before the Special Senate Committee on Atomic Energy in December 1945, said: "There has been a great deal said about a 3000-mile high-angle rocket. In my opinion such a thing is impossible . . . The people who have been writing these things that annoy me have been talking about a . . . rocket shot from one continent to another carrying an atomic bomb, and so directed as to be a precise weapon which would land on a certain target such as this city. I say technically I don't think anybody in the world knows how to do such a thing and I feel confident it will not be done for a very long period of time to come. I think we can leave that out of our thinking." On the same subject, Frank Malina said last year that in 1936 Clark Millikan was dubious about the future of rocket

"It is difficult to understand how those who want the university to become involved in the problems of society now can demand that the university eliminate its involvement in the most serious problems of our society."

propulsion, and that in 1938 a senior Army officer on a visit to Caltech stated there was little possibility of using rockets for military purposes.

In the 1930's Robert Millikan, in answer to an English bishop's proposal that a 10-year moratorium be imposed on research to allow civilization time to cope with its creations, said: "The bishop need not worry about science, or about the absurd possibility that mankind, armed with the energy of the atom, might blow itself to kingdom come. That energy is destined to stay locked in the atom. The Creator has put some foolproof elements into his handiwork and . . . man is powerless to do it any titanic damage." Similarly, in 1933, Lord Rutherford, the father of nuclear physics, said: "Anyone who expects a source of power from the transformation of the atoms is talking moonshine." Karl Darrow published a paper presenting five reasons why Nature would never allow a chain reaction to take place. In 1938 nuclear fission was discovered by Hahn and Strassman, and in 1942 a chain reaction was achieved by Fermi.

In 1944, Theodore von Karman said: "Obviously, it is an intriguing question whether there are any intrinsic limits for flight velocity. Many people will ask 'Shall we ever fly faster than sound?' I do not believe that at the present this question can be answered by a straight yes or no." In the same lecture he said: "To some extent the question of supersonic flight is analogous to another intriguing problem discussed sometimes by serious men, more often by authors having more imagination than scientific knowledge. I mean the question of the feasibility of navigation off from the gravitational field of the earth. Of course, some fabulous new fuel would change the situation completely in both cases. However, basing the consideration on power plants and fuels which are available or which we hope to have with reasonable expectation, the answer to the question of the feasibility of planetary navigation is probably negative." This talk was given in April 1944; on October 17, 1947, Captain Charles Yeager of the Army Air Force flew the Bell X-1 rocket research aircraft at supersonic speed in level flight.

One should also not forget that in the 1890's a Bishop Wright said that God did not mean for us to fly—if He had, He would have given us wings. Bishop Wright had two sons, named Wilbur and Orville.

Now, I have not presented these examples to deride a number of eminent and extremely competent scientists of the past. I am merely seeking to show that none of us, not even the best of us, is very competent in predicting the future. And this is why planning fails and cannot help but fail.

The search for new technology is often best approached by indirection, and a decentralized pluralistic decision-making system such as we now have, with a multiplicity of research-sponsoring and -planning agencies, provides us with a redundancy which serves to minimize the harmful effects of high-level planning.

Finally, I would like to make some reference to the attempt by some segments of the academic community to force the termination of all defense-related research on university campuses. I have great sympathy for those who would like to see science more deeply involved in socially constructive activities, who are concerned about the uses to which science is being put, and who have a hunger to make science relevant and benign. However, the elimination of defense-related research from the campus would not solve the problems they wish to see solved, and would introduce certain new problems.

First, most of the research supported by the DoD on university campuses, though highly relevant to DoD needs, is regarded by the investigators as basic research. But not only the DoD benefits from this research; the so-called socially constructive agencies benefit at least as much. For example, the Navy pioneered and developed techniques for preserving whole blood for relatively long periods of time by means of rapid freezing techniques. While it is true that such preserved blood is of great medical value for military personnel aboard ship, many more civilians will benefit from the resulting improvement in operation of blood banks throughout the country. On the other hand, there are many cases in which the results of NSF-supported research have been used by the military for less humanitarian purposes.

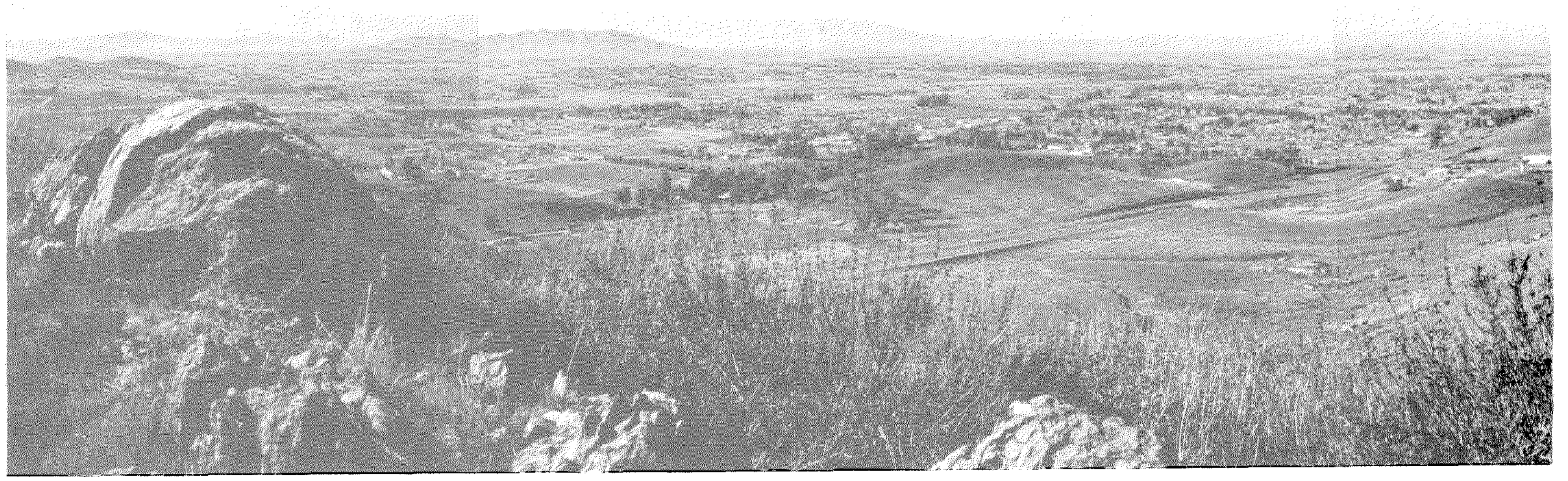
The fact is that the results of free and unhampered basic research, freely published, may be used by any agency of society for whatever purpose. A scientist seeking support for basic research from a particular agency does not necessarily share the motives of that agency. He has his own, presumably lofty, motives for undertaking that research. Similarly, if the research is truly basic, it would be difficult for him to assess the ultimate social consequences of his work, whoever the sponsor might be. It appears to me that it is the nature of the research which is important, not the identity of the sponsor. Hence, forcing DoD research off campus into research institutes and industrial laboratories will not prevent the DoD from benefiting from the non-DoD research which remains on campus, but will deprive the faculties and graduate students of hundreds of millions of dollars of research support which is sorely needed for both scientific and educational purposes.

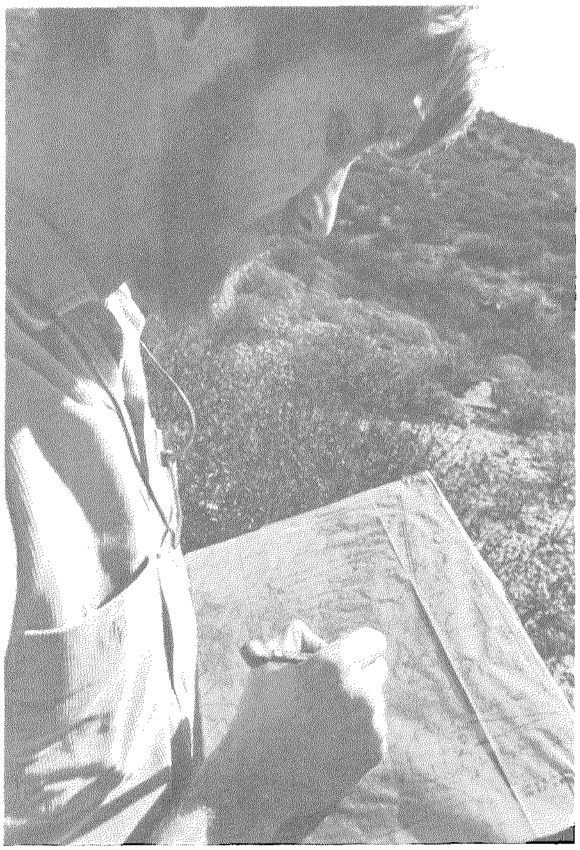
Second, if DoD research were forced off campus, there is no assurance that the scientists on campus would be able to turn their efforts to "social" problems. Social desirability does not insure technical feasibility. The reason why many scientists apply for DoD support is that the basic science problems which the DoD will support are comparatively easy problems which discipline-oriented scientists know how to attack. The difficult social problems of racial intolerance, urban congestion and decay, pollution of the environment, and international tension and conflict—depending as they do on the preferences, desires, and emotions of human beings—do not yet appear to be amenable to solution by the techniques which scientists know how to employ.

Finally, it is clear that American society places a high value on military strength for defense. Most Americans believe that our country cannot rely for survival upon purely ethical superiority in a world which includes the Soviets and Czechoslovakia, the Biafrans and the Nigerians, Israel and the Arab countries, and mainland China. The USSR is increasing its military R&D effort at a disturbing rate, and both West Germany and Japan have announced plans for major increases in their efforts. Even the disenchantment with the Vietnam war has not made America feel that defense is dishonorable and unethical. Of all the unforgivable things the Department of Defense might do, in the view of most Americans, the most unforgivable would be to allow this nation to be conquered through a technological surprise. They have not forgotten what the world now would be like if Hitler's Germany had been the first to create the atomic bomb. Thus defense research is now and will long be, I am convinced, an integral part of our society. It is therefore difficult to understand how those who want the university to become *involved* in the problems of society now can demand that the university eliminate its involvement in the most serious problem of our society. They should demand, rather, that the university use all its considerable influence to assure that our military strength be used *only* for defense.

Disengagement of the university from defense research would deprive society of an important safeguard. The record shows that university scientists have consistently led efforts to awaken our society to the dangers of the misuse of technology, of the arms race, and of the pollution of our environment. Increasingly, major national decisions must be made on issues that involve considerable scientific or technological complexity, and therefore government agencies and their industrial contractors often have a near-monopoly on the relevant information. Participation of university scientists in DoD-related work gives them the technical backup they need to provide sophisticated and independent criticism of public policy. Defense research will be done, whether it is done on or off the campus. But if the university were to withdraw from DoD-related work and remain aloof, who else would be available to make independent analyses and challenge the government positions on complex technological questions?

The university serves a unique public-service function in defense research. It is to be hoped that the university community will avoid the practice of what Reinhold Niebuhr called "the strategy of fleeing from difficult problems by taking refuge in impossible solutions."

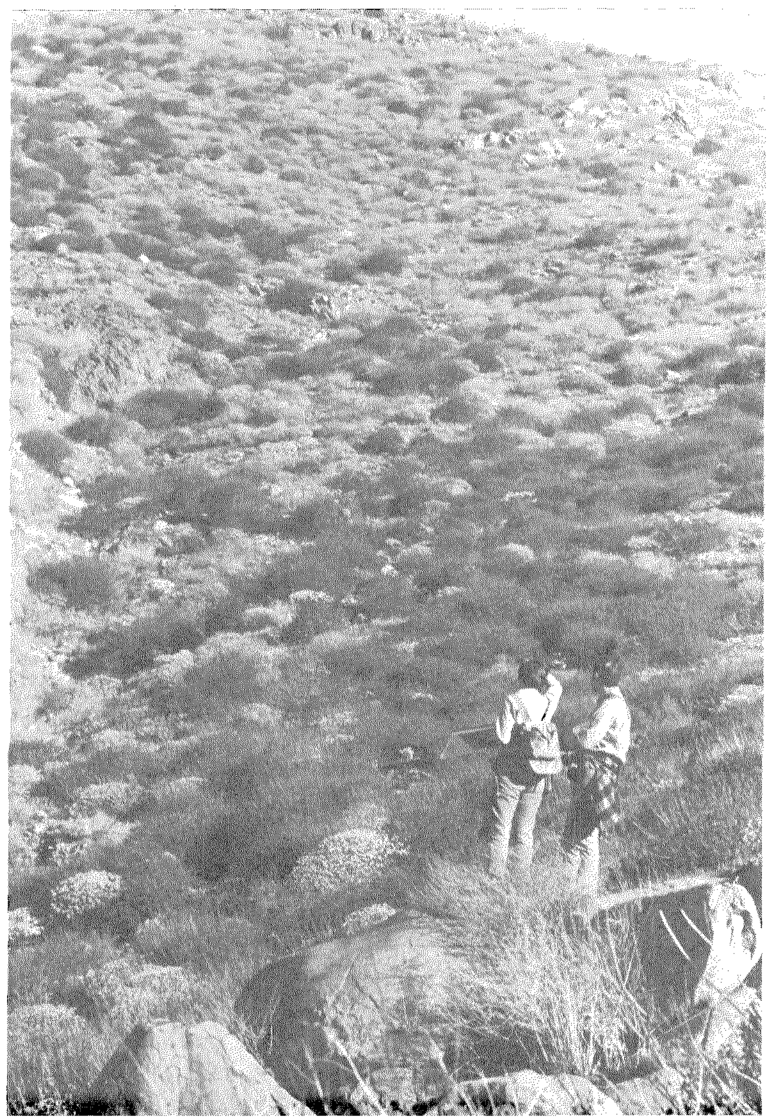


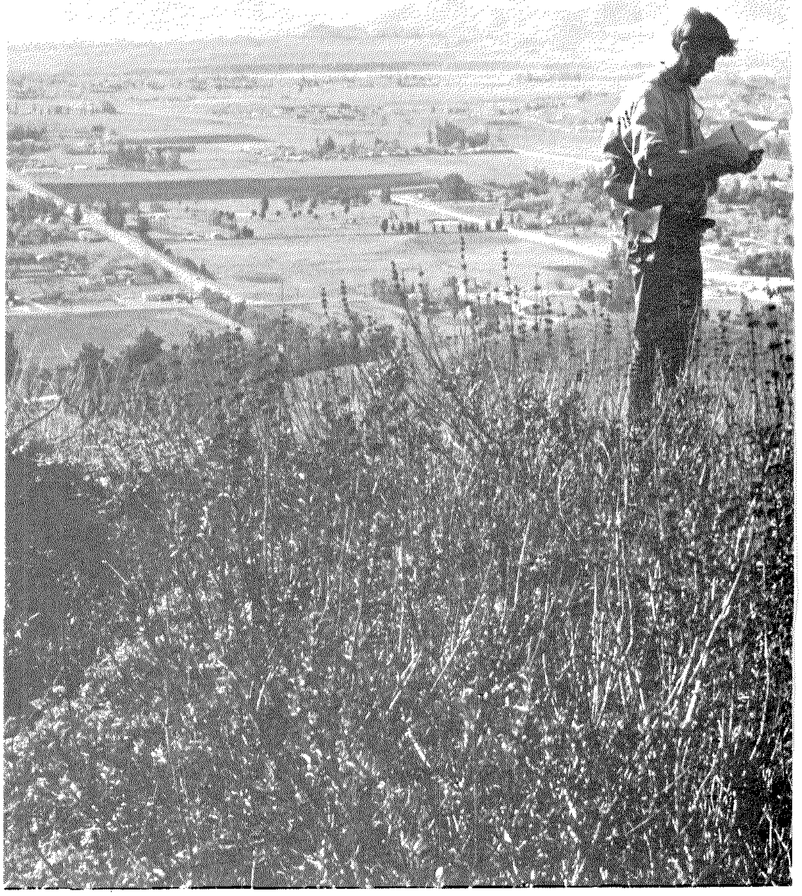


IN THE FIELD

Field investigations—the kind that hardy geologists have been making for years, armed with little more than a blank map, a compass, and a hammer—remain a major part of the work of most of the Caltech geology faculty, no matter how sophisticated their laboratories have become. And students in the division get a healthy dose of field work in the course of their education—generally two full-year courses plus a six- or eight-week summer expedition just to get a BS degree.

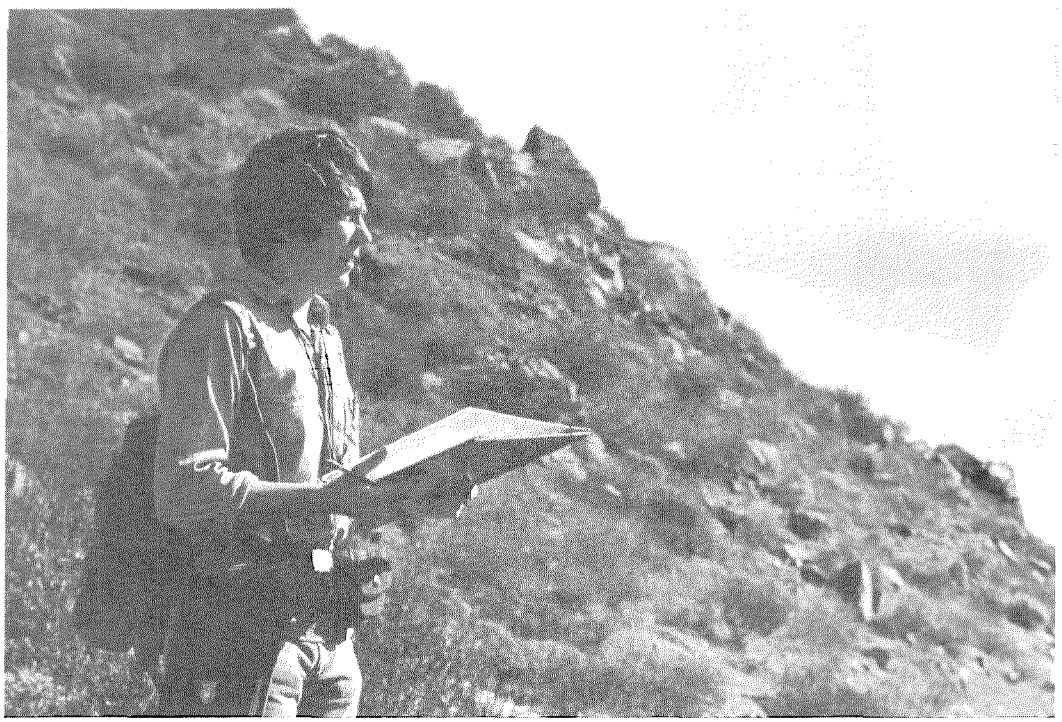
These pictures of the Ge 121 *advanced field geology* class, taken on one of the nicest January 31sts in memory, show that not only does earth abide, but that geologists in the field haven't changed too much either. The group includes graduate students Jo Laird, Robert Powell, and David Tiffany; seniors Mark Boulé and Richard Doyle; Hugh Taylor, professor of geology; and graduate teaching assistant Bruce Carter. This particular field trip was number three in a series of eight being made to map the western Jurupa Mountains, a region consisting largely of metamorphic and igneous rocks in Riverside County, about an hour's ride from the campus.

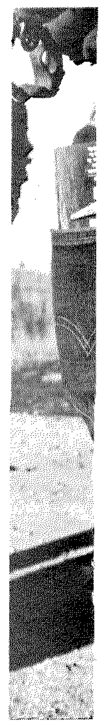


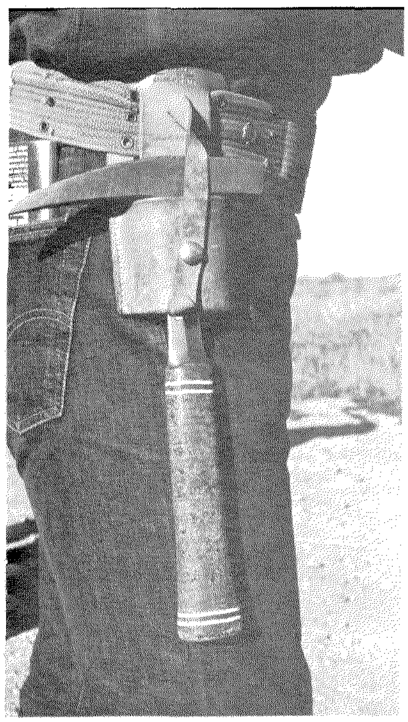


Not too long ago the geologist, like the poet and philosopher, could produce profound insights with almost no overhead expense. A rocky hillside is still his greatest—and cheapest—research tool, but the techniques of other disciplines have refined his observational abilities in the last few decades.

In his laboratory today he is scarcely distinguishable from a chemist or physicist. The rooms are crammed with glassware, blinking lights, and computer printouts for all sorts of elaborate analyses. But for the most part that equipment serves one basic purpose—to help him better understand the origin and history of a sequence of rocks observed in the field. The geologist then combines histories of individual rocks into histories of regions, regions into land masses, and when he examines histories of land masses, gets some clue about what has happened to make the earth the way it is.







Can Excellence Be Managed?

No.

It can, however, easily be aborted.

by Harold Brown

In thinking about questions of management and how management should function at Caltech—both on campus and at JPL—one is immediately drawn to the fact that we insist on excellence and strive for preeminence. That in turn raises the question of how management can function in an atmosphere of excellence and how it can encourage its development.

My frame of reference is that of having been a manager, at least in part, since a few years after I got my PhD and almost entirely for the past 15 years—in a development laboratory, in government, and now at a university.

Can excellence be managed? Let me point out some distinctions between the campus and JPL in this matter. The campus is a center of discovery of new ideas and of teaching—at the frontiers of science. Even in its very considerable and very important activities in engineering and applied science, the campus is centered on ideas and experiments, not on objects or development. But JPL is a center of excellence too—excellence in development, excellence in systems design, excellence in developing and applying advanced technology.

These differences between the specific aims of the campus activities and the specific functions of JPL lead to some differences in what constitutes the appropriate style of management. But similarities exist too, and, in my opinion, the similarities are greater than the differences.

The similarities make it possible to formulate some guidelines for discovery and development kinds of activity at Caltech—both on campus and at JPL. The first guideline I would suggest for the function of management is that it exists to help place the technological and educational activities of the Institute in a broader setting. On the campus, the function of these educational and research activities is to develop new scientists, engineers, and educated and thinking men. The development of these ideas, and these people, is a very important value in itself. For that reason the activities on campus must have a core, a center, determined by the interests of the faculty and of the students.

Though some people outside of JPL may think its aim is simply the performance of very difficult technological feats, those are not in themselves the objective of the activities at JPL. Even more than on campus the purposes of the activities at JPL are inherently oriented toward the demands and needs of the larger society around us.

But even on campus we have to pay attention to society's *long-range* interests. We in the United States have always believed that education and fundamental science and technological research are important to society. In the last few years in particular that belief has been somewhat eroded. This erosion is a great danger to the university and to society. And it seems to me that this places upon management, top management particularly, the responsibility for closing the gap between the inner, encapsulated purpose of the Institute and the pressures and desires of the world outside.

This responsibility involves, first of all, explaining science and technology and development to the public. They don't really understand it, but many of them want to. Good science writing is rare, but in our own local area we get more than our share from some of the science writers of the *Los Angeles Times*. Two examples come immediately to mind of people in the Caltech family who have done a great deal on informing the public. One is Al Hibbs at JPL, and the other is Professor Kip Thorne on the campus, who last year won a national award for the year's best science writing in physics and astronomy.

A second example of how to connect the inside and the outside and to think of what society will want or will profit by 30 or 40 years later is quite different. This is the question of deciding in which field to work—such as the choice made in 1928 on the campus to go into biology.

Now the campus is moving into behavioral biology and into behavioral

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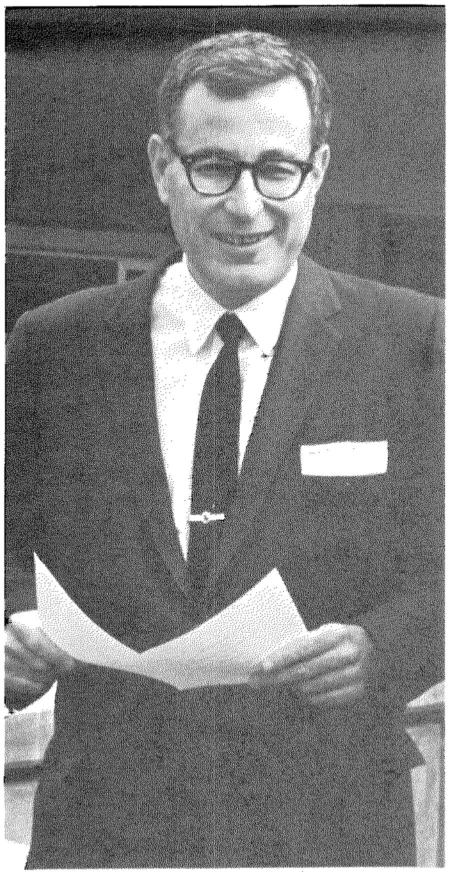
Adapted from a talk given to the Caltech Management Club on January 13.



Harold Brown

"Administrators, including university presidents, are overhead . . . This doesn't make us managers second-class citizens, but it should help us keep our priorities straight."

science and social sciences. Both the campus and JPL, in their own ways, are moving into environmental engineering and into some of the questions that deal with environmental pollution and transportation.

Another guideline for management is to take the responsibility for producing a form of governance that fosters acceptable relations among the various parts of the community of the institution: on campus, the relations between faculty and students and between both of them and nonacademic personnel; at JPL, relations among people in functional areas and between them and scientists.

As to further examples of what to do and what not to do, I suggest first, not competing with subordinates, but instead helping them grow—and second, remembering what your central goal is, which requires knowing what's going on around you.

At a university it is of central importance to have a critical and inquiring spirit. Good faculties have it; students have come to have it in even greater measure. I would suggest that JPL's greatest value to NASA in terms of its Caltech connections is the existence of that spirit on the campus, and a primary management function at JPL is to preserve and expand it at JPL.

But to question society's, or NASA's, or even students' central beliefs these days is to risk (indeed sometimes to ensure) the displeasure of those bodies. Management has the function of explaining the need for the people at our kind of institution to ask questions—and to defend that need. It also has the responsibility to see that there is some sort of coherence to the activities of teaching, learning, doing research, or doing development, and to see that the right to question and to criticize is exercised in a way which does not infringe upon the freedoms of others.

A third function of management is to remember that there are limits to the role that management can play. Specifically, if administration becomes an end in itself, it will destroy excellence in the institution that is being administered. Caltech is dedicated to science and to technology, not to administration, or to buildings and physical plant, or to accounting. Those functions—like the president's office—are service organizations. In the tug-of-war between efficiency and responsiveness we need to try to maximize both, but there is even less excuse for lack of responsiveness of the administrative functions than there is for inefficiency.

Financial strictures on campus and at JPL, which are very real, strongly dictate prudence and efficiency. On the campus we are accountable to the donors of the money, and to the students who pay tuition; and at JPL we are accountable to the taxpayers for the tax dollars that support the operation. This accountability dictates prudence and efficiency. But the best accounting system and the most careful adherence to regulations in the world will not produce Nobel Prizes, brilliant students, or successful Mars probes; and without those things we have nothing to offer to justify our existence.

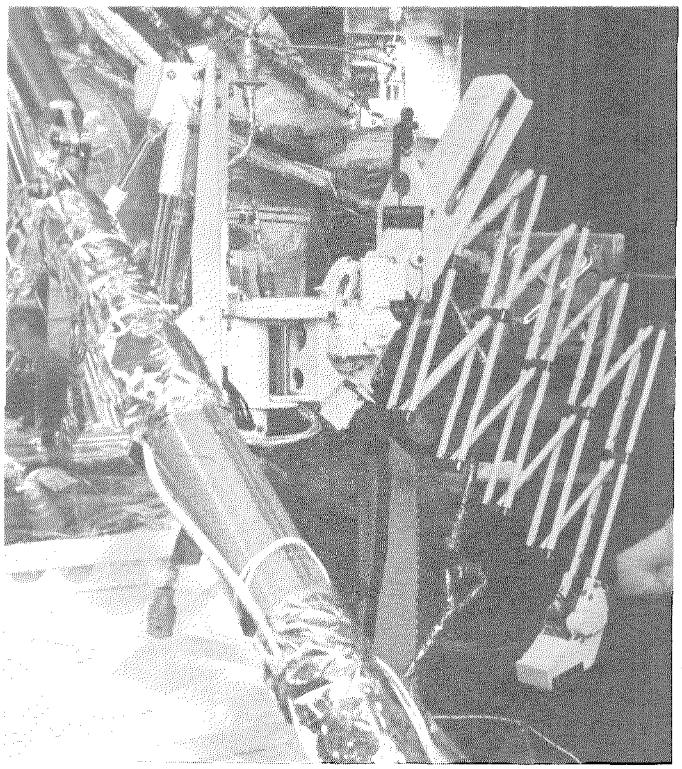
What I'm saying is that administrators, including university presidents, are overhead. The success of what we are trying to do depends upon the talents of our outstanding faculty and students on campus, and on the talents of the engineers and scientists at JPL. No matter how well the rest of us do our jobs, only they can keep Caltech and JPL excellent. This doesn't make us managers second-class citizens, but it should help us keep our priorities straight.

As managers and administrators, we often claim—and sometimes we have—a better long-range view of what actions and policies are best for the work of our professors and students, our engineers and scientists. We're supposed to have more wisdom as to how best to make their activities flourish. That is the justification for our function. But we must not let this lead us into thinking that the Caltech campus or the Jet Propulsion Laboratory are therefore to be run for *our* benefit. They are not. And whenever we take actions aimed otherwise than at enhancing *their* work —the work of our engineers and scientists, our faculty and our students—and *their* achievements, then we undermine the health of the Institute and the Laboratory. How well a manager performs his function determines whether he is helping to solve the problem or is a part of the problem.

So, my answer to the question: "Can excellence be managed?" is that it cannot be managed into existence. It can, however, easily be aborted. It is our job to nurture, encourage, and augment excellence. In that sense I think we can hope to manage excellence, and I know we shall all keep on striving to that end.

On Meeting an Old Friend, Slightly The Worse for Wear, After a Lapse of Two and a Half Years

by Ronald F. Scott

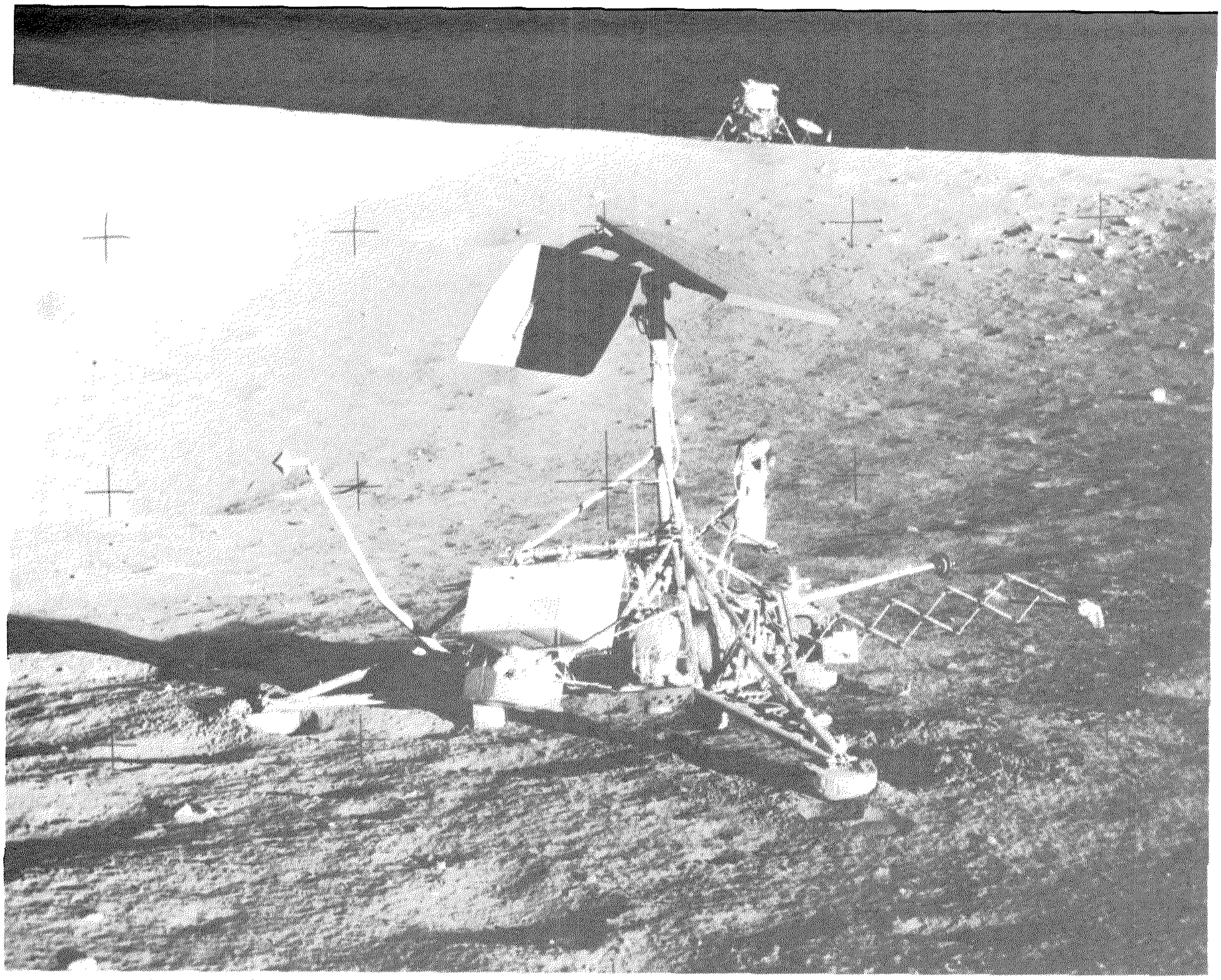


Surveyor III in the clean room before the 1967 launch from Cape Kennedy, where Scott got what he thought would be his last good look at the surface sampler.

During and following the first manned landing on the moon last July, I was in Houston to assist in the evaluation of the physical and mechanical properties of the lunar soil. On one of these occasions I was asked what I thought of the idea of aiming Apollo 12, the second manned mission, at the landing site of Surveyor III, a Jet Propulsion Laboratory unmanned vehicle that soft-landed on the moon in April 1967. I was enthusiastic about the goal, but dubious as to the chances of accomplishing it. Apollo 11 had landed several miles from its intended landing site in a location that was not identified until the descent movies were processed and examined after the astronauts' return.

There was a special reason for my enthusiasm, however. Seven years ago I had proposed using a device called the surface sampler on the Surveyor spacecraft series, to explore the mechanical properties of the moon's surface—whose nature was only speculation at that time. In the course of time, much delay, many committee meetings, and a good deal of hard work, a surface sampler was mounted on the third Surveyor spacecraft. As principal investigator for the sampler as an experiment, I spent some time with Floyd Roberson, my JPL colleague, at Cape Kennedy late in 1966 taking part in the final calibration of the surface sampler.

When that was over, the spacecraft was moved from the "clean room" to be loaded with propellant, and we said goodbye to the surface sampler, with the hope that we would next meet on television. On the sharp end of an Atlas-Centaur rocket, the surface sampler departed the earth on April 17, 1967, for a landing site in Oceanus Procellarum on the moon. Surveyor III became operational on the lunar surface on April 20, 1967. For the next two weeks Floyd Roberson and I happily and sleeplessly

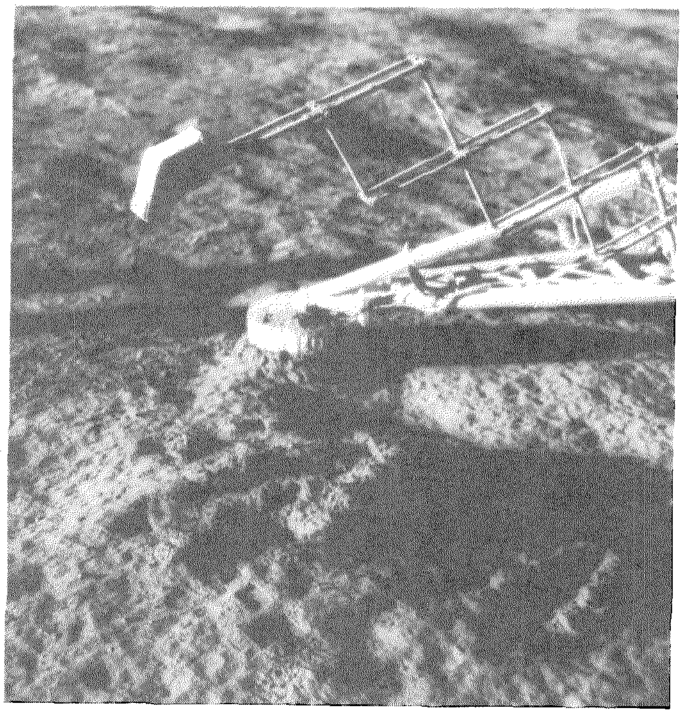


Uncle Sam's used spacecraft lot.

played with the lunar surface soil on the inside surface of a 650-foot-diameter crater close to the aiming point. The surface sampler performed very well, and I finished that lunar day with a good understanding of the nature of the lunar surface, as well as some controversial results.

For no particular reason that I can recall, we tidily raised the surface sampler as high as it would go and moved it to the extreme right edge of our area of operations before the spacecraft was put to bed. When Surveyor III failed to respond after the lunar night, I considered that my time with that surface sampler was over, and retired to work over my results. Two more surface samplers were flown, on the following Surveyor, which crashed, and on Surveyor VII which landed successfully near the crater Tycho far to the south. We exercised the Surveyor VII surface sampler thoroughly in its first lunar day in January 1968, and sent a few commands on

"The smell of the lunar surface pervaded the room, and we proceeded to photograph the scoop from all angles."



The surface sampler as it appeared to the Apollo 12 astronauts on the moon last November. The trenches it dug two and a half years before are still clearly visible.

the second lunar day in February to which it responded for the last time.

Meanwhile, the Apollo program was moving ahead. Among the innumerable pieces of information needed for the manned landing, the results from the surface sampler operations gave assurance that the landing itself, and the astronauts' activities on the lunar surface, could proceed safely—as far as the soil's strength was concerned. The landing of Apollo 11 demonstrated the satisfactory nature of a number of assumptions which had been made, and more ambitious plans were laid for Apollo 12 last November. If the Lunar Module landed sufficiently close to Surveyor III, Astronauts Conrad and Bean were to visit it, and remove selected portions of the Surveyor with a pair of cutters. Because the surface sampler had a high-strength steel tape attached to it, it was not thought possible that the astronauts could bring back a part of the sampler. However, they did plan to remove the Surveyor television camera, some tubing, and a piece of cable. The condition of these components has great scientific and engineering interest due to their stay of two and a half years on the lunar surface.

As part of the Apollo Soil Mechanics Team, I was sitting in a "Science Support Room" at Mission Control in Houston on November 19, 1969, when we heard Bean's excited words during the final stages of descent:

"Okay, look out there; I think I see my crater . . .

"There it is, there it is; son of a gun—right down the middle of the road—

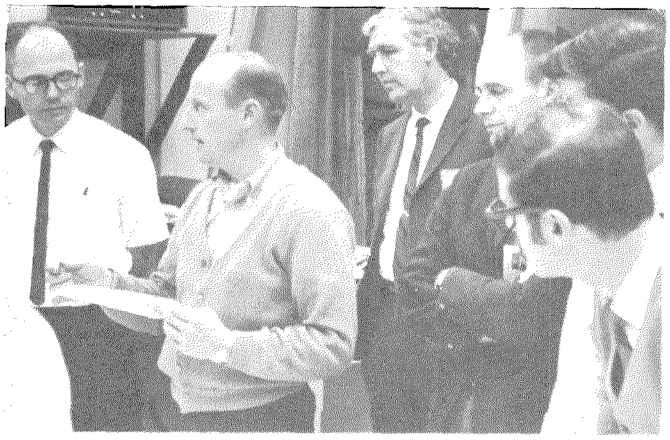
"Look at that crater—right where we are supposed to be."

For the first time I realized that Conrad and Bean might really manage to visit Surveyor III. Later, Conrad emerged from the landed Lunar Module and reported he could see Surveyor to the southeast on the inside slope of what had become "its" crater. Following a first trip outside the Module to arrange a number of pieces of equipment on the surface, and after a rest inside, the two astronauts disembarked again on a journey of exploration which was to include Surveyor toward the end. After two hours of traversing the lunar surface, taking samples, and driving core tubes, the men made their way to Surveyor and began poking around it. They obtained various pieces of the vehicle as required, and then Conrad remarked casually that he had got the scoop and had put it in the scoop bag. This was entirely unexpected, and I did not know at the time how he had accomplished it. When the crew returned to earth, I asked Conrad what had happened. He said he had, as an experiment, put the wire cutters to the sampler's steel tape, and, as expected, he could not cut it. Then he gave the cutters a twist, and to his surprise, the tape parted at a weld. All he needed to do to free the scoop was to snip through three aluminum supporting arms and some wires behind the first joint. He was able to do this because in April 1967 we had fortuitously left the sampler in its most elevated position. Astronauts in space suits cannot at present bend down.

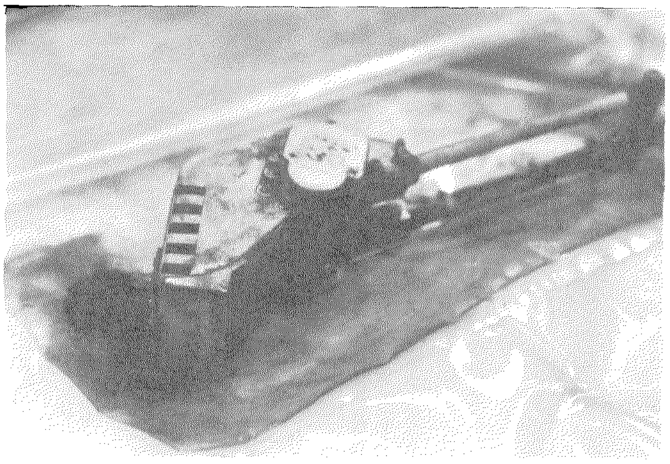
Along with the Surveyor television camera and other parts, the scoop spent the few weeks after the astronauts' return in quarantine at the NASA Lunar Receiving Laboratory in Houston. In due course, the quarantine was raised, and the returned surface sampler part—inside two murky teflon bags—was brought into an examination room. A small group of people was present, including astronauts Conrad and Bean, as well as myself, excited not for the first time, and probably not for the last, by the events of the space program. Some discussion ensued about the process of removal of the parts from Surveyor and the astronauts' observations at the time, and then we were ready to open the bags for a preliminary inspection of the scoop. Because the lunar soil adhered to the sampler during our operations of two and a half years ago, and had remained on it, the scoop was accompanied by a small amount of granular material in its inner bag. Since it had not been preserved in a vacuum, the soil had floated around in the bag under the zero gravity conditions of the return trip and had coated the entire mechanism and the inside of the bag with a fine particulate layer. If I had known I would see it again, I would have left the scoop completely packed with lunar soil.

The bags were opened carefully, almost reverently, not from any religious motivation, but to prevent any scattering of the fine dust. The smell of the lunar surface (Armstrong and Aldrin described it as a burnt smell, like spent pistol caps, and as usual with astronaut observations, I find it an accurate evaluation) pervaded the room, and we proceeded to photograph the scoop from all angles in case any changes occurred in the next few weeks. Except for the dust coating, it seemed to be in remarkably good condition.

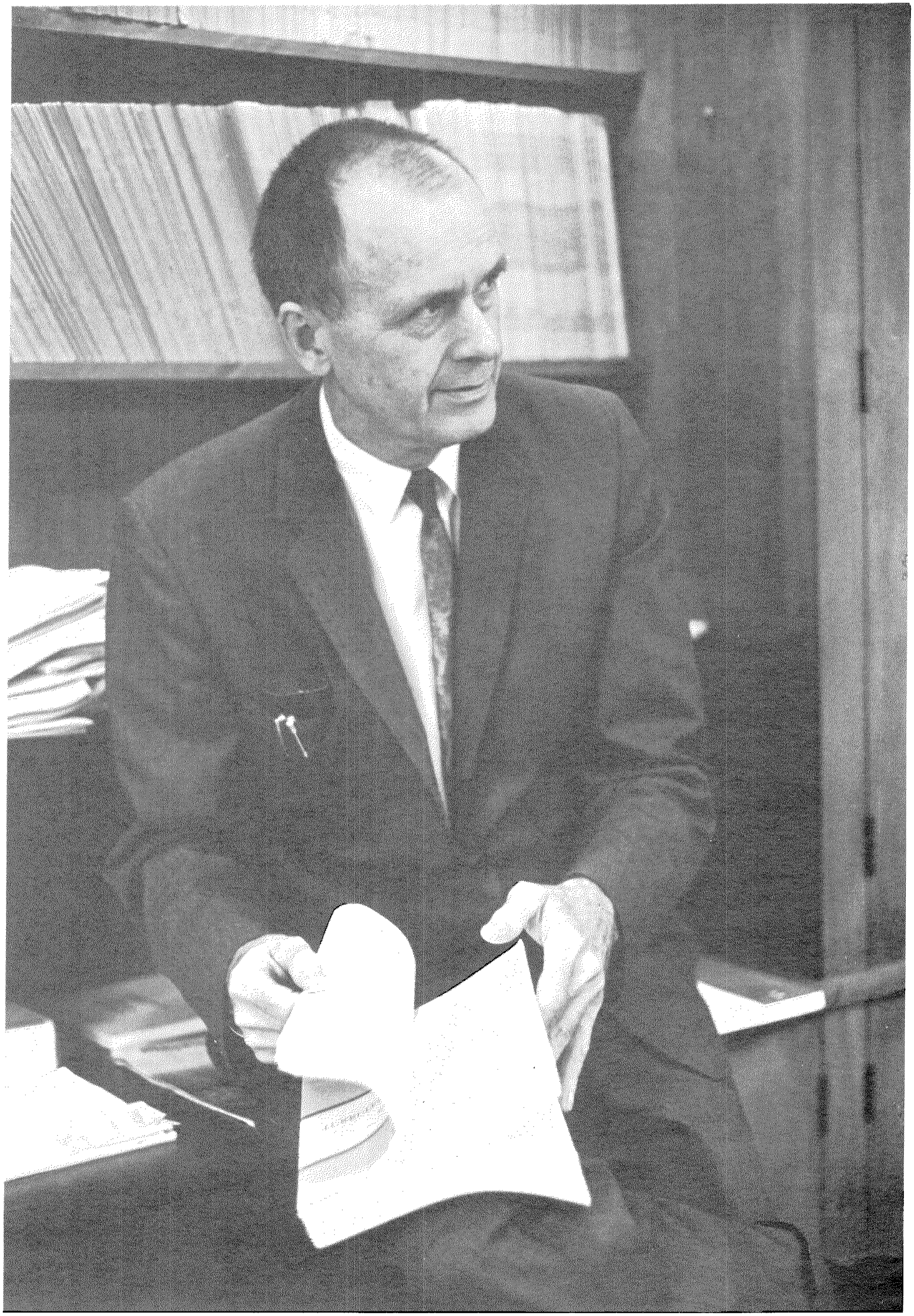
Later, the bags were re-sealed, and the Surveyor parts were transported to Hughes Aircraft Company, the builder of Surveyor spacecraft under the supervision of JPL, for scientific and engineering examinations. Although these studies will take a number of months, I already have one of those inner feelings of satisfaction, which we recognize in life: a circle completed.



Reunited with the surface sampler at last, Scott (third from the left) listens as astronauts Conrad and Bean explain the lucky circumstances that enabled them to bring the device home.



The object of his affection and attention—the surface sampler, covered with lunar dust, but apparently little the worse for its ordeal.



Girding for the Next Decade in Engineering

Meet the new chairman of Caltech's engineering division. All he wants to do is make Caltech the Mecca for the great engineers and applied scientists of the world.

Francis Clauser (pronounced as in Santa Clauser) likes being where the action will be the day after tomorrow. That's one reason he came to Caltech last July as chairman of the division of engineering and applied science. He was convinced then that Caltech was already starting to make the kinds of changes that will dominate the next decade in engineering education. And he is enthusiastic now about his opportunity to further these changes.

Among the ideas he brings to his new job:

- Engineers today must be more science-based, more broadly educated, and more socially conscious than they used to be.
- Many of the difficulties now facing the human race as a result of the destructive effects of modern technology are not the result of engineers doing a poor job, but the outcome of a lack of perspective, responsibility, and ethical concern for the projects on which they work.
- The seventies will find engineers spending more of their undergraduate time in the humanities and social sciences as a part of their professional training. As they gain a broader sense of the world around them and identify more clearly with the thinking and needs of society, they may learn to set appropriate professional goals.
- Although undergraduates already complain about the lack of opportunity to learn a specific discipline, specialization is probably going to have to be delayed until the graduate years.
- A vast number of exciting developments are taking place in modern industry, and our classroom and research laboratories need the stimulus of hearing about them. To strengthen the ties between industry and engineering

"I cannot tell you apart," Von Karman said. "But one thing I do know; two students, two dissertations. Each of you can do one, one of you can do both, or both of you can do both, but when we are through—two dissertations!"

education at all levels, outstanding men from industry will be invited to teach at Caltech for limited periods.

Clauser first came to Caltech in 1932 when he and his identical twin, Milton, transferred from Kansas City Junior College as undergraduates studying physics. It was the time of Millikan, Einstein, Morgan, Pauling, Von Karman, Clark Millikan, and Bateman. "The environment," he recalls, "gave me a feeling of total immersion in a sea of intellectual riches."

As seniors, the Clausers were members of William Smythe's famous class in electromagnetic theory—which that year also included first-year graduate students William Fowler, Simon Ramo, Dean Wooldridge, and John Pierce. Another classmate was Carl Overhage, later director of the Lincoln Laboratories at MIT—a post that Milton Clauser now holds. (Smythe's carefully preserved grade book reveals that Fowler led this class of outstanding physics students, except for the third term, when Francis Clauser tied with Wooldridge to nose him out.)

During their senior year Smythe suggested to the Clausers that they apply for graduate fellowships in physics. "But we made two wrong assessments," Francis admits. "First, we decided our only future in physics lay in teaching; and second, we decided we didn't want to teach. History has proved us wrong on both counts. Having made those decisions, we looked around for something with more action where we could use what we'd been learning. Aeronautics was then, as now, strongly based in science, so it was our top choice."

The Clausers applied for fellowships in aeronautics and waited—and waited. Von Karman's aversion to paperwork kept their applications buried on his desk for such a long time that, in desperation, they went to see him in person. Unfortunately, they couldn't understand the Hungarian accent cascading out around his big black cigar so they left the interview not knowing whether they had been accepted or not. They *had*, or course—and before long they even learned to understand Von Karmanese, the most important language in their lives for the next three years.

Von Karman, in turn, had his own initial difficulty with the twins. "I cannot tell you apart," he announced. "But one thing I do know; two students, two dissertations. Each of you can do one, one of you can do both, or both of you can do both, but when we are through—two dissertations!"

The two dissertations included an experimental one on turbulent boundary layers, coming out of work with Von Karman and Millikan, and a theoretical one on compressible flow, inspired by discussions with Von Karman and Bateman, the applied mathematician who was one of the world's few experts at the time on compressible flows. Even then Francis Clauser was ahead of his time. Few people studied compressible fluid flow until much later.

When Clauser left Caltech with his PhD in 1937, he went to work for Douglas Aircraft in Santa Monica. He took with him, as his bride, Caltech's humanities librarian, Catharine McMillan.

At Douglas he worked on aerodynamics and flight testing. When World War II began, he had put together a design research section which had powerful impact on the eventual course of aircraft design in the United States. It included several men who are now at Caltech with Clauser: Paco Lagerstrom, professor of applied mathematics; Hans Liepmann, professor of aeronautics; and Milton Plesset, professor of engineering science—who says that "Douglas had the best design research group, and Clauser was the best man."

The group developed new methods of designing airfoils and new aerodynamic shapes for tails, wings, nacelles, and air scoops. It urged Douglas to undertake the design of a supersonic plane, which it did—the X-3.

After the war, when Douglas formed the RAND Corporation, Clauser's design group undertook RAND's first project—the design of a satellite vehicle for the Air Force. This venture, undertaken 11 years before Sputnik, was described by Clauser in 1946, in a paper on the *Preliminary Design of an Experimental World-Circling Space Ship.*

At this point Clauser found his interests turning from industry toward research and education, and he accepted an invitation from Johns Hopkins University to establish a department of aeronautics there. He patterned it after the one at Caltech and, like Caltech's, it became internationally famous.

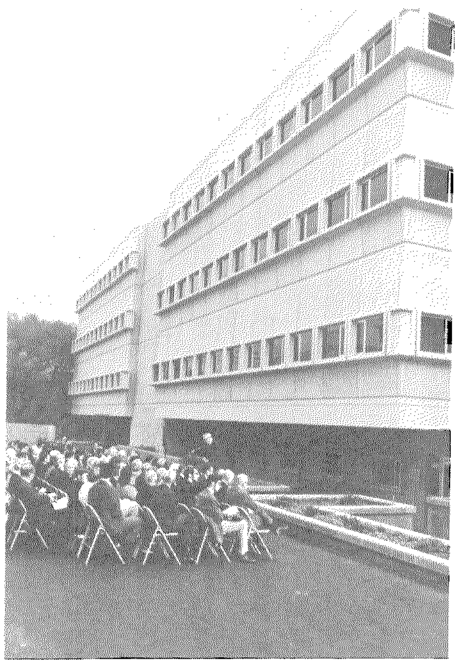
During the next 19 years Clauser became increasingly intent on establishing broad-based undergraduate instruction in science and engineering—master courses to cut across disciplines and delineate scientific principles so basic that, once understood, they could be applied by a student to any specific discipline he might later choose.

His work at Hopkins brought Clauser a wide reputation as a successful educational innovator. In 1964 Dean McHenry, chancellor of the University of California's newest campus, Santa Cruz, asked him to come there and set up a new school of engineering. Even though he did not feel quite ready to leave Johns Hopkins, he did take a leave of absence in January 1965 to lay out a plan for Santa Cruz. By May he was so enthusiastic about the imaginative Santa Cruz arrangement—a full-scale university of up to 27,500 students divided into small colleges of from 500 to 800 students—that he accepted the offer to become the university's vice chancellor for academic affairs.

But the opportunity to return to Caltech last summer to replace Fred Lindvall as chairman of the engineering division and devote full time again to engineering was too exciting to refuse. He calls the current engineering faculty "absolutely first rate," and with the new members he intends to recruit, his goal is "quite simply, to make Caltech the Mecca for the great engineers and applied scientists of the world."

Clauser is as absorbed in the remote past as he is in the future, as evidenced by his deep interest in archeology. A trip through the Near and Middle East in 1960 and 1961 led Francis and Catharine Clauser to a study of ancient history and learning to read and write Egyptian hieroglyphics. Armed with these accomplishments and a lot of enthusiasm, they have visited many of the world's great archeological sites, and they plan to explore even more. Catharine Clauser predicts that it will be quite awhile before they retire to their 14 acres of redwoods at Santa Cruz.

Downs-Lauritsen Laboratory of Physics



The Downs-Lauritsen Laboratory of Physics, dedicated on January 23, relieves an overcrowded condition that has plagued Caltech physicists for years. The new facility consists of two connected buildings—the Charles Lauritsen Laboratory of High Energy Physics, on the east, funded by the Atomic Energy Commission; and the George Downs Laboratory of Physics, funded in part by the estate of Caltech alumnus George Downs and in part by the National Science Foundation.

The Lauritsen building is devoted to research in high-energy or elementary-particle physics. The occupants include six faculty engaged in experimental research and six doing theoretical research. The experimentalists design and construct equipment at Caltech, take it to one of the large accelerators operated by the AEC to run the experiments, then return with the data to Caltech. The very active theoretical staff, as division chairman Carl Anderson explained at the dedication ceremony, uses "equipment consisting of heads, paper, pencils, and wastebaskets—the latter three items provided by the AEC."

The Downs Laboratory houses activities related primarily to space research, sponsored for the most part by NASA. The work includes studies in infrared, gamma-ray, and x-ray astronomy; solar physics; particles and fields in interstellar space; cosmic rays; and other areas of astrophysics. The facility houses eight faculty members. In both laboratories, postdoctoral fellows and graduate students also participate in the research activities.

The dedication ceremony included the tributes excerpted on these pages and remarks by AEC commissioner Theos Thompson and by Lloyd Herwig, staff associate of the NSF. The day's activities concluded with a performance of Bach keyboard music by concert pianist Rosalyn Tureck, the widow of George Downs.

George Downs

Excerpts from a tribute presented at the dedication by Howard Cary

George Downs' interest in and love for Caltech and for science sprang, of course, from his two years' association with the Institute as an undergraduate student. He was here shortly after I was, and I know the excitement that a young man has at the California Institute in his first year or so, when he is introduced to this magnificent structure of human knowledge and he is oriented and becomes aware of the broad horizons of man's inheritance in science and in the humanities.

It's especially thrilling to experience it at an institution like Caltech, where one can rub elbows (in an intimate way) with those who are at the forefront of science. We were very fortunate in my time in having instructors like Ira Bowen, E. C. Watson, E. T. Bell, Clark Millikan, and many others. This is *so* exciting for a young man that it influences the rest of his life. It is very appropriate that George Downs was instrumental in making facilities available for the continuation of this same thing.

George had a very difficult time in his early years because the Depression caught up with him in his second year at Caltech. He spent his period of time digging ditches for the late, lamented Works Progress Administration. Subsequently, he made connections with a very talented mechanical engineer named James Lansing, and very quickly mastered the fundamentals of acoustics. He had carried on intensive self-study, and he made advances in the development of acoustical devices—particularly loudspeakers and amplifiers for the then-budding sound movie business—which were far in advance in performance to anything then available.

But he encountered difficulties because he was a young man and he didn't have the academic credentials that others did. It was at this point that George Downs decided to grow his magnificent beard—at a time when very few beards were to be seen. On the campus at that time all I can recall is the Mephistophelian beard of Professor Martel in civil engineering. And, as nearly as I can recall, outside of Professor Merrill (who was part-time) in aeronautics, those were the only beards on campus. This set George apart and made him remembered by people he met, and he carried this through his life as, perhaps, a trademark.

Although I first became acquainted with George in 1937, it was not until 1946 that George and William Miller and I formed Applied Physics Corporation for the production of scientific instruments. At that point my association became a collaboration, and a very close one that continued for many years. It was a remarkable collaboration for me, and I learned enormously from it.

George did not work like other people. His methods were so totally different that sometimes they were difficult to understand. Primarily, he was the best consultant that I have ever encountered. He was going to do all the good he was going to do for you in the first hour that he encountered a problem. Until that problem changed in some material fashion, he might just as well go away and not come back. But his consulting was perfectly amazing.

I had the experience repeatedly, in the early days of Applied Physics, of struggling with a problem for a week or more, having George come by to make friendly conversation, and then asking me, of course, what I was doing. I would explain the problem I was working on, and then be amazed to have him say, "Well, I think you should do it like this." He would then outline a point of view that was quite radical. Sometimes I would be a little miffed because, supposedly, I was an expert in spectroscopic instrumentation and George did not have much of a background in it. Here he was, after ten minutes of fill-in on a problem, telling me how it could be solved. Then I would have the experience of working for another week and coming to the conclusion that what he had said a week previously was how the problem should be solved.

Now, I'm not making a joke; I had this happen frequently, and I have heard others speak of this also. I can recall Professor Smythe, for example, telling me once of an encounter with George at Point Loma at the Underwater Sound Laboratory, being somewhat astonished by George's instant evaluation of Smythe's proposals on transducers, and being given advice on how the device would work when Smythe was struggling with field equations of the most complex form. George and I often discussed how he did this, how he could reach instantaneous conclusions. There were two or three reasons. One was that he had a very retentive memory. He also had a fine sense of analog—a problem in one area, in his mind, could be instantly translated to a problem in a totally different area. He told me that fundamentally he was an expert in solving one particular equation, namely, the equation of propagation of a wave motion, and that this had so many applications that he very often could find solutions in very strange circumstances.

Our collaboration continued for many years. Not only in engineering matters, but in business matters he lent a sense of judgment, of integrity, of an insistence on excellence that had a very strong influence on me and a very strong influence on our company.

One of George's outstanding characteristics was his love of people, his desire to associate with people to exchange ideas. He was one of the best conversationalists I've encountered, because he was retentive, interested, and very perceptive.

His many parties, where he functioned as a superb host, are well known. In the later years of his life George carried on simultaneously an astonishing number of activities. He was a director of many companies . . . and simultaneously was active here at Caltech as an associate in engineering, as a director of the Associates, and as a director of the Coleman Chamber Concerts. He was involved in various informal organizations like the Wine and Gluttony Society, the Electronic Club (a group of us oldtimers), and various discussion groups. He was a remarkably versatile man, loved and admired by many people. It's a pleasure to see so many of his old friends here to join me in paying tribute to his memory.

Charles Lauritsen

Excerpts from a tribute presented at the dedication by Robert F. Bacher

To give a proper account of Charles Lauritsen's contributions to science, to Caltech, and to our country would require a long recital of his many outstanding accomplishments and the many honors which came to him in acknowledgment. I'm sure that Charlie would not have liked me to do this, and I won't.

Charles Lauritsen was an excellent physicist, a moving spirit at Caltech, and a wise man. He started the work in nuclear physics at the Institute, and as head of the Kellogg Laboratory he created conditions unparalleled for graduate students and young postdoctoral fellows to learn about experimental physics. He had an extraordinary talent for mechanical design and ingenuity in its use in experimental physics. In addition, and most importantly, he had the scientific understanding and insight to recognize problems and devise ways of solving them. He was thus a rare combination, and a great inspiration to his younger colleagues and students.

Charles Lauritsen's contributions to Caltech are many and cover a wide range. He appreciated the importance of theoretical physics and encouraged Robert Oppenheimer, who became his close friend, to spend part of his time here in the period before World War II. Oppenheimer brought along many of his students, and their presence with him had a lasting impact on Caltech.

During the war Charlie directed the Caltech rocket project for the Navy. This project was extraordinarily successful in its rapid development of new ideas in that field. His ingenuity and insight soon made him an expert not only in the technical problems but also in many of the military problems.

After the war his advice on technical questions of great importance to the government was sought by many agencies and departments, and because of his broad understanding he became involved in many important questions of national policy. He recognized the importance of separation of classified research from educational activities and led the way in establishing such policies on this campus. He became deeply interested in arms limitation and control. His understanding of these was based on a strong technical foundation as well as an appreciation of the difficult national and international policy problems involved. He felt that scientists ought to know more about this subject, and he was the moving spirit in establishing at Catlech a seminar on arms limitation and control which ran for many years, and in which he was a principal participant.

Charles Lauritsen's remarkable insight brought solutions to many difficult problems in science and in government. Because of the widespread recognition of that extraordinary talent, his impact on Caltech and on our nation is much more important than is commonly realized. He was a wise man—one of the few wise men it has been my privilege to know well. It was impressive to watch him survey a new problem, using his enormously varied background, and quickly bring to it some new point of view which was significant to its solution.

When I came to Caltech just over 20 years ago, I needed a great deal of help and counsel in beginning to guide the course of the division of physics, mathematics and astronomy. Charlie was always helpful. He was a person of great integrity, whose judgment could be relied upon to be objective. He was completely selfless in his dedication to the principles in which he believed, and he was indefatigable in their support.

Above all, Charlie was a great human being. Nothing made him happier than working with his colleagues and students. He taught easily, directly, and by his own example. No matter what the subject was, his standards were high. But he had his own particular and very relaxed manner in achieving them.

Twenty years ago, several of us were drawing up plans for the electron synchrotron constructed here in Pasadena under the sponsorship of the AEC. A new laboratory was being set up for this purpose in the building which had just been made available after the polishing of the 200-inch mirror for Palomar Mountain. Starting a new laboratory is always a difficult job, and the first work in building that accelerator was greatly helped by both the wise counsel and the active support of Charlie Lauritsen. Work in this area would surely not have been started without his encouragement and help. It is particularly appropriate that this fine new laboratory devoted to the study of high-energy physics should bear his name, And it is also appropriate that the Lauritsen and Downs Laboratories are so closely associated, because Charles Lauritsen and George Downs were friends who admired and respected each other, and both of them loved Caltech.

Research Notes

Old Data, New Answers—I

"Ill-posed" mathematical problems are those in which the answers are so sensitive to the accuracy of the data that they have been considered unsolvable. Any errors in the data are magnified to produce absurd and unrealistic answers. But now a method for solving them has been developed by Joel N. Franklin, professor of applied mathematics, and a whole array of scientific and engineering problems have become amenable to solution.

What Franklin has done is to restate the ill-posed problem so that the correct answer does not depend so heavily on the precision of the data.

One person who has already found the method useful, Don Anderson, director of Caltech's seismological laboratory, explains that one can describe statistically the data errors and plausible solutions and include those estimations in the calculations of a set of unstable equations.

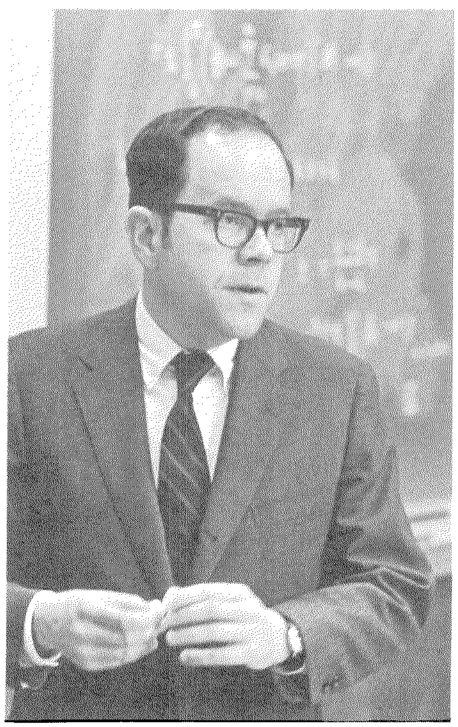
A graduate student working with Anderson, Tom Jordan, says that the data available about the changes of the properties of rocks with depth and increased pressures just don't give enough information to answer questions about the nature of the earth's interior. Franklin's theory lets them introduce new information—namely, educated guesses on what the properties will be.

The principal clues to the nature of the earth's interior are contained in the free oscillations of the earth caused by large earthquakes. If the structure of the earth were known, how it oscillates could be predicted. However, the geophysicists have to work the other way: They have the information about the free oscillations and want to determine the structure.

Their problem is to determine what the composition of the earth would be for it to oscillate as it does. The solution takes the form of a likely mathematical model of the earth, which is then tested in the computer to see if it oscillates the way the real earth does. If the computer shows the same oscillations, then the model is correct. If not, another model is made up and tested.

The computation job is immense. Billions of calculations are involved in what amounts to the solution of a system of linear equations.

The method is an extension of the prediction theory of Norbert Wiener and Norman Levinson. The general definition of ill-posed problems was first given in 1902 by French mathematician Jacques Hadamard, who regarded this class of problems as fundamentally beyond the practical reach of mathematics.



Joel Franklin

Old Data, New Answers—II

The earth's mantle is an 1,800-mile-thick layer of material between the crust and the core that has been thought to be uniform in composition. But, according to Don Anderson, director of Caltech's seismological laboratory, it is now clear that the earth is layered most of the way down, and there are about ten separate layers in the mantle. The mantle layering consists of changes in crystal structures in the rock, with some crystals changing shape two or three times as heat and pressure increase with depth. Also, the mantle has perhaps 20 percent more iron at greater depths than was thought, more silicates with depth, and more magnesium at shallower levels.

Anderson's work, which results from the application of the Franklin method described above, implies that the earth has undergone a series of differentiations. Anderson now thinks that the upper 250 miles of the mantle developed out of the lower part, much as the earth's crust differentiated out of the upper mantle. Anderson also says that mantle temperatures are considerably lower than supposed, ranging from about 1,800 degrees Fahrenheit near the top to 5,400 near the base.

The new work confirms the finding that the upper mantle, about 30 to 90 miles down, seems to be at least partly molten. This is the region believed to supply the magma for volcanoes and to form the "sea" on which huge plates of the crust float. Most earthquakes occur at the boundaries of the plates where they are colliding or pulling apart.

Anderson says the next step is to determine how the mantle's composition varies horizontally, and how it varies under the oceans and continents. Variations down to 250 miles are known, but the region below that is still a mystery.

Anderson is assisted in the work by three graduate students. Thomas Jordan is involved in the over-all mathematics; Charles Sammis is working on atomic theory of solids; and Bruce Julian is examining the velocity of earthquake waves with depth.

Galactic Explosions

What is the origin of the spiral arms of galaxies? And of the peculiar S-shaped pairs of streamers that some galaxies have instead of spiral arms? Also of galactic bridges, the streams of material linking two or more galaxies?

Halton Arp of the Hale Observatories has some answers, based on a study of the unusual celestial objects pictured in his *Atlas of Peculiar Galaxies*. In a recent scientific paper, Arp suggests that titanic explosions have ejected matter from such galaxies. The ejected material leaves trails of gas, dust, and stars; these appear as spiral arms or S-shaped arms in galaxies that are rotating. Occasionally they appear as galactic bridges.

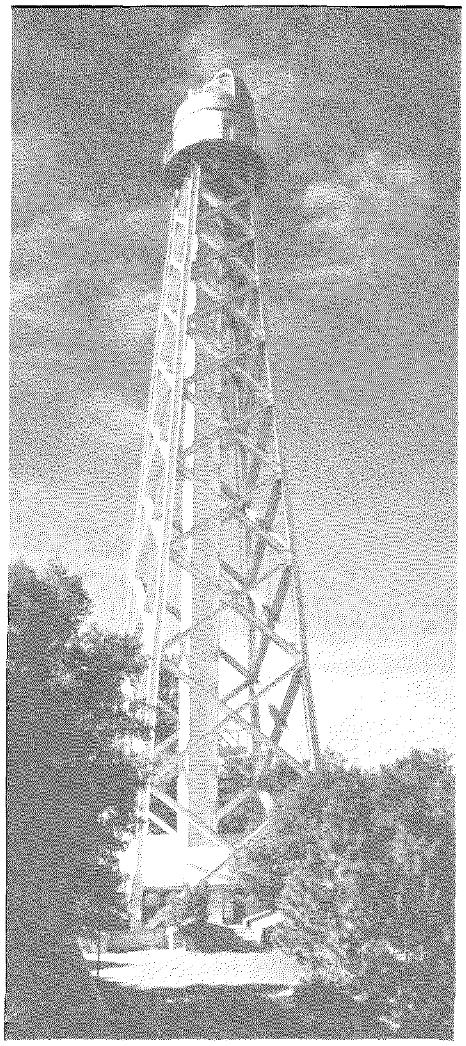
Arp, who earlier suggested that quasars were shot out of galaxies, does not attempt to explain why a galaxy, the largest unit of matter known, would become unstable enough to experience a king-sized explosion. However, an explanation is offered by Fred Hoyle, visiting associate in physics at Caltech and Plumian Professor of Astronomy and Experimental Philosophy at Cambridge University in England. It is possible, he says, that matter and antimatter are somehow generated in equal amounts in the cores of galaxies, with the matter being ejected and the antimatter remaining in the core.

Recoating the Mirror

A delicate rebrightening operation performed on the 200-inch mirror has brought the Hale telescope on Palomar Mountain back up to near-maximum efficiency. A crew directed by Bruce Rule, Caltech's chief engineer for the Hale Observatories, dismantled the 14.5-ton mirror, washed it, then coated it with aluminum only 1/150,000 of an inch in thickness.

The high-temperature coating process was performed in a vacuum chamber at the observatory using 360 tungsten filaments to heat the aluminum until its atoms boiled off and condensed on the cold glass, forming a brilliantly reflective surface.

It was the seventh aluminizing of the 200-inch mirror since it was installed in 1948; the last time the job was performed was in 1960. This latest rebrightening is expected to last for about nine years.



Mt. Wilson's 150-foot solar telescope

Faster Solar Observing

For the last 13 years the Hale Observatories' 150-foot tower telescope at Mt. Wilson has provided astronomers with nearly all the magnetic observations that are made of the sun. The telescope produces regular daytime records of the sun's shifting magnetic fields in the form of maps (magnetograms), and also records in pictures (dopplergrams) the large-scale motions in the solar atmosphere. The instrument scans back and forth across the sun, recording the polarity and measuring the strength of the magnetic fields with a magnetograph. At any one time it can observe an area 1/10,000 of the sun's disk—a square 12,000 miles on a side.

At present it takes about an hour to map the entire solar disk, but a new system will cut this time to 15 to 20 minutes when it goes into operation next fall. A computer and associated equipment to improve the performance and speed of the telescope are now being assembled in the Astro-electronics Laboratory at Caletch under the supervision of Edwin W. Dennison, staff member of the Hale Observatories. The computer will control and run the telescope as well as collect and analyze the data.

The new instrumentation may make it possible to learn whether there is any link between sunspots and possible long-term variations in the rotation of solar gases. The improved telescope also will be better able to measure and record the velocities of the gases that rise and fall in the solar atmosphere.

Magnetic observations of the sun are useful in developing methods of forecasting outbursts of activity on the sun's surface, such as solar flares. The speed and sophistication of the new equipment may well improve these forecasts, with the possible practical application of greater safety factors for astronauts, who can be endangered by high-speed atomic particles while in space.

Funds for the improvements, to cost $210,000, are being supplied by NASA and the Air Force Cambridge Research Laboratory.

The Month at Caltech

Missed Alliance

An idea that warmed the blood of undergraduates and stirred up heated debate among faculty members faded last month when the board of trustees of Immaculate Heart College decided to proceed with its original plan to move the campus of the Catholic liberal arts girls' school from Hollywood to Claremont.

The IHC plans—developed over the past five years—to associate with the Claremont Colleges were interrupted last December by an enthusiastic invitation from some Caltech undergraduates and faculty members to move next to the Institute campus in Pasadena.

In January the *Los Angeles Times* reported that "conversations are under way between Caltech and Immaculate Heart College that could lead to a future alliance." Although no merger between the two schools was ever contemplated, the sale to IHC of some Caltech land on the northwest corner of San Pasqual St. and Wilson Ave. could have facilitated the exchange of students in certain classes, and would have made the girls of IHC more available for social contacts with Caltech students.

But Caltech could not make even this limited kind of commitment without long-term evaluation, and IHC was faced with deadlines relating to federal grants and loans, as well as a pressing construction schedule at Claremont.

Sister Helen Kelley, president of IHC, in reiterating her school's intentions to move to Claremont, said on January 27: "Whether anything beyond the very useful cooperation which now exists between Caltech and IHC would have developed from prolonged discussions is impossible to say. That IHC and Caltech know one another better and for the most part appreciate more one another's goals and procedures can only be considered as mutually advantageous and reason enough to continue to discuss other means of cooperation in the future."

Conference No. 1

"As part of our effort to find ways in which to solve the problems of the nation and the world—particularly those for the creation of which science and technology must bear a substantial responsibility—we plan to hold during 1970 a series of four conferences, each in its way exemplifying an interaction between science and technology on the one hand and human behavior and society on the other."

President Harold Brown made this announcement in his inaugural address last October. Now the first of the four conferences has been set for March 16-18, when the Institute will bring scientists from the U.S. and Canada to the campus to discuss the Biological Bases of Human Behavior.

Robert L. Sinsheimer, chairman of the division of biology, is in charge of the conference. Speakers include:

Jane Lancaster—The Evolution of Human Tool-Using Behavior

Harry F. Harlow—Induced Psychology in Monkeys

David Hamburg—Recent Evidence on the Evolution of Aggressive Behavior

David Koehne—Evolution of Primate DNA

Carleton Gajdusek—Physiological and Psychological Characteristics of Stone-Age Man

Kennedy McWhirter—Socio-genetic Influences on Chromosome Complements

F. R. Sergovich—Population Cytogenetics and Behavior

John Money—Cytogenetic Psychology

I. Michael Lerner—Polygenic Inheritance and Intelligence

Arthur R. Jensen—The Heritability of Intelligence

Irving I. Gottesman—Genetics and Psychopathology

Richard Lewontin—The Nature of Human Variation

Samuel Bogoch—Individual Variability of Nervous System Proteins

Bert LaDu—Genetic Variations in Drug Metabolism and Drug Response

William Nyhan—Human Purine Metabolism and Behavior

R. Luchsinger—Inheritance of Speech and Speech Defect

Barton Childs—The Genetics of Reading Disability: Present and Future

Admission to all sessions is free, and tickets are available from R. L. Sinsheimer in Caltech's division of biology.

Future conferences will cover Technological Change and Population Growth (May 6-9), Technological Change and Human Environment (October 21-23), and Technological Change and Economic Development (December 2-4). Over-all chairman of the conference series is Harrison Brown, professor of geochemistry and of science and government.

Birthday Party

"I thought I was in on—if not in charge of—the wheeling and dealing in astronomy," says Jesse Greenstein, "but three of my former post-docs set up the whole symposium, and I didn't suspect or hear a thing. It got started because I went around feeling sorry for myself, saying that I was suddenly old and that astronomy was therefore dying."

Greenstein, professor of astrophysics, staff member of the Hale Observatories, and executive officer for astronomy at Caltech, was surprised with a symposium on "The Chemical History of the Galaxy," held on the campus on January 12 and 13 in honor of his 60th birthday. The organizers were George Wallerstein, chairman of the astronomy department of the University of Washington; and two staff members of the Hale Observatories—Wallace Sargent, associate professor of astronomy at Caltech, and Leonard Searle.

The symposium guest book was signed by 120 people, about 50 of whom had worked with Greenstein at Caltech. Expenses were covered by a grant from the U.S. Air Force Office of Scientific Research, which has been supporting Greenstein's work on stellar abundance determinations and nucleosynthesis for many years.

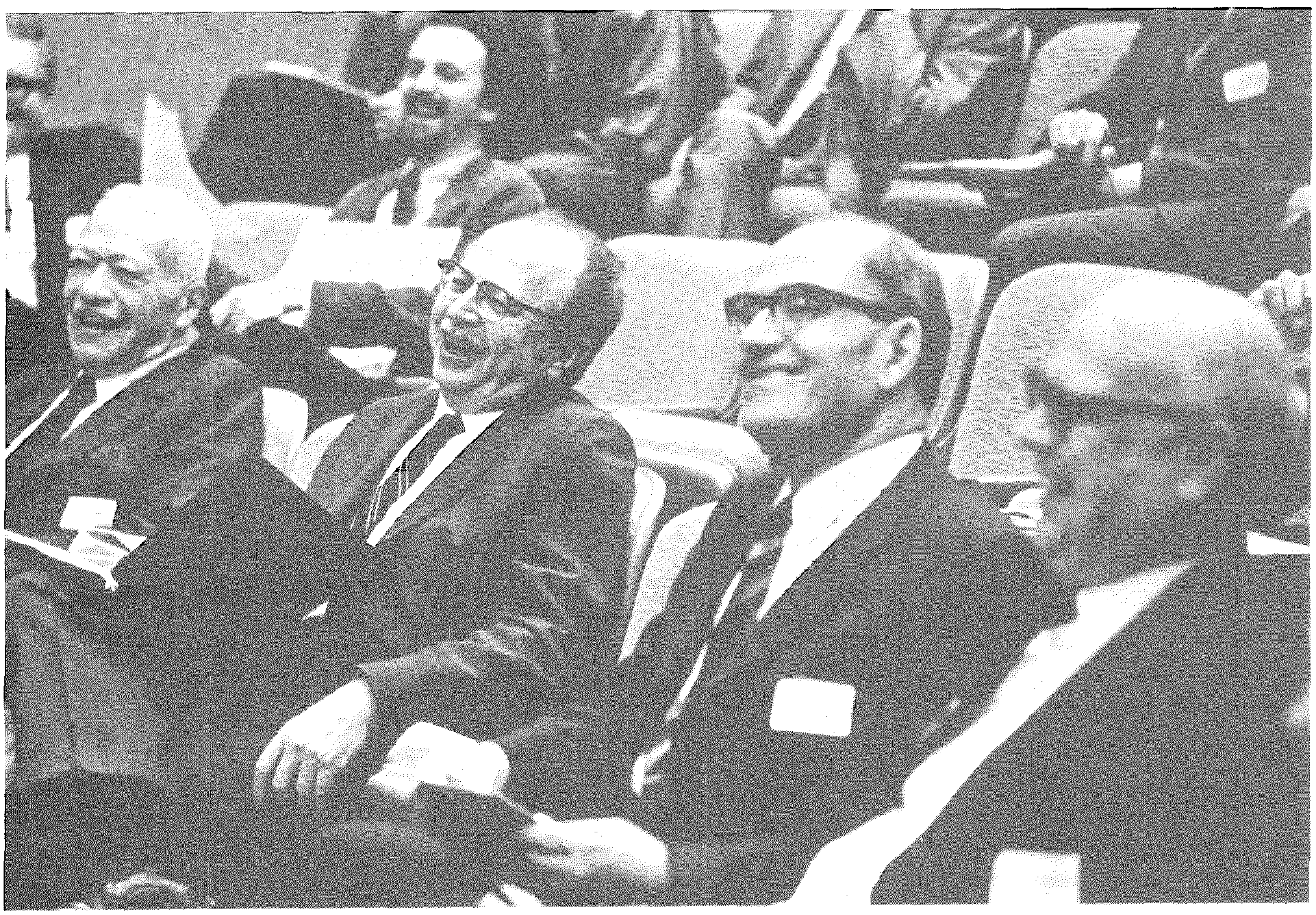
About half of the papers were devoted to the chemical compositions of stars and especially to the significance of astronomical evidence for differences depending on the age of the star and to the nuclear explanation of changes of composition from star to star. The rest of the papers discussed theories of the origin of the chemical elements inside and during explosions of the stars, and made comparisons of the theories with the observed facts about the composition of stars.

One basic issue emerged from the papers and led to some spirited discussion: whether or not the continued nuclear reactions in stars have played a large part in the production of chemical elements. The view of most Pasadena astronomers and physicists, described by Greenstein as the "Caltech dogma," is that stars differ substantially in their chemical composition, and this reflects large amounts of activity in still older stars. In opposition, A. Unsold of Kiel University (Germany) pointed out how amazingly alike most of the stars are in our own and other galaxies, which he interprets to mean that perhaps all the chemical elements were made in violent explosions at essentially the same time—at the beginning of star formation.

"Geochemical Clues to Nucleosynthesis," a paper by Gerald Wasserburg, professor of geology and geophysics at Caltech, included pictures of the Apollo 11 moon rocks and studies of their composition and age. The high accuracy attainable when one can actually handle the material prompted William Fowler, professor of physics and spiritual father of nucleosynthesis theory, to note wryly that in the 22 years he and Greenstein have worked together at Caltech they have collaborated on only two papers—and Wasserburg's report convinced him that both papers were wrong.

Greenstein says that the symposium left him with the general feeling that 60 is not too old, that the study of nucleosynthesis in relation to the evolution of the Galaxy is still a lively subject, and that almost all of the major problems are still unsolved.

A most happy fellow at the symposium in honor of his 60th birthday is Jesse Greenstein, flanked on his right by Rudolph Minkowski, retired staff member of the Hale Observatories, and on his left by Albrecht Unsold of the University of Kiel and by William Fowler of Caltech.



Nobel Dinner

At an Athenaeum dinner on January 26, the Caltech faculty honored its two 1969 Nobel Prizewinners—Max Delbruck, professor of biology, who shared the prize in physiology and medicine; and Murray Gell-Mann, Robert Andrews Millikan Professor of Theoretical Physics, who received the prize in physics.

The prizewinners, just back from Sweden, were welcomed and congratulated formally by President Harold Brown and by Robert Christy, chairman of the faculty.

"The unique luster of a Nobel Prize," said Christy, "illuminates its surroundings in a reflected radiance that multiplies it many times. Each institution that has had any association with a Nobel Prizewinner claims him. Thus Murray is no doubt claimed by Yale, MIT, Chicago, and Caltech—not to mention Churchill College, Cambridge, and the Institute for Advanced Study in Princeton, where he spent sabbaticals. Similarly Max will be claimed by Gottingen, Bristol, Copenhagen, Zurich, the Kaiser Wilhelm Institute, Vanderbilt, and Caltech.

"We are of course particularly happy that Caltech is included in these lists, and that they are here now. We all bask in the reflection of the honors awarded them."

The following adaptation of Max Delbruck's remarks reflects his own opinion about these honors.

I have led a very simple and harmonious life. Ever since my earliest youth, it has been my favorite pastime to impersonate the distinguished lovable old windbag. Now that I *am* a certified distinguished lovable old windbag, I have no difficulty at all in conforming to this role. One of the aspects of this role is that nobody can stop me, however long I talk. So I want to get a few things that I think worthwhile off my chest.

Now, what is this Nobel Prize business all about really? Our dear friend Richard Feynman four years ago gave a marvelous lecture on this subject. And the impression that I came away with from his lecture was that the Nobel Prize is the world's greatest publicity stunt. After all, what does it amount to? By some random selection procedure, you pick out a person, and you make him an object of a personality cult.

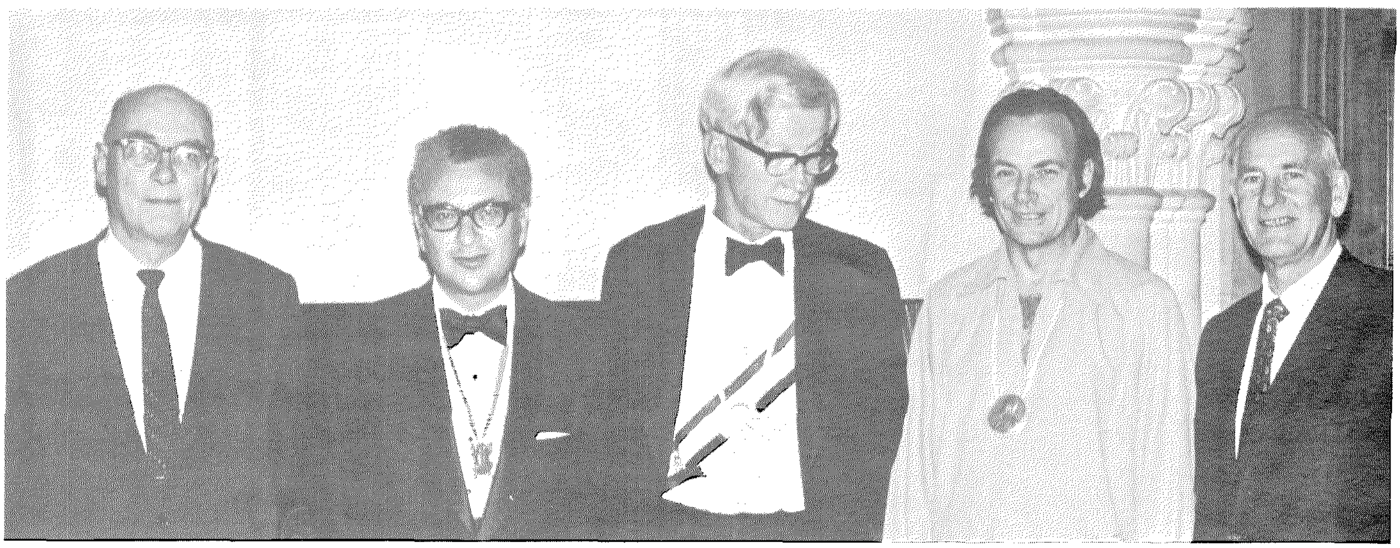
My thesis is—and I think that most of you would agree to it—that there are no geniuses, that all of us, first we are funny young people, and after a while we are funny old people. And in between, each of us is trying to make the best use of the exceedingly limited knowledge we have. Moreover, one of the illusions, which I find especially perplexing, is that even if Mr. X's personality represents a true value within his own ivory tower, it does not follow at all that he has a message for TV audiences.

I want to talk about the reactions of the laureates to this situation.

At the Nobel ceremonies a strange psychological situation developed. Of course it's not only scientists who get the prize; a literature prize is also handed out at this occasion. It is quite rare that scientists are asked to meet with artists and are challenged to match the other's creativeness. Such an experience may well humble the scientist. The medium in which he works does not lend itself to the delight of the listener's ear. When he designs his experiments or executes them with devoted attention to the details, he may say to himself: 'This is my compositon; *this pipette is my clarinet.*' The orchestra may include instruments of the most subtle design. To others, however, his music is as silent as the music of the spheres.

He may say to himself, like Thucydides, 'My story is an everlasting possession, not a prize composition which is heard and forgotten,' but he fools only himself. The books of the great scientists are gathering dust on the shelves of the learned libraries. And rightly so. The scientist addresses an infinitesimal audience of fellow composers. His message is not devoid of universality, but its universality is disembodied and

Joining new Nobel Laureates Gell-Mann and Delbruck (second and third from left) at a dinner in their honor on January 26 were three other Caltech Nobelists—physicists Carl Anderson and Richard Feynman, and trustee George Beadle, former biology division chairman at Caltech and retired president of the University of Chicago.



anonymous. While the artist's communication is linked forever with its original form, that of the scientist is modified, amplified, fused with the ideas and results of others, and melts into the stream of knowledge and ideas which form our culture.

The scientist has in common with the artist only this—that he can find no better retreat from the world than his work, and also no stronger link with the world than his work.

When, at the Nobel ceremonies, scientists are brought together with a writer, the scientists can look back on their lives during which their work addressed a diminutive audience, while the writer (in this case Samuel Beckett) has had the deepest impact on men in all walks of life. We find, however, a strange inversion when we come to talking about our work. While the scientists seem elated to the point of garrulousness at the chance of talking about themselves, like I am doing now, Samuel Beckett, for good and valid reasons, found it necessary to maintain a total silence with respect to himself, his work, and his critics. He did not even turn up. He did not even give an excuse. He sent word that he was unable to attend. He did not say he had a funeral or a dentist appointment; he just didn't turn up. We must realize that he was acting in accordance with the rules laid down by the old witch at the end of a marionette play entitled *The Revenge of Truth.*

"The truth, my children, is that all of us are acting in a marionette comedy. What is important more than anything else in a marionette comedy is keeping the ideas of the author clear. This is the real happiness in life. And now that I have at last come into a marionette play, I will never go out of it again. But you, my fellow actors, keep the ideas of the author clear—yea, drive them to the utmost consequences."

Of course there were many parties in Stockholm, and in this connection I found one thing troubling. While the aspects of publicity and randomness were very obvious to all of us, it was also clear that the Swedes themselves took enormous pride and pleasure in their parties. And they were wonderful parties, just because our hosts took so much pleasure in them. That was one of the great surprises. Even the Royal Family obviously did not consider this a chore, but their big occasion.

So what do you do about that? I found another quote that I want to read, in a story where a young Italian girl and a Danish nobleman are talking about love and about parties. The girl says: "I suppose that even in your country you have parties, balls, and conversazione? (This is taking place in Italy.)

"Yes," he said, "we have those."

"Then you will know," she went on slowly, "that the part of a guest is different from that of a host or hostess, and that people do not want or expect the same things in the two different capacities."

"I think you are right," said Count Augustus.

"Now, God," she said, "when he created Adam and Eve, arranged it so that man takes in these matters (in the matters of love) the part of a guest, and woman that of a hostess. Therefore man takes love lightly, for the honor and dignity of his house is not involved therein. And you can also surely be a guest to many people to whom you would never want to be a host. Now tell me, Count, what does a guest want?"

"I believe," said Augustus when he had thought for a moment, "that if we do, as I think we ought to here, leave out the crude guest, who comes to be regaled, takes what he can get, and goes away, a guest wants first of all to be diverted, to get out of his daily monotony or worry (and we certainly did in Stockholm). Secondly, the decent guest wants to shine, to expand himself, and to impress his own personality upon his surroundings. And thirdly, perhaps, he wants to find some justification for his existence altogether. But since you put it so charmingly, Signorina, please tell me now: What does a hostess want?"

"The hostess," said the young lady, "wants to be thanked."

The Stockholm festivities, at which my fellow laureates and I were entertained with such incomparable grace and splendor, left one thing wanting, which I found disturbing. In some cases it was difficult to identify the hostess to whom I would wish to express my thanks.

Now, this dinner tonight is the last of the parties that are going to be given in this connection. And, again, I find it difficult to define a hostess. So, whom should I thank? It would perhaps seem trivial to say you thank your wife for having been your hostess for the last 30 years, or all women in your life, starting with your mother. But I guess that is what it must be. So, in this sense, I'll conclude.

Alexander Goetz

Alexander Goetz, 72, retired associate professor of physics, died of cancer on January 12 at his home in Altadena. The physicist, who joined the Caltech faculty in 1930 and retired in 1966, specialized in studying how aerosols pollute the atmosphere.

Goetz was educated in his native Germany and was a Rockefeller fellow at Caltech from 1927 to 1930. He held about 40 patents, many for devices for studying microscopic smog particles.

Awards and Appointments

William A. Fowler, professor of physics, has been named winner of the American Physical Society's 1970 Tom W. Bonner Prize for nuclear research in astrophysics. The $1,000 award is for leading and stimulating laboratory studies of the nuclear processes in stars, thus increasing man's understanding of the origin of the elements and of the evolution of stars.

Norman Brooks, professor of civil engineering, is one of the initial 16 members of a Science and Technology Advisory Council established by the California State Assembly to act as an "early warning device" for identifying state problems. The Council, the first of its kind among the states, is expected to concentrate on problems of environment, urban development, criminal justice, and health.

Brooks is noted for engineering activities related to environmental control, particularly in oceans and lakes. He has served Ventura, Los Angeles, Orange, and San Diego Counties as a consultant on problems of ocean outfall facilities for sewage disposal. Last year he was a member of the President's Panel on Oil Spills. At Caltech he is currently coordinating development of a curriculum in environmental engineering science.

Weigle Lectures

The first Jean Weigle Memorial Lecture at Caltech was given February 2 by Herman M. Kalckar, professor of biological chemistry at Harvard Medical School. Kalckar has made extensive studies of biochemical genetics in man and microorganisms with special reference to galactose metabolism. A recent extension of this work correlates diverse aspects of the physiology of bacteria, and this phase of his research was the subject of his Weigle lecture.

Memorial lectureships at both Caltech and the University of Geneva were established by friends and colleagues of Jean Weigle after his death in December 1968. Weigle, who was head of the physics department at Geneva for 17 years, left there after suffering a heart attack in 1948. When he visited Caltech's physics department in 1950, the physicists referred him across the campus to physicist-turned-biologist Max Delbruck, and Weigle became a member of Delbruck's Phage Group. Among his early contributions to their research was the introduction of the possibilities of the electron microscope—which made possible some important discoveries in molecular biology.

The first Weigle Memorial Lecture in Geneva was given last October 27 by Caltech biologist Robert Edgar (now provost of College Number 6 at the University of California at Santa Cruz), who worked closely with Weigle at Caltech for more than ten years. Edgar's topic, "Blueprint for a Virus," summarized the work done in isolating conditionally lethal mutations in defective genes in bacteriophage—work that underlies current research on the molecular basis of heredity. That work, says Edgar, reflected Weigle's guiding influence both in scientific contributions and in elegant and precise research techniques. "He was a bridge," Edgar says, "between Caltech and the University of Geneva. For almost 20 years he fostered a continuous two-way traffic in ideas, people, and scientific accomplishment—a really reciprocal brain-drain."

Movable Deanery

When Robert Huttenback was promoted from master of student houses to dean of students, he had to move his office from centrally located Lloyd House to the first floor of Throop Hall. Once there, he found that about the only time students ever dropped in to see him was when they were summoned—hardly the makings of friendly chats. So Huttenback, in his usual direct way, took the dean's office to the students by setting up a doughnuts, hot chocolate, and good-talk shop in the Winnett Student Center plaza about once a week. Business is booming.

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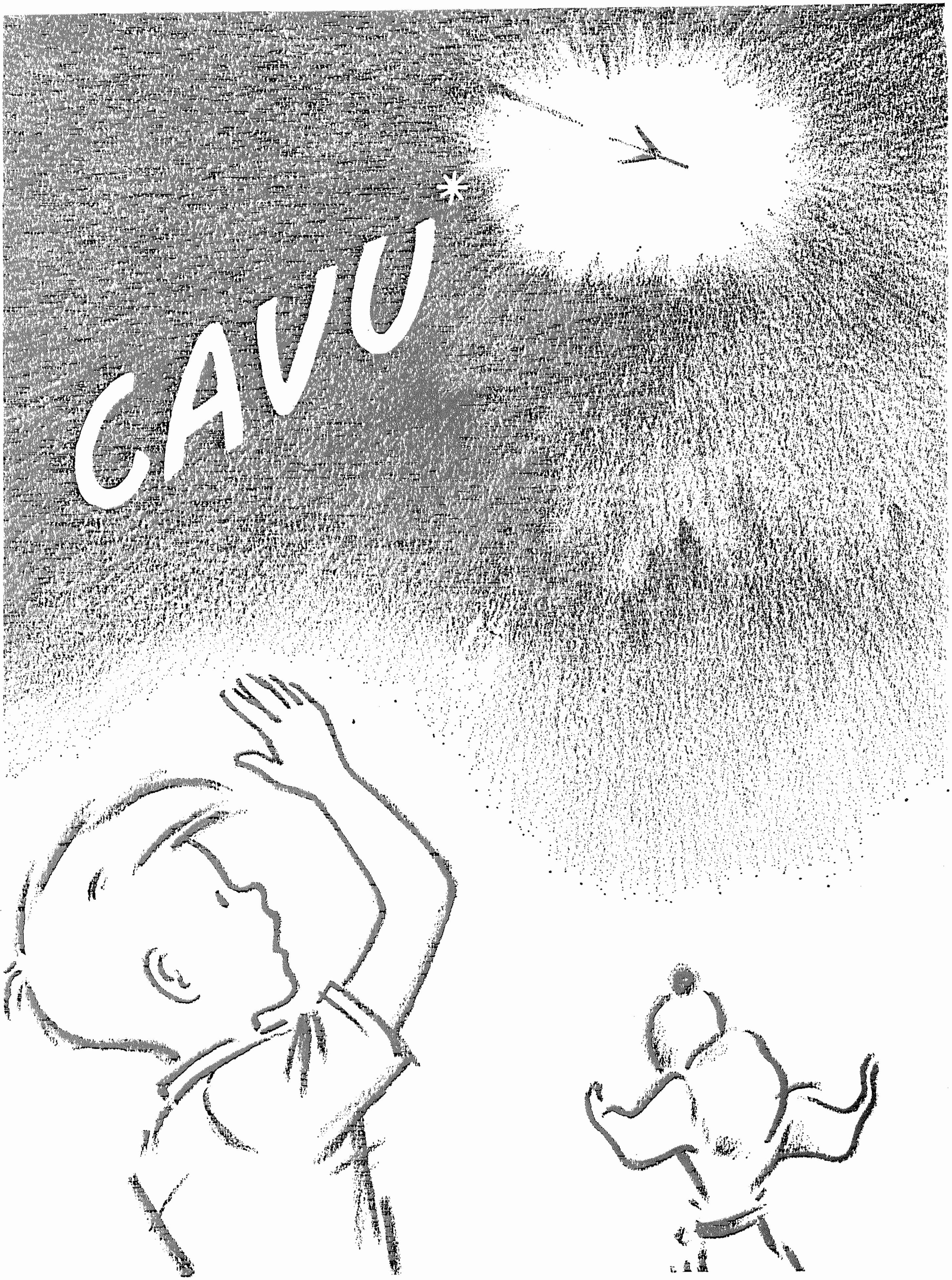
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Sound involved? It is.

Exploring for nickel is complicated and expensive.

To start it fast and efficiently, you *start in the air.*

You dip into your pocket and come up with over $300,000 for a plane you'd feel safe in flying 20,000 miles a year—at an altitude of 500 feet—going 120 mph.

Then into your pocket again for another $200,000. That's what it will *cost you to modify your plane and install* equipment you'll need to locate nickel if it's around.

The bomb is like a microphone. You let it dangle from a 500-foot cable. The sensing devices inside detect mineral deposits on the ground and *relay the information up to the* electronic equipment in the plane.

To be effective, it's got to be close to the grouhd. So be careful. If it gets snagged in an unusually tall tree, you're in trouble.

Of course, once you've acquired all this expensive equipment, you'll want to go to where the nickel is.

One of your best bets would be northern Canada. So dress warmly and bring lots of supplies.

Up there it gets down to 40° below, and you won't find many towns around.

You've got the equipment and you know where to go. Now you'll need men. Well-trained, experienced men.

If you can get five like the ones pictured below, that would be ideal.

From left to right they are: Trevor Blachford, data processor. Randy Dutchburn, navigator. Heikki Limion, group head. Bob Veale, pilot. Paul Wessler, equipment operator.

Bob, Paul and Randy work the plane hunting for the nickel.

When they land, they turn their electronic readings over to Trevor for interpretation.

Heikki, who is a geophysicist, studies the information looking for *indications of possible nickel deposits.*

Eliminate even one of these men and you've hurt your chances of finding nickel.

Well, that's it. Except for one thing.

Once you've found an indication of nickel, your work is just beginning.

You'll have to have experienced men to go in on the ground and examine the area. Then you'll have to send in more men to drill hundreds of holes for your evaluations.

So be patient. This takes a lot of time.

If the findings turn out negative, don't be discouraged. That's how it goes—99 out of 100 times.

But if, with a combination of effort and knowhow (and a nod from Lady Luck), you happen to find a worthwhile *deposit of nickel, there are a few things* you'll need to know: like how to mine it, process it and get it to market.

Before we give you that information, there's a couple of things you'd better check on.

Like putting your hands on a few dozen million dollars and a few thousand workers.

Once you've got these two things worked out, you're ready to tackle the big problems.

Nickel helps other metals resist heat, cold, impact, pressure, abrasion, *corrosion... to advance engineering in* vital fields—power, desalination,

miles at 500 feet in 40° below, cable at 120 miles per hour

electronics, transportation, aerospace.

We're doing everything we can to produce more nickel. Searching around the world—Indonesia, Australia, Guatemala, Canada. We've found ways to extract nickel from ores thought too poor to mine a few years ago.

We count our blessings and respect our surroundings. From nickel ores, we recover platinum, palladium, twelve other commercially useful elements. Make iron pellets for steel. Convert smoke in our stacks to chemicals for other industries. On sand left from processing ore, we grow meadows of hay.

We are explorers. We're in 18 countries. Miners, researchers, market builders. We bring opportunity to underdeveloped lands, new technologies, new payrolls, new tax income. Nickel in the ground is useless. We put it to work.

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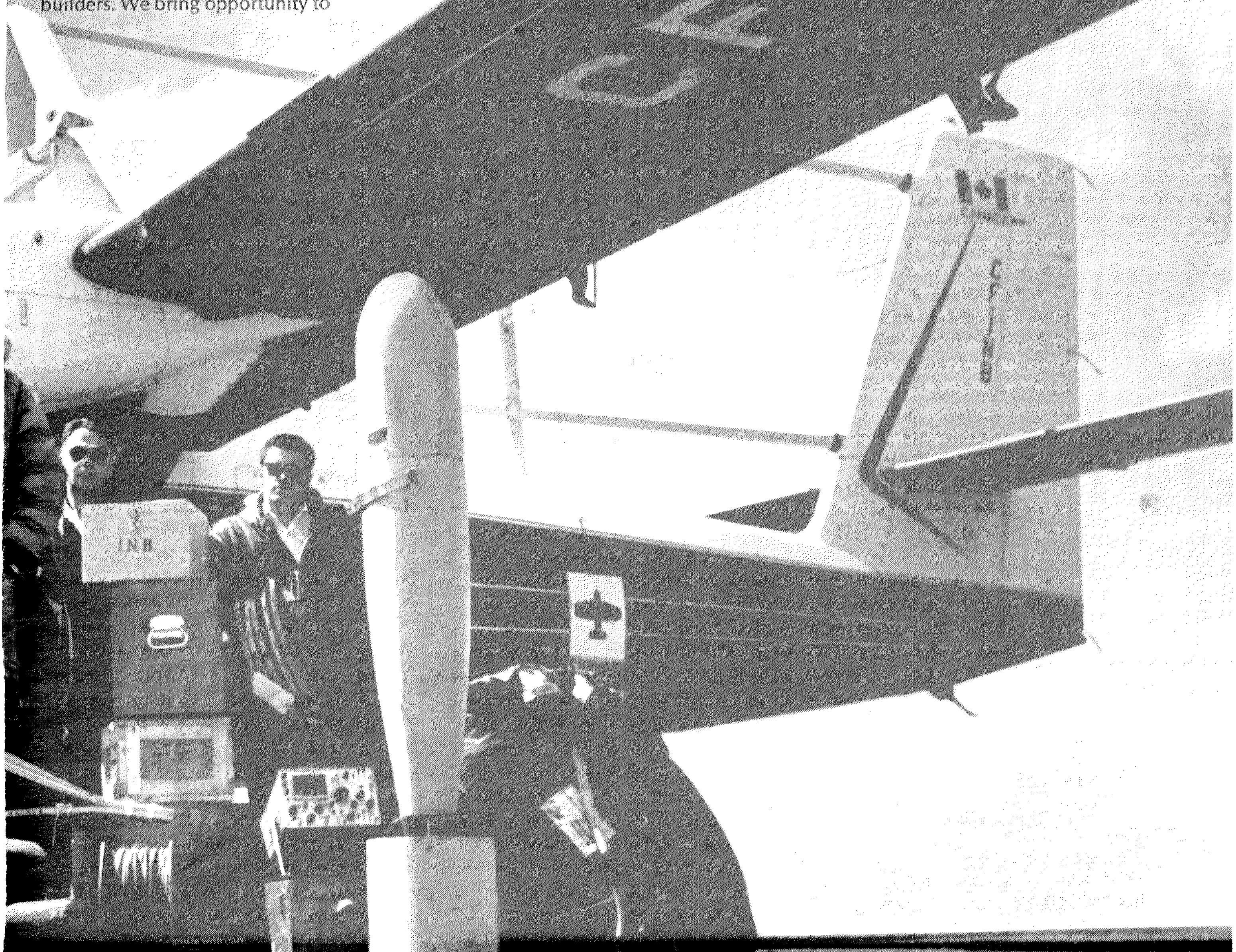

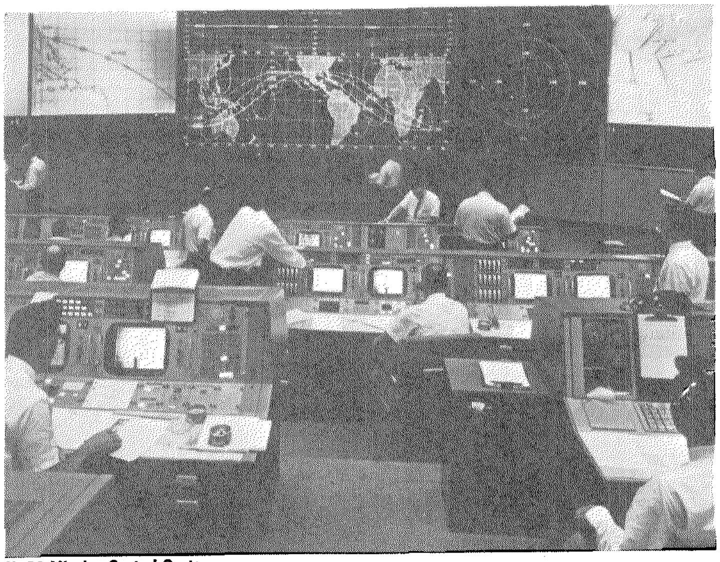

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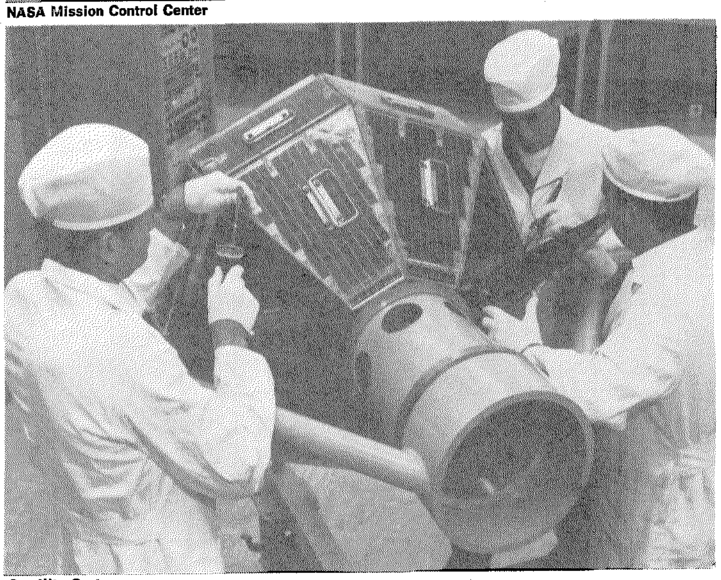

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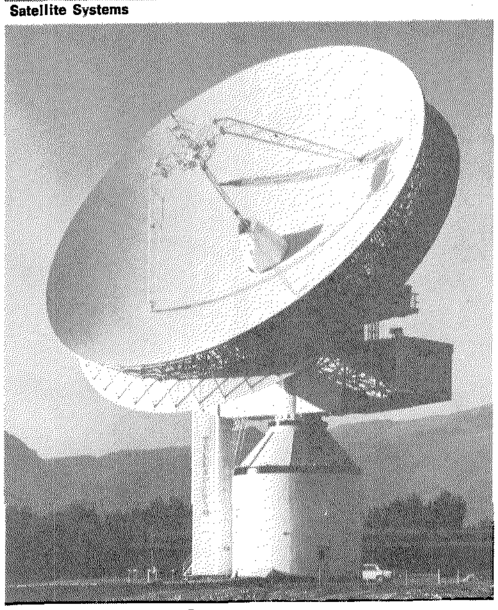

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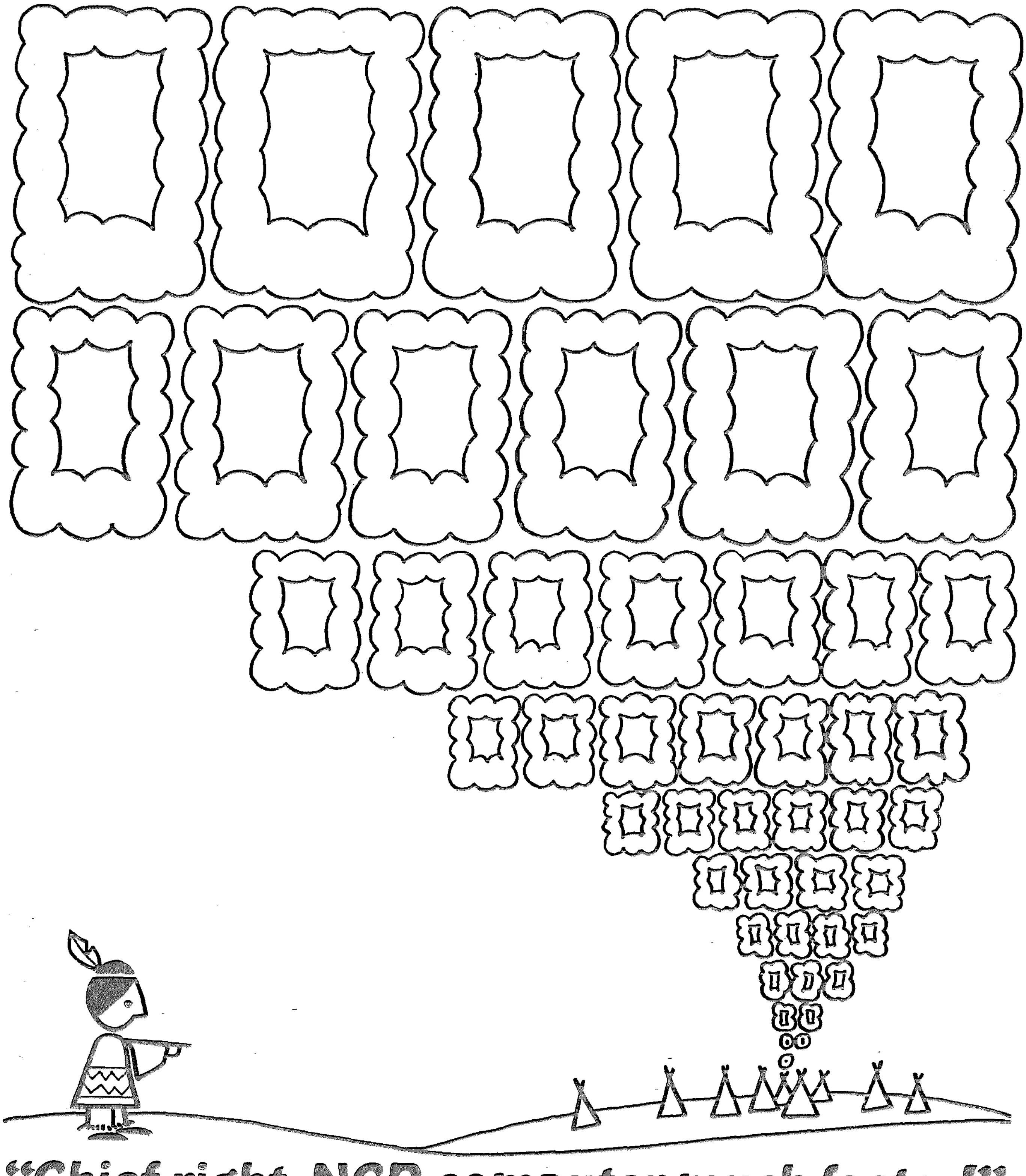

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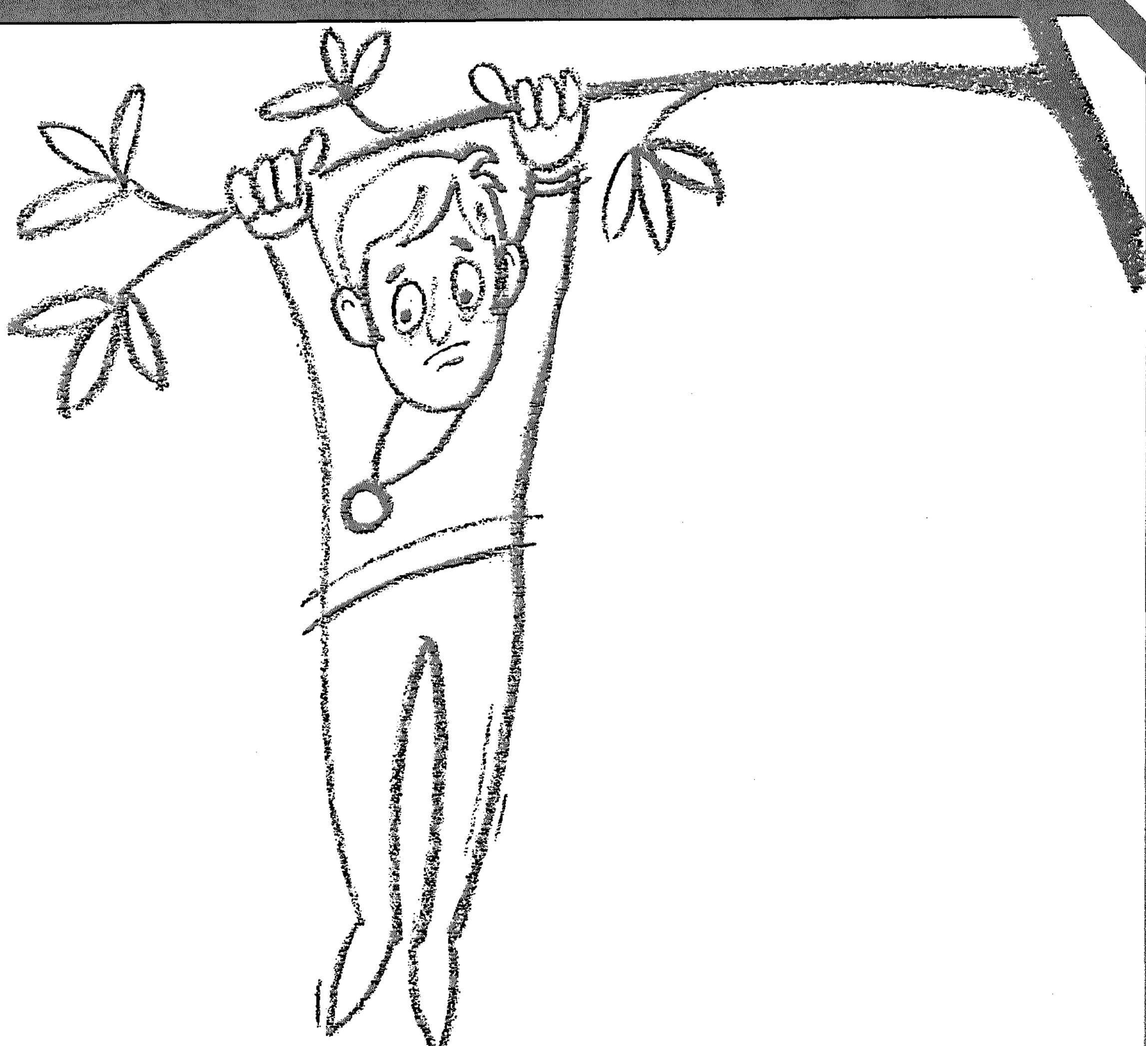

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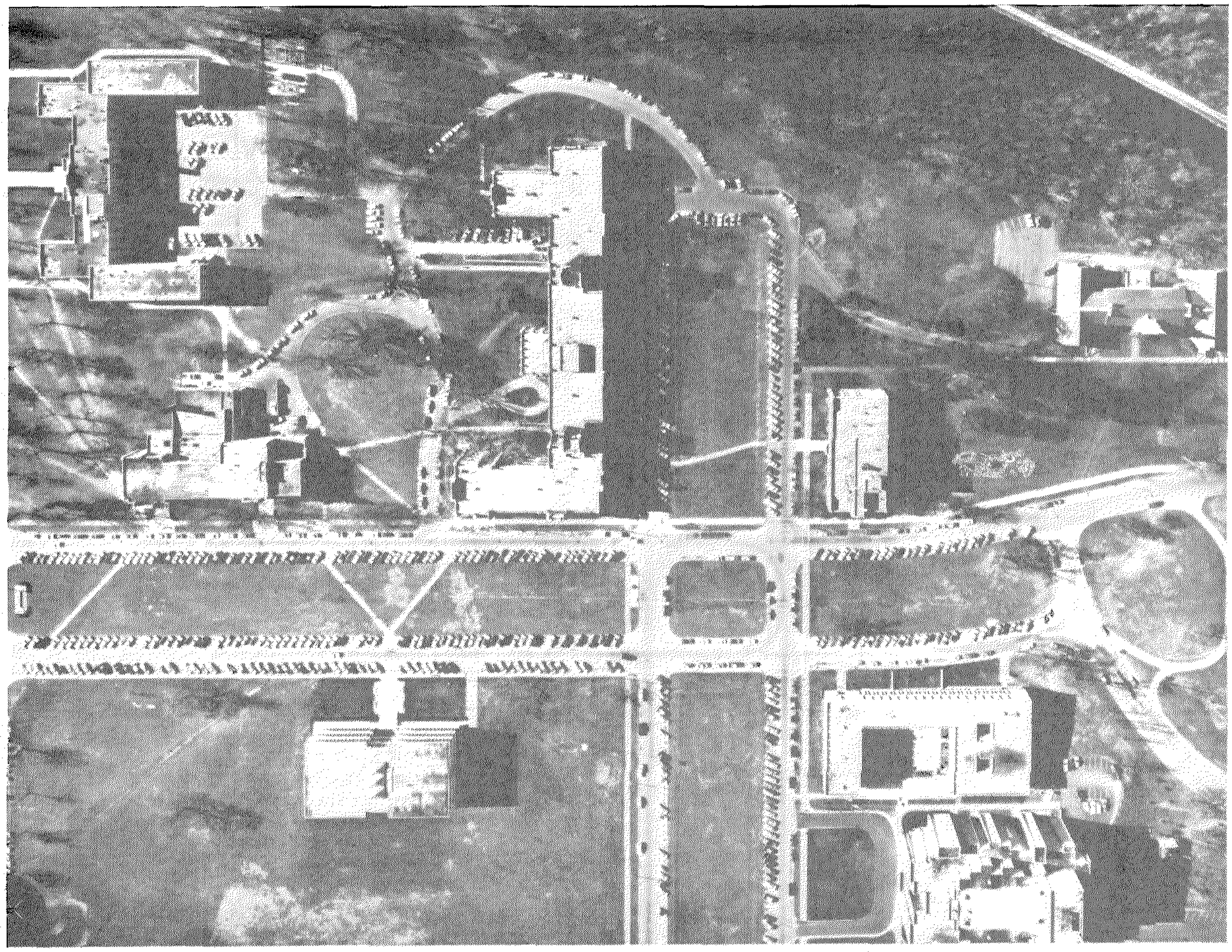

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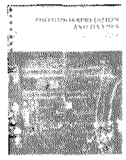

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