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Engineering and Science

MARCH 1970

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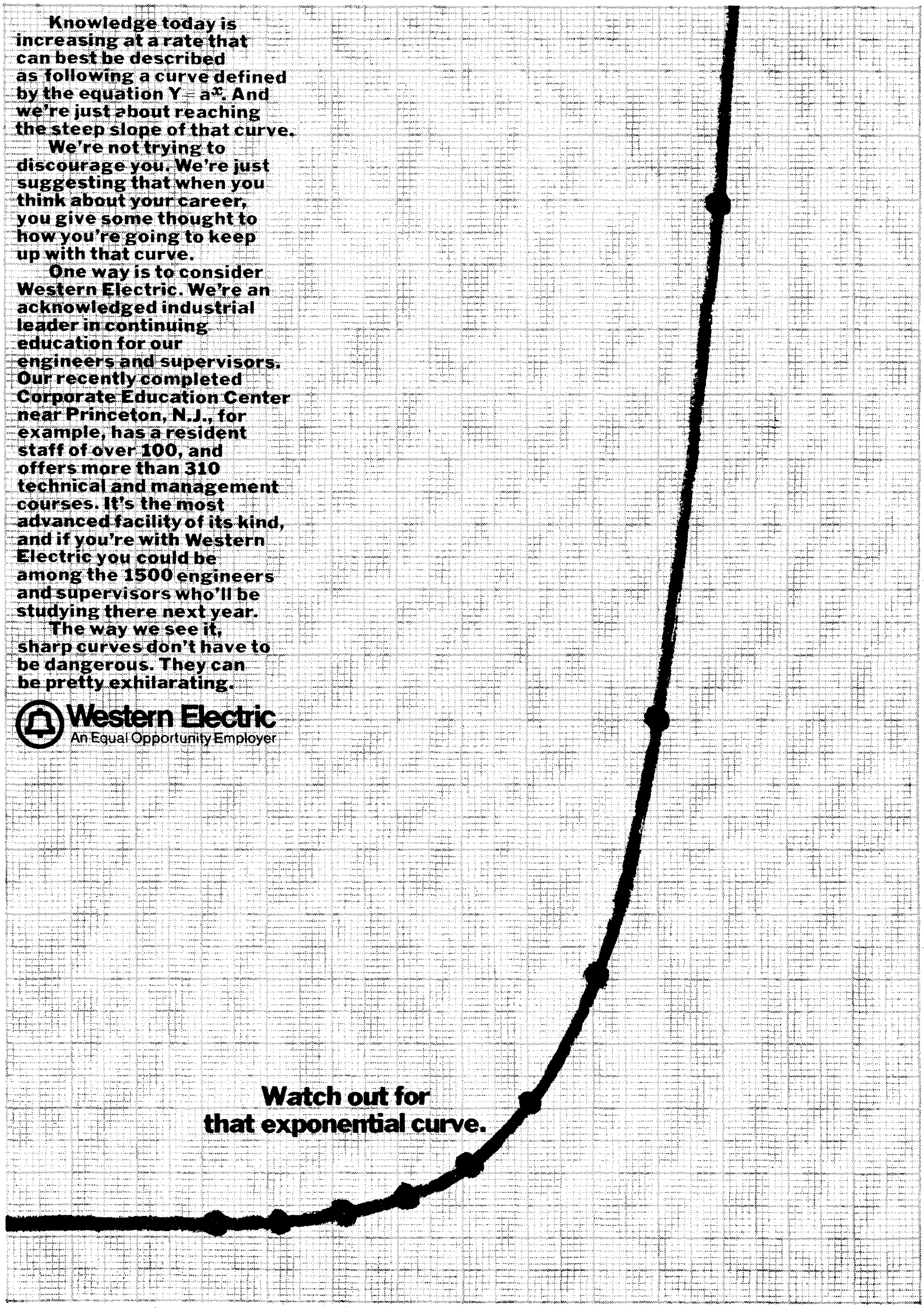
Knowledge today is increasing at a rate that can best be described as following a curve defined by the equation $Y = a^x$. And we're just about reaching the steep slope of that curve.
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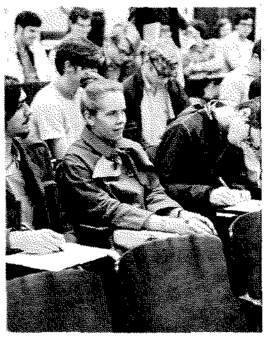

Engineering and Science

In this issue

Resident President

On the cover, Mrs. Harold Brown sits in on a freshman class as part of the Browns' two-day stay in an undergraduate house. Mrs. Brown's reactions, as well as those of a group of their hosts in Dabney House, appear on pages 14-17.

Controlled Fusion

Roy Gould's exposition of the state of the art of power generation by controlled fusion reflects his optimism that it may someday be practical. His article, beginning on page 6, is adapted from a Caltech Lecture Series talk on February 16.

Caltech versus Pollution

Work on problems of pollution seems to have popular support now. Harold Brown's discussion of what contributions a university —particularly one like Caltech—can make is adapted from a speech to the Institute for the Advancement of Engineering on February 28.

Extragalactic Extravaganza

When we approached Caltech radio-astronomer Marshall Cohen last year for an article about a Soviet-American joint investigation of radio sources, he advised us to wait until his co-workers could provide us with some interesting details. Two of them—Caltech alumni Barry Clark and Kenneth Kellerman—*finally* were able to, and their narratives were worth waiting for. The article begins on page 24.

STAFF: *Editor and Bus. Manager*—Edward Hutchings, Jr. *Managing Editor*—Bruce Abell '62/*Associate Editors*— William K. Cassell, Jacquelyn Hershey, Kathleen Marcum, Laurie Spaulding/*Photographer*—Floyd Clark.

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The Octoputer

RCA's many-tentacled computer does time sharing plus regular computing. It's a generation ahead of its major competitor.

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Then came the whirling dervishes of time sharing that let a lot of people work at once.
Now there's a new kind of creature that does time sharing and batch work together. So lots of people can use it—efficiently.
It's the Spectra 70/46. The Octoputer. There's nothing else quite like it on earth or under the sea.
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And it does your big batch jobs in its spare time.
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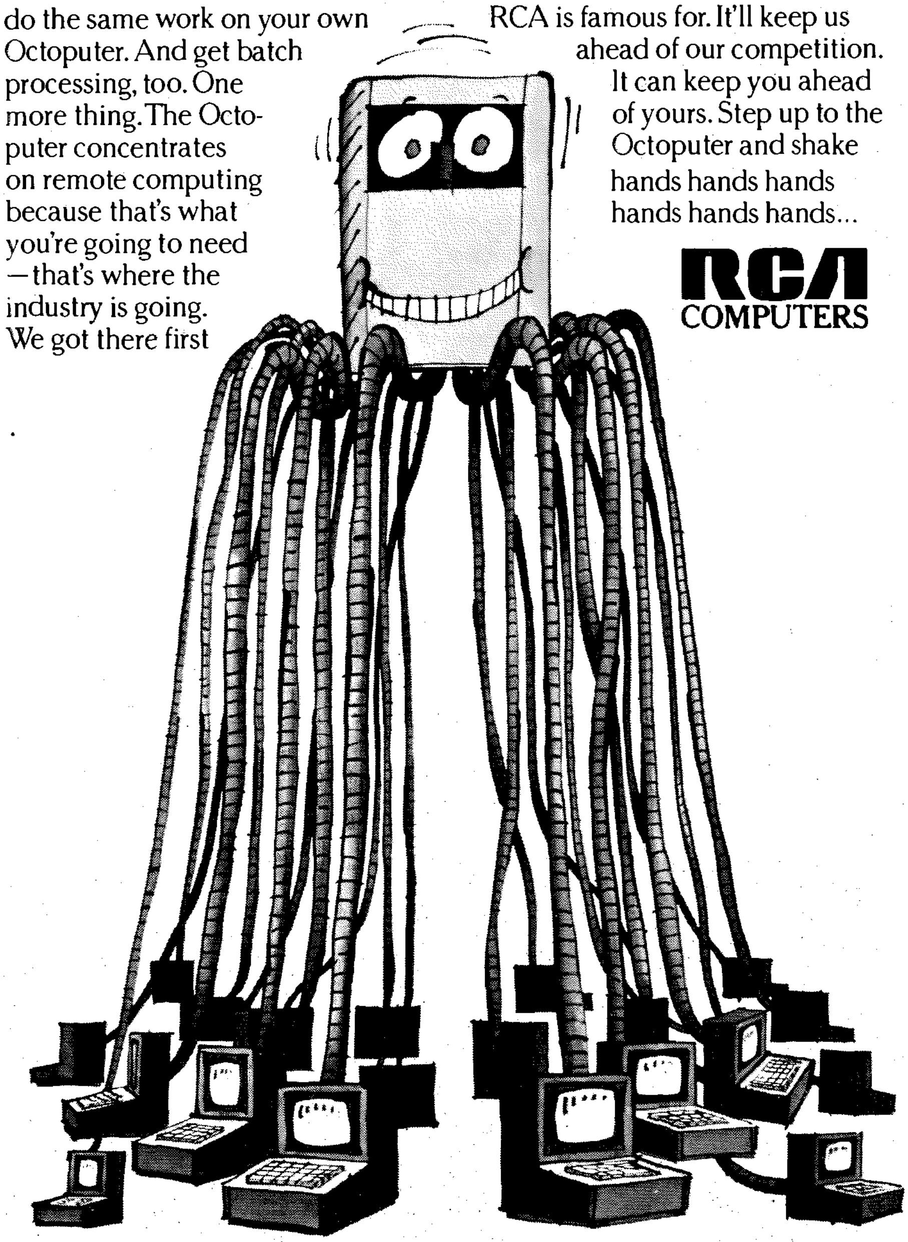
See if it's not more efficient to do the same work on your own Octoputer. And get batch processing, too. One more thing. The Octoputer concentrates on remote computing because that's what you're going to need—that's where the industry is going. We got there first because communications is what RCA is famous for. It'll keep us ahead of our competition. It can keep you ahead of yours. Step up to the Octoputer and shake hands hands hands hands hands hands...



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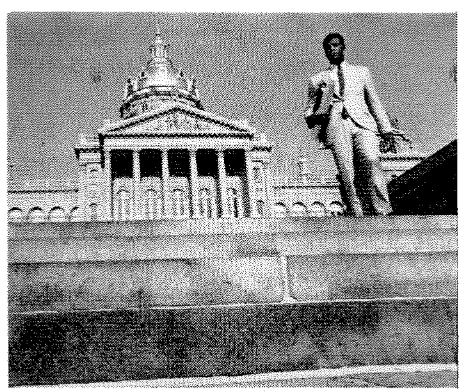
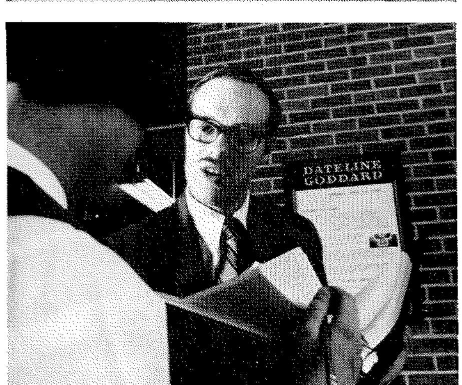
Here's what three recent graduates are doing.

Doug Taylor, B.S. Electronics Engineering '67, is already a senior associate engineer working in large-scale circuit technology. Aided by computer design, Doug is one of a five-man team designing integrated circuits that will go into IBM computers in the 1970's.



Soon after his intensive training course, IBM marketing representative Preston Love, B.S.'66, started helping key Iowa commissioners solve problems. Like how to introduce school kids to computers, without installing one. His answer: share one in Chicago by phone cable.



Soon after his IBM programmer training, John Klayman, B.S. Math '68, began writing programs used by a computer system to schedule every event in the Apollo tracking stations. And when the finished programs were turned over to NASA Goddard Space Flight Center, he was responsible for making them work.

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Controlled Fusion-- Clean, Unlimited Power Generation

by Roy W. Gould

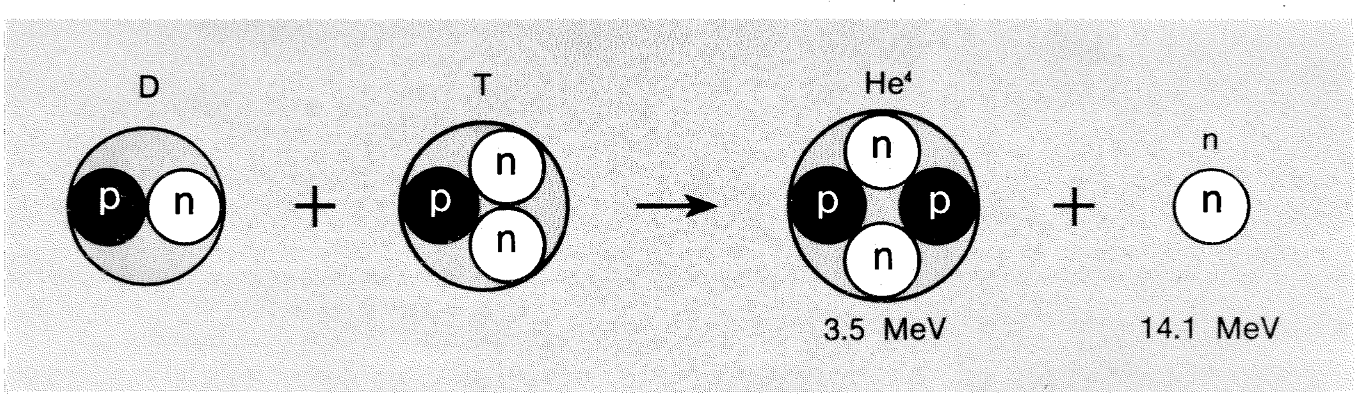
Our sun, like many other stars, produces its energy—the energy that sustains life on earth—from the thermonuclear fusion of nucleon isotopes of hydrogen into helium nuclei, with an accompanying release of energy. Hydrogen bombs, which man has modeled after the stars, derive their destructive force from similar thermonuclear processes. It now seems likely that by the end of this century fusion will play a third key role in our lives—the production of the electrical power that civilization so heavily relies on.

Electrical power consumption in the United States has been doubling and is expected to continue doubling every ten to twelve years. By the year 2000 we will duplicate our electrical generating capacity five times over. Only nuclear power can satisfy this voracious appetite. We must conserve our valuable and limited fossil fuels (coal, gas, and oil) for more important specialized uses in the chemical and steel industries. Increased air pollution from fossil fuel power plants would be intolerable.

Nuclear power from fission reactors already accounts for about 1 percent of the nation's electrical power. Power from fission reactors will certainly increase rapidly in the years ahead. But because fission fuel resources are also limited, development of a fission breeder reactor, which breeds fissionable fuel from more common nonfissionable elements, and the research toward the development of a fusion reactor are now being vigorously pursued in the United States as well as in Western Europe, Russia, and Japan. In the United States, research on controlled thermonuclear reactors is carried out in four major laboratories: the Lawrence Radiation Laboratory (Livermore, California), The Los Alamos Scientific Laboratory, the Oak Ridge National Laboratory, and the Princeton Plasma Physics Laboratory, with smaller but significant programs of varying size in many American universities and industrial laboratories.

Fusion reactors would "burn" deuterium, a heavy isotope of hydrogen which occurs about once in every 7,000 atoms of hydrogen. The energy available from fusion of the deuterium (about ⅛ of a gram) in one gallon of water is equivalent to that of 300 gallons of gasoline. The current cost of extracting this deuterium from the water is only four cents. Deuterium in the oceans would be sufficient to supply the world's energy needs for 10 billion years (at the current rate of consumption). This was the principal argument advanced for beginning work on fusion reactors in the early 1950's.

In a fusion reactor, deuterium (H^2) and tritium (H^3) nuclei come together and fuse into an unstable nucleus which then ejects a very energetic neutron—about 14 million electron volts (eV) of kinetic energy—leaving behind a helium nucleus which also has considerable kinetic energy—about 3½ million eV. The most difficult aspect of this kind of reaction is that it is very hard to get these two particles together in the first place. They are both charged and strongly repel one another. In order to come together and react, these particles must approach one another with kinetic energies of about 10,000 eV. But when they do react, they give back 17 million eV—a very handsome gain. The source of the most difficult problems in a fusion reactor is that the reacting deuterium-tritium (D-T) mixture must be heated to fantastic temperatures—as much as 100 million degrees Centigrade (180 million degrees Fahrenheit). These kinds of temperatures occur



In a fusion reactor, deuterium and tritium nuclei come together and fuse into an unstable nucleus, which then ejects a very energetic neutron—the power source. The problem is to get these two strongly repelling particles together in the first place.

A tamed hydrogen bomb running your electric toothbrush? It's beginning to look possible.

in the interior of the stars, and we provide them in the hydrogen bomb by exploding a fission bomb. At these temperatures, all the atoms have shed their electrons to form an ionized gas, or plasma.

The fusion reaction energy generation rate increases sharply with the temperature of the reacting mixture, and the D-T reaction rate is much more favorable than others for the same temperature. While we can get the necessary deuterium easily from sea water, we must manufacture, or "breed," the necessary tritium. Fortunately, tritium breeding can be carried out in the reactor itself. Eventually, it should be possible to dispense with tritium breeding and burn pure deuterium.

At the very least, the fusion reactions must make up for energy which is radiated away because of the high temperature. For the D-T mixture this happens at 45 million degrees (4,000 eV) and at 400 million degrees (40,000 eV) for the D-D mixture. These are said to be the ignition temperatures. For the reaction also to provide the energy to heat the fuel which is constantly being added as well as provide some useful energy to drive an electrical generator, the operating temperature must exceed the ignition temperature.

Furthermore, the particles must be kept around long enough to undergo fusion reactions. The more dense the mixture, the more likely is a fusion reaction and the shorter the time the particles need be contained.

In a typical fusion reactor the plasma density would be about 10^{15} ions/cubic centimeter, which is 25,000 times less dense than the air we breathe. High-vacuum techniques would be required in a reactor and in all experiments trying to achieve reactor conditions. We are limited to this very low density by the necessity to contain the tremendous pressure (about 10 atmospheres) of the very hot plasma on its container.

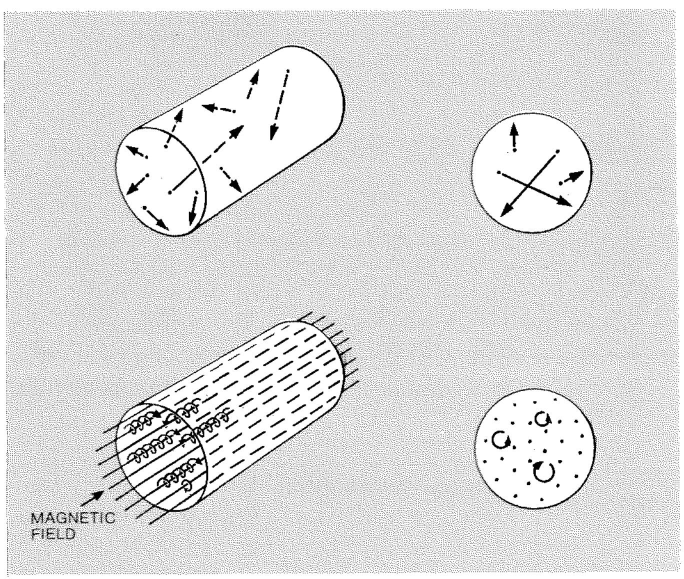
Thus we have to contain the plasma particles for a half second, during which time they can rattle back and forth (at a density corresponding to high-vacuum conditions) in their container, striking the walls about 10,000 times.

No ordinary container can hold the plasma. The reasons are somewhat unexpected. It's not so much what the plasma would do to the walls of the container, but rather what the walls would do to the plasma. The hot plasma would immediately be cooled by contact with the cold walls, with only slight damage to the walls. A few foreign atoms knocked out of the wall by the hot plasma particles (plasma impurities) increase the plasma radiation loss,

Roy Gould, professor of electrical engineering and physics, will spend the next two and one-half years on leave from Caltech to the Atomic Energy Commission in Washington, D.C. As assistant director for controlled thermonuclear research, he will oversee the AEC's growing (nearly $30 million next year) research program on controlled fusion.

contributing to the cooling. In a practical reactor the impurity level in the plasma must be kept very, very low. However, the very high temperature is actually an advantage; as a result of it the particles which have to be contained are charged, and one can therefore contain them with magnetic fields (if you can't beat 'em, join 'em).

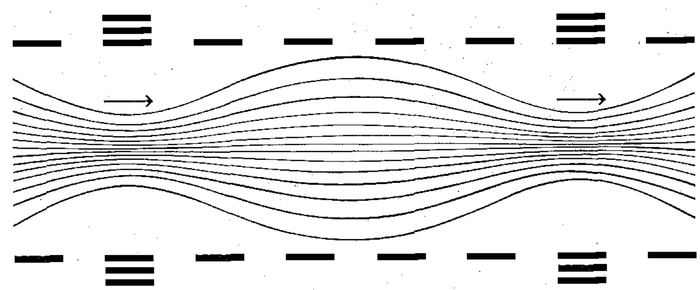
A magnetic field bends the otherwise straight orbits of charged particles into very tight helical orbits. The plasma particles may not move from one field line to another (unless they happen to collide with another particle, in which case they can take up a new helical orbit on a nearby field line). Such collisions are rare in such hot



In the absence of a magnetic field, charged particles move in random directions, striking the walls and allowing cooling of the plasma. The introduction of a magnetic field contains the particles in tight helical orbits and restricts their movement across the magnetic field lines.

plasmas, but do give rise to a very slow leak, called classical diffusion. This sets a fundamental limit to the containment time of charged particles in any magnetic containment scheme. However, the leak due to classical diffusion is slow enough to be tolerable in a fusion reactor (which is good because we probably couldn't do much about it).

The particles, because they are free to move along the magnetic field lines, readily leak out the ends. One solution is to plug the ends. This can be done partially by "squeezing the ends," making the magnetic field much stronger at each end. When a particle moving along a field line

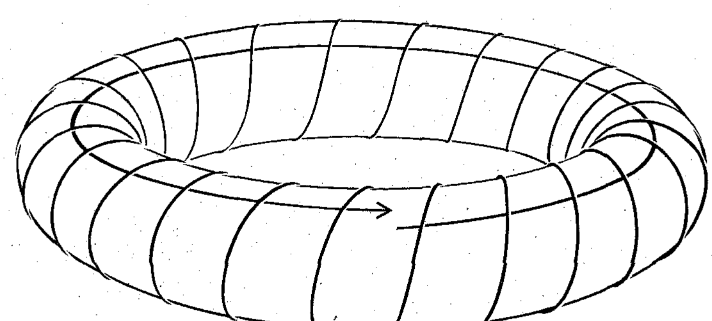


Because the particles are free to move along the magnetic field lines, they readily leak out the ends. One solution is to plug the ends. This can be done partially by making the magnetic field stronger at each end.

encounters the region of increased field strength, it is reflected. But even in these mirror machines certain kinds of particles can still leak out the ends, and the residual end loss of particles from a mirror machine is potentially serious in the "reactor sweepstakes" race.

Another way to eliminate leakage at the ends is to actually eliminate the ends, wrapping the cylindrical container, and also the magnetic field lines, around on themselves in the form of a doughnut (or toroid).

Toroidal, or closed, confinement sounds like a much better solution to the end problem, but it is not as simple as it seems. Whenever magnetic field lines are curved, the magnetic field strength varies. When the magnetic field is not spatially uniform, the charged particles have the bad habit of drifting across the magnetic field and escaping. The loss of ions due to this drift can be eliminated by twisting the magnetic field. A particle trying to follow such a twisted field line drifts away from this field line during the first half-dozen revolutions, but then drifts back toward it during the second half-dozen revolutions. The net effect



Another way to eliminate leakage at the ends is to eliminate the ends, wrapping the cylindrical container and the magnetic field lines around on themselves in the form of a toroid.

If either of the two general approaches worked exactly as suggested, we would probably have a working reactor today.

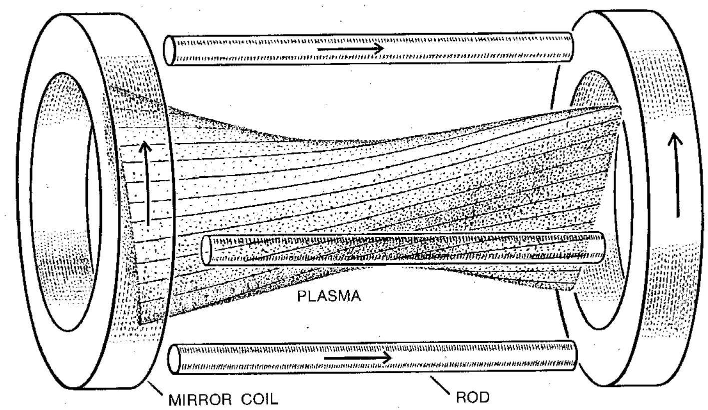
is that it comes back to where it started and doesn't escape. The twist, or rotational transform, is very important and can be provided by some additional coils located outside the plasma—as in the Stellarator type of machine, or by currents in the plasma itself as in the Russian Tokomak.

If either of these two general approaches—using open and closed magnetic field lines—worked exactly as suggested, we would probably have a working reactor today. What, then, is the problem?

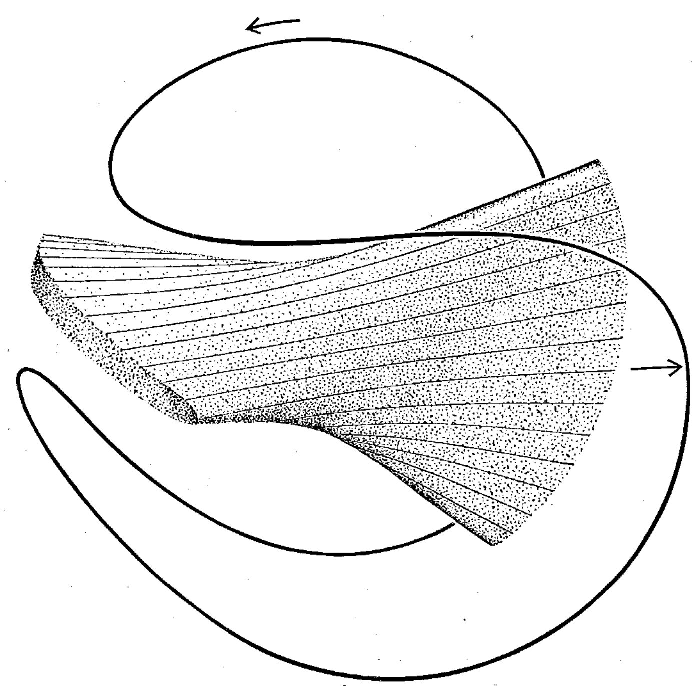
These approaches work beautifully for one or two, or even a few million ions—and this has been demonstrated very nicely by experiment. However, as we try to increase the density of ions, we find that the particles can cooperate with one another to produce substantial electrical currents; hence, they can make their own magnetic and electric fields. Some of these plasma currents and fields are innocuous enough—some are even necessary to the confinement of fusion reactor plasmas. But some are very detrimental.

If the field lines are curved toward the plasma, the magnetic field diminishes in strength away from the plasma, and creates an unstable situation. If a small ripple develops on the plasma boundary, allowing some of the plasma to move from a region of higher magnetic field into a region of lower magnetic field (away from the surface), the plasma finds a reduced containing force, thus allowing the ripple to grow even larger and allowing even more plasma to leave the confined region. If the field lines curve away from the plasma, the magnetic field increases away from the plasma, and a ripple meets an increasing pressure of the increasing field and is forced back. One refers to these situations as having bad curvature and good curvature, respectively. Clearly, one should only seek systems with good curvature.

The idea of good and bad curvature is basic to all containment systems, so let's see how it applies to the mirror machine. The mirror machine has bad curvature in the middle where most of the plasma is located, and has good curvature only at each end. According to this criterion, one would expect the mirror machine to be unstable and to lose its plasma by developing increasing ripples, or flutes, on the surface of the plasma which eventually come in contact with the solid walls of the machine. Indeed, this is exactly what happens, and this



A solution for the instability of the mirror machine was proposed in 1962 by the Russian physicist Ioffe. By adding four extra current-carrying bars, he reduced the instability and provided the necessary good curvature for the plasma.



Another method of achieving good curvature is called Baseball. The connection of the Ioffe bars to the mirror coils at either end results in a single continuous current path—in the form of the seam on a baseball.

The Plasma Physics Division of the American Physical Society once discussed whether it should make an award for the "instability of the year."

flute instability has been nicely documented in mirror machines.

In 1962 a Russian physicist, Ioffe, observed that by adding four extra current-carrying bars the flute instability could be stabilized and the plasma in the center could "see good curvature no matter which way it looked." (Ioffe was awarded the U.S. Atoms for Peace Award for his discovery.)

There is another way (topologically the same) to make this field. It is called Baseball, and there is a single continuous current path—in the form of the seam on a baseball.

The most striking confinement results in magnetic wells—also known as "minimum B" fields—have been obtained in the 2X machine at the Lawrence Radiation Laboratory. Here the ion temperature is already 80 million degrees, and the confinement appears to be limited only by classical scattering. These results are very encouraging.

Toroidal systems, with no "end problems" because they are closed, might be expected to be better than the open systems such as the mirror. The only leak in the simple toroidal system is expected to be from collisions which permit a particle to step from one field line to an adjacent field line—and after many such steps to make its way across the field to a metal surface—the vacuum chamber. These losses due to classical diffusion would be tolerable in a fusion reactor when using a strong magnetic field and large sizes—the containment time could be made many seconds.

However, most plasmas do not behave in this simple way. During World War II theoretical physicist David Bohm was working on the Manhattan Project with another form of plasma—gas discharges in a magnetic field—for the purpose of separating the various isotopes of uranium. It was observed that the ions of these plasmas escaped much faster than could be explained by classical diffusion—and Bohm "invented" a formula to describe this much shorter lifetime—now called the Bohm time. Perhaps Bohm understood where this formula came from—but he never bothered to write down his explanation. Nobody paid much attention to it until the fusion research program started, nearly ten years later. In an effort to understand Bohm's formula, some of the earlier experiments in gas discharges were repeated, and it was found that there was a different explanation for the high loss rates—and Bohm's formula didn't really apply to this situation after all. Nevertheless, there is a growing body of evidence that Bohm's formula really applies to many toroidal confinement experiments—even though the origin of the formula is unclear.

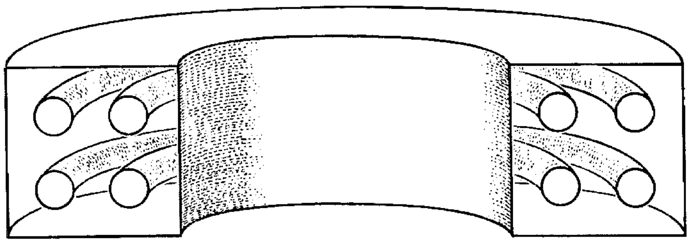
The Bohm containment time is typically a thousand to a million times shorter than the classical containment time; it also decreases with increasing temperature rather than increasing, which would be disastrous for a fusion reactor. The Bohm containment time also does not increase as fast with magnetic field as does the classical containment time.

In recent years it has become quite a challenge to young theorists to derive (and therefore to explain) the Bohm formula. This anomalous diffusion might result from turbulent fluctuations in the plasma. Indeed, with suitable assumptions about the nature of the turbulent fluctuations, one can actually obtain Bohm's formula. The fluctuating electrical fields associated with the plasma turbulence play a role very similar to that of collisions and allow the plasma particles to escape much faster.

The causes of plasma turbulence are only partially understood, but at the heart of the matter is the instability phenomenon. The interchange or flute instability is a relatively simple one—and can now be suppressed. But there are a lot more instabilities where that one came from. In the past ten years the plasma theorists have uncovered literally dozens of new instabilities. The Plasma Physics Division of the American Physical Society once discussed whether it should make an award for the "instability of the year." Fortunately, the rate at which new instabilities are being discovered is now falling off, and many of these new instabilities are variations of the old ones. They can probably be eliminated (or their effects minimized) through the use of the minimum B fields, or fields with good curvature.

It would certainly be desirable if one could find a way to have minimum B or favorable curvature everywhere in a toroidal system. This turns out to be topologically impossible in a closed system—the field lines cannot everywhere curve away from the plasma and still close on themselves.

However, it is possible to have favorable curvature—or minimum B properties—in most regions. It is also important for the favorable curvature regions to be connected by the field lines to the unfavorable regions with



Good curvature is essential to any containment system. In a closed system, it can most readily be achieved by using floating rings, each ring carrying a large current in the same direction.

a short connection length and for the effect of the favorable curvature regions to outweigh the effect of the unfavorable curvature regions. Systems employing this idea are called average minimum B systems. (One might say average favorable curvature.)

The average minimum B property can readily be achieved with closed field lines by using floating rings, which carry the currents that make the magnetic field. Each circular ring inside the toroidal chamber carries a large current in the same direction, and the good curvature regions do predominate.

Although it would probably be impractical to have big metal rings suspended in a reactor, this arrangement has nevertheless led to some very striking advances in containment principles. Floating ring devices do provide an average minimum B field, and turbulent fluctuations are either absent or exist at a low level.

Containment times in some toroidal systems are very considerably increased over the anomalous Bohm value, and some are even believed to exhibit classical containment times. In those devices still exhibiting anomalous losses, the remaining losses are also found to vary with magnetic field in the manner given by Bohm's formula, although the losses are not nearly as large. Depending on the particular machine, the remaining anomalous losses are thought to arise from the supports which must be used to hold the rings, or from electric fields associated with rings which are electromagnetically levitated, or with small deviations in the precise azimuthal symmetry of the magnetic field. The cause of these residual losses is now under careful investigation, and they can probably be reduced still further.

The evidence that containment times which greatly exceed the Bohm containment time could be achieved in toroidal, or closed, systems was first presented in 1968 at the third International Conference on Plasma Physics and Controlled Fusion Research in Novosibirsk, Russia. (The next International Conference will be in 1971 in Madison, Wisconsin.) At the Novosibirsk Conference very significant improvements were reported in several different toroidal devices. The model C Stellarator at the Princeton University Plasma Physics Laboratory and a British Stellarator yielded about 10 Bohm times. Stellarators are heated by inducing a current to flow around the torus, and this current heats the plasma to modest temperatures—about 1 million degrees (or 100 eV). This method ceases to work after the plasma gets hotter. The detailed reasons for the still relatively poor containment in Stellarators are still not well understood, but their twisted magnetic field has only a weak shear and is not average minimum B.

Multipole devices, where the desired minimum average B property is easily achieved by the floating rings, yielded —both in the United States and in England—20 Bohm times.

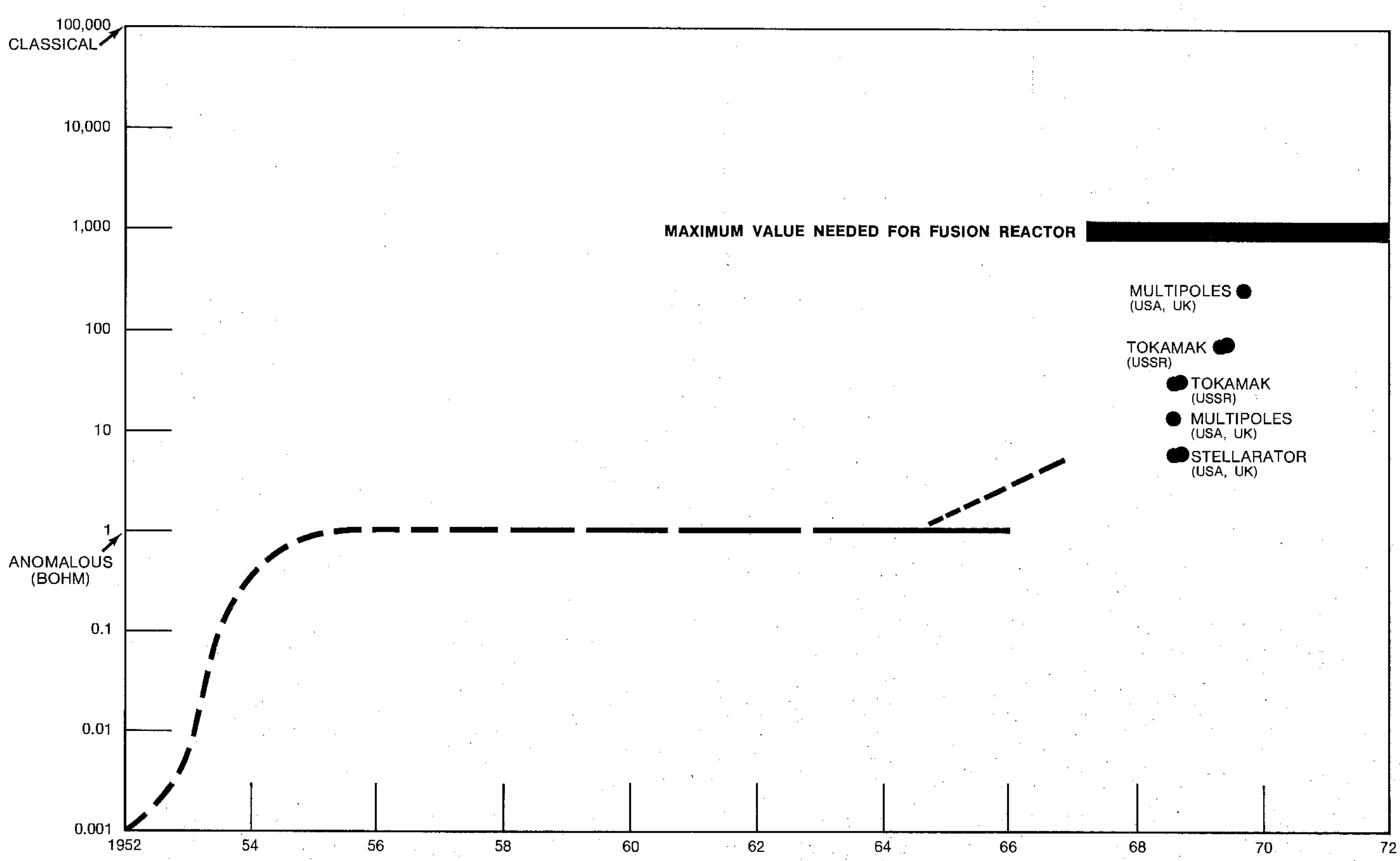
Still better performance was reported (30 Bohm times) from the Russion Tokomak. The Tokomak magnetic field is very similar to that of a Stellarator, except that the twist, or rational transform, is provided by the same current that is used to heat the plasma. In the Stellarator, the rotational transform is provided by currents flowing in coils external to the plasma. Since these currents could be controlled independently of the heating current and the two devices were otherwise quite similar, U.S. scientists felt that it was preferable to carry out their research on containment in Stellarators. They were more flexible. The reasons for the superior performance of the Tokomak are still not understood, and the 1968 results were put forth as tentative. The manner in which some of the experimental measurements were interpreted depends on a careful understanding of exactly what's going on, and the Russians weren't sure. To resolve the major questions in their interpretation of the Tokomak experiments, they needed to know whether there were an excessive number of high-energy electrons associated with the plasma current. They didn't think there were, but confirmation required sophisticated and difficult laser scattering techniques developed only in the past five years. Apparently the

Russians have not developed this measurement technique well enough, because they invited a team of British scientists who were skilled in these measurements and had the necessary giant-pulse laser to do the job. This unique and fruitful cooperative venture, which took about six months, confirmed the original optimistic interpretation—the energetic electrons were not found.

Since that time refinements have led to 100 Bohm times in the Tokomak, and the United States scientists have decided that, because of these striking results, the Tokomak configuration must now be taken very seriously—even though the reasons for its success are not yet understood, and despite the fact that the rotational transform can be changed only when changing the heating current.

Meantime, multipoles have gotten better. The Gulf General Atomic d.c. octopole has yielded containment times of nearly a quarter of a second or 300 Bohm times. During the high-density phases the losses appear to be mainly due to classical diffusion. I must hasten to add that this device is strictly a research device—it is not in the reactor sweepstakes. The plasma temperature is only 50,000 degrees (5 eV) rather than 5,000 eV, and it also has a low plasma density.

Nevertheless, it has provided a most significant advance, and the specter of Bohm is rapidly disappearing. Furthermore, it is now known that it is not even necessary to reach the classical diffusion limit to make a practical toroidal reactor—1,000 Bohm times will easily suffice. That prospect is now in sight.



Evidence that containment times which greatly exceed the Bohm containment time could be achieved in toroidal, or closed, systems was first presented in 1968. Although there have been significant improvements in performance since then, none of the three major testing devices (the Stellarator, Multipole, or Russian Tokomak) have achieved the maximum value needed for a fusion reactor, 1,000 Bohm times. But that prospect is now in sight.

Fusion power plants avoid the possibility of a potential nuclear accident since they are inherently safe against runaway reactions.

Progress on the basic plasma problems has been so substantial in the past few years that people have begun looking toward the other problems which must be faced in building an actual power-producing fusion reactor. Last September the first International Conference on Fusion Reactor Engineering Feasibility was held in England. To the plasma physicist, for whom the plasma problems have been (and still are) a truly uphill battle, the remaining problems appear more straightforward—and maybe they are.

The heat from fusion reactors will probably be used in a conventional thermal cycle—to make steam for turbines which drive electrical generators. This aspect of the fusion reactor design need not concern us too much, since it is common to most power plants and that technology is available. But there are technological problems which would be unique to a fusion reactor—particularly how to get the energy out of the fusion reactor. Since most of the energy of the fusion reaction is in the kinetic energy of the neutrons and the neutrons are not contained by the magnetic field which contains the ions, the plasma will have to be surrounded by a blanket which absorbs neutrons. Of course this blanket must not be in direct contact with the extremely hot plasma.

The neutron blanket serves several very important purposes. It must slow down, or moderate, the fast neutrons—turning their energy to heat which can be carried away to the turbines with a liquid coolant. The blanket also must breed the tritium needed for the plasma from some other plentiful material, until we learn to reach the more demanding conditions for a pure deuterium reactor. Tritium does not occur naturally, but is produced by neutron bombardment of lithium. We not only have the necessary neutrons, but lithium in chemical combination with other substances is also a suitable moderator to slow down the neutrons. Finally, the blanket must reduce the flux of the escaping neutrons to a safe level. On the inside of the blanket is the vacuum wall which faces the extremely hot plasma. It is subject to extremely high thermal stress and the extremely high neutron flux from the plasma. It must be cooled effectively with a coolant that doesn't absorb neutrons and is under high pressure. The neutron flux problem is similar to that encountered in breeder fission reactors, and the materials problems are severe. Since the coolant has to flow through regions of high magnetic field, it cannot be an electrical conductor—the power required to circulate it would be too great. The engineering problems associated with the vacuum wall are probably the most formidable of all and may be the ones which limit ultimate reactor performance.

Outside the blanket are the coils which produce the magnetic field—as high as 100,000 gauss may be required. Originally it was thought that as much as one-third of the fusion reactor's electrical power would be required to make this field by conventional current-carrying coils. The breakthrough in high-field superconducting magnets has completely revolutionized this picture. Superconducting coils are even now being used in several containment experiments.

This gives an idea of some of the great engineering challenges that lie ahead. In addition to technological problems of full-scale fusion reactors, consideration is also being given to environmental and sociological factors expected to affect the competitive position of such energy sources. In the case of fossil-fueled plants, the need to reduce objectionable combustion products to levels acceptable to society could be reflected in increased costs of power from such plants. This problem does not present itself in the case of fusion (or fission) power plants. Fusion power plants will not produce large quantities of radioactive wastes. While the internal structure of a fusion power reactor will become radioactive, the waste products from fusion reactions are nonradioactive. Hence, restrictions imposed by the environmental hazards of radioactive wastes will have little effect on fusion power costs.

Fusion power plants avoid the possibility of a potential nuclear accident since they are inherently safe against "runaway reactions." They contain only as much fuel as they can burn. Thus, they may not suffer from public safety restrictions that could cause increased capital and operating costs, insurance costs, and transmission costs due to limitations on plant locations.

Studies show that, with fusion reactors using D-T as a fuel, the thermal pollution to the environment could be reduced substantially below the values for existing power plants. (Comparable reductions are also projected for fission-reactor plants of the future.) The possibility of other fission fuel cycles and/or of direct conversion of fusion energy to chemical and electromagnetic energy could reduce the thermal pollution problem still further.

PRESIDENT-IN-RESIDENCE

At the invitation of some undergraduates, Harold and Colene Brown made Dabney House their residence for the first two days of March. They moved into the resident associates' suite, while RA's John and Sandra Webb took up temporary refuge in the president's house on the east side of the campus. What happened during the Brown's two-day visit seemed far from what any of the participants had expected, though in retrospect they're glad they did it, as they reveal below.

A Different World
by Colene Brown

Since the students didn't want to structure our visit, I arrived on the scene and had the uncomfortable feeling that they didn't quite know what to do with me. I was assigned to a very nice young man who took me to his classes the first morning. It was a cold, wet day—I had on warm, high boots, and he was barefooted. What a contrast! I almost wished I had left my boots behind.

I went to freshman math and chemistry, where I couldn't begin to understand the material. So I looked at the faces, amazed at how young some of them looked, and tried to imagine what it was like to try and understand what was going on. I wondered whether all of them did.

In the afternoon I went to a biology lab with two older fellows who are working under a grad student on measuring enzymes in the digestive system of a fly. They were full of interest and information on their project which they shared with me, and they showed me all the equipment involved. This was quite a different experience than attending the morning classes. I felt that these students really benefited from this part of Caltech. To me it seemed a relief—even fun—to work on a project with just one or two other people. They seemed so capable and interested in their experiment.

But the students forgot about the second day—what was going to happen to me! I had to ask around to get my day filled—but that didn't prove too difficult. Once I asked, I had more than enough classes to attend. The atmosphere in the house is a little bit creepy for an outsider, and the students don't always make it easy. You just sit around with them. But there I was . . . They invited me, so I thought, "I'm going to use this opportunity if I can."

At breakfast the fellows (who seemed to be different ones than I saw at dinner the night before) didn't even notice me. You just have to approach a table and take a seat next to some poor guy and say something to him. Then, chances are, he's very willing to talk with you. But it does make you feel like going off in a corner. I wonder if the fellows themselves really talk to each other?

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Honored Guests
by the Men of Dabney

When you've never had the president and his wife as house guests, there's nothing to tie to in the way of protocol. We decided we wouldn't structure their visit at all, and now we think maybe this was a mistake. They didn't see as many house members as they possibly could have if we had brought some of them out of the walls. And we didn't think Mrs. Brown would be interested in seeing the whole house, but it turns out she would have been, and we wish she'd spoken up.

When the two days were over, some of us who had arranged the thing were a little let down, but in retrospect I think this is because the visit didn't coincide with our overblown fantasies—we had thought of Dr. Brown hanging around the lounge all afternoon playing bridge with the rest of the trolls. The biggest disappointment was the relatively little time Dr. Brown was able to spend in the house. And of course the first evening was pretty sticky. Everybody gave them—and especially him—the Honored Guest treatment. People who talked to him were pretty stiff. Others hovered around the outskirts, unsure how to approach the Great Man. It was like one of those damn sherry receptions. And of course everybody flattened against the wall when the Browns went in to dinner. You've got to admit that Dr. Brown in the role of Casual Dinner Partner was hard for any of us to take hold of. But things loosened up after dinner that Monday night, and later there must have been ten of us up in their apartment (they had the Webbs') and we talked for several hours. We got the impression that he really cares a lot about improving the Institute and is working hard at it. What we talked about mostly were ways it might be done, and how to take the teaching load off the physics people, and other changes in curriculum.

The Browns went to bed pretty early, and so they missed a lot of how a Caltech house really operates. To know it, you have to wander into a lounge at four in the morning—and maybe six fellows have just decided to go up to Mt. Wilson, the six consisting of three that want to go up there and three that don't want to but have cars.

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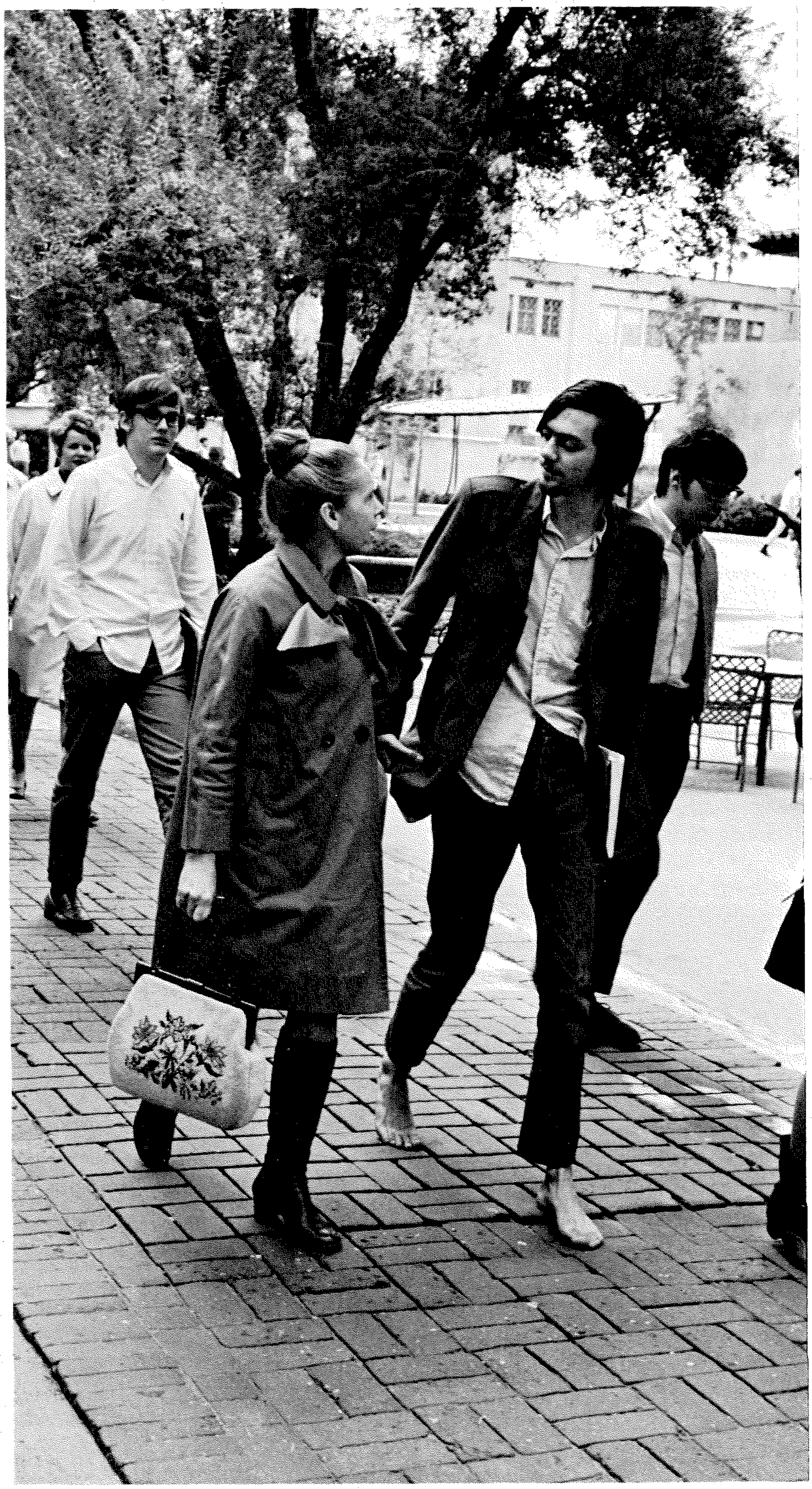


"I was assigned to a very nice young man who took me to his classes the first morning."

Colene Brown . . . *continued*

Well, there isn't much conversation at breakfast anyway. I guess everyone is too sleepy—me included.

I was interested in seeing if these kids wouldn't like to go to something with me. I really wanted to show them off outside the campus, but they weren't interested—mainly because they are too busy and, besides, they'd probably feel that they'd have to put on a tie and jacket.

But they did ask questions about our lives, like "What was it like living in Washington? I'll bet you went to a lot of parties," or "How does Dr. Brown feel about being president after a year here?" and "What does he do, really? What do you do?" But they don't seem to have much conception of what the answers might be. I really felt I was in a different world there, and I think this is what they probably feel themselves.

They missed the boat as far as showing me more. I was left to my own devices, and yet I didn't feel free to snoop around the house very much. I would have liked a tour of the steam tunnels and to have seen where they do their wash. And most of all I would have liked to have been invited in some of the rooms and just talked with the fellows.

They did volunteer some things on their own—the kinds of things that have to be done on the spur of the moment. I feel closer to them for having done that. Now, if I pass them on campus, I make a point of saying hello—even though they may try to look the other way. But, as in Dabney House, if I stop them and say something, they're glad to be recognized.

It was really quiet when we were in Dabney, which surprised me. I couldn't sleep the first night; I could hear various sounds, like kids coming in and playing the piano—and one broke a window. Of course, I think half of them were over at our pool. The second night the house was deadly quiet; I expect they were in our pool again!

We met a freshman who said he's leaving Caltech. It's not that he isn't making his grades—he said he just can't take the atmosphere. I got that feeling myself at being so confined.

I do have a much better understanding of how difficult it is to be a student; I lived through a bit of this myself when I was in college. But I couldn't begin to compare myself with the brightness of these kids; they're *so* good. They have the opportunity to make something of themselves and solve some of the problems of the universe. If anyone can do something, the Caltech student has the potential, and we were impressed with their concern for achieving the best they can.

It was a unique opportunity for these fellows to talk to my husband. They complained because they had only the two days, and only the evenings. But even his family is lucky to get that much time with him. And I know Harold felt the conversations with them will be very, very useful to him.

"It was a unique opportunity for these fellows to talk to my husband."

"You've got to admit that Dr. Brown in the role of Casual Dinner Partner was hard for any of us to take hold of."

"I arrived on the scene, and I had the uncomfortable feeling that here I was and they didn't know what to do with me."

"She is easy to talk to, and even the shyer types had something to say, because she'd just wade in and start talking."

Men of Dabney . . . *continued*

Although we appreciate the many demands on Dr. Brown's time, it would be interesting if they could live in the house for a week and possibly change their sleeping times from—say 8 a.m. to mid-afternoon.

Also, both nights, the Browns missed a swimming party over at their own house, where John and Sandy Webb were staying. The Browns, incidentally, had offered the Webbs and guests the use of the pool, which was, thoughtfully, heated. Those of us that went over there had never really thought much about where the president lived, before.

We were really impressed with all the energy Mrs. Brown had, and how much she entered into. In fact a couple of the fellows, who play a continuing game of classifying people as objects, saw her as a rotary bladed lawn mower. She is easy to talk to, and even the shyer types had something to say, because she'd just wade in and start talking.

Everybody that spent any time with them was impressed with how easy they both were to talk to. And although Dr. Brown couldn't spend too much time in the house, still there was something positive and important in the mystique of having the president around—it would not have been nearly so good, in this respect, if he had just spent an evening.

Mrs. Brown went to classes with several of the fellows, and she looked like she got a lot out of it—the funniest experience was probably in Dino Morelli's class. He had sent word out that everybody better come that day because we'd all done badly on the last test, and he was really going to lay into us—and then here comes Mrs. Brown in and sits down. It blew his mind. He couldn't do anything he'd planned.

Having the president and his wife for even as short a visit as this one was well worthwhile, and we appreciated their obvious interest in student life. Although they only met about 20 of the 50 house members, still this is a pretty good proportion considering everything—because there are some who loiter, and are always in view—and others that some of the more sociable of us hardly ever see.

The University and Environmental Research

The bandwagon of saving the planet from environmental pollution and from suffocation is well occupied now. Indeed, everyone is on it. Being on it is rather like being against sin, and, as in the case of sin, the universal practice is to point the finger at other people. But in the case of pollution few have so far approached the sinners' bench to confess. The universal enthusiasm for preventing environmental pollution and the equally universal reluctance to admit responsibility for it raise a serious question in my mind as to whether the nature and magnitude of the problem are understood.

The concrete actions necessary to limit man's effects on his environment through his own consumption and behavior will strongly affect us all in what is likely to be —at least initially—a very painful way. For we are all involved—not just the engineers who design the automobiles and the manufacturers who make them and the salesmen who sell them and the oil companies that produce the gas to run them, but the consumers who buy them on the basis of large size, rapid acceleration, and all the other characteristics that increase the contribution to environmental pollution.

Who is responsible for solid wastes? Not only the container manufacturers who produce no-deposit, no return, throw-away containers, which neither decay nor rust. Equally—or more—responsible are the consumers, you and I, who buy the products and throw away the containers without making any effort to separate them so that the reclaimable can be reclaimed.

Another example. Americans consume electric power at the rate of about one kilowatt per person 24 hours a day. We are very proud of that consumption rate; we often describe it as having the equivalent of a large number of mechanical servants working for each of us. However, the power is almost entirely produced from fossil fuel. Its consumption thus contributes substantially to the sulphur dioxide contamination of the atmosphere in many places, and contributes about a quarter of the nitrogen oxides released to the air of the Los Angeles Basin.

To a rather good approximation, the three basic factors that multiply together to give a "figure of demerit" for total adverse impact on the environment are:

(1) Total population.
(2) What we have been accustomed to think of as the average standard of living.
(3) A coefficient corresponding to the degree of attention paid to environmental quality in making industrial, agricultural, or consumer decisions.

Recently we have been concentrating on the third of these, but we must remember that it is the enormous growth of the first two factors which has created the danger. Reducing the coefficient cannot save us if we let the first two remain unbounded variables. Specifically, no cure for environmental problems is possible unless the population of the earth, and of the U.S., stops growing.

For example, the stationary power production in the world is a function of total population and power per person. In thermal terms it amounted to about $3x10^{18}$ calories in 1967 in the United States, and about 10^{19} in the world. The sun irradiates the earth with about 10^{24} calories per year, and the earth absorbs perhaps half of it, so the ratio of solar energy to locally developed energy is "only" 1/500 of 1 percent. If earth population doubles by the year 2000, and the rest of the world demands electric power consumption per person equal to what the United States now has, the 1/500 will become 1/50 of 1 percent, still leaving what is probably a safe margin.

But by that time the atmospheric CO_2 content will be 25 percent more than it was in 1900, because the principal power source in the 20th century is fossil fuel. If fossil fuel continues as the principal source of energy in the 21st century, the "greenhouse" effect of the CO_2 could cause a substantial atmospheric temperature rise, with unknown but possibly very serious effects. This assumes that a possible contrary effect—reduction of radiation reaching earth by the increase of particulate matter in by Harold Brown

Being on the bandwagon to save the planet from environmental pollution is like being against sin. As in the case of sin, the universal practice is to point the finger at other people.

the upper atmosphere from combustion—does not take place in a magnitude sufficient to reduce the earth's temperature.

Looking at this kind of problem, we must face up to the fact that when we talk about polluters—the guilty ones in connection with the environmental health question—we mean us, all of us. We would do well to remember this as we seek solutions.

It is probably good that everyone is in on the act now. At Caltech, and at many other universities, the problem attracted attention some time ago. I need only mention the name of Professor Arie Haagen-Smit, who, about 20 years ago, undertook to investigate what produces smog in the Los Angeles Basin. You will recall that at the time everyone knew the smog to be entirely the result of backyard trash burning. Haagen-Smit investigated the chemical constituents of eye-irritating smog, examined the complex photochemical reactions among some of the sources, and was able to show that the bulk of the end products could be traced to automobile emissions and some stationary power plants.

But university participation in altering the environment goes back to a still earlier time. In the case of Caltech, for example, it includes the work on bringing the water supply into the Los Angeles Basin and also the design of the electrical transmissions system back in the 1920's. It includes the fundamental aeronautical design which led to the development of the aircraft industry, and to the jet aircraft whose exhausts at present contribute about 10 percent of the particulate matter to the atmospheric pollution. Thus I would certainly be the first to acknowledge that Caltech's technological contributions of the past bear some of the responsibility for the environmental problems which we face.

But we have also for a long time been interested in their solutions. For well over 20 years now Caltech has carried on work in environmental engineering, with faculty members such as Jack McKee, Norman Brooks, and Sheldon Friedlander doing research and training undergraduate and graduate students in problems of environment and ecology, and of waste disposal. In the early 1960's extensive laboratory research facilities in the William M. Keck Laboratory of Environmental Health Engineering were set up to advance this activity, and the Kerckhoff Marine Laboratory at Corona del Mar has been doing work in marine ecology for a long time. Last year, recognizing that the need for protection and control of man's environment had reached the crisis stage, Caltech instituted a specific degree program in environmental engineering science leading to the MS and PhD degrees.

What can the university do about the protection of the environment? The answer will be different for each institution, depending on its size, its specialties, and its particular interests.

All universities are dedicated to scholarship and to teaching. In the case of environmental pollution and control, as for other situations, the university's contribution will be new knowledge and able people. Some also have developmental adjuncts—for example, the Jet Propulsion Laboratory of Caltech. The developmental and systems engineering capability they provide can help deal with problems which have reached crisis proportions. But by and large, the university will not be unique in this particular capability.

What the university at its best may be able to contribute, and which will not be available elsewhere, is a critical look at problems and at proposed solutions. The university, on the other hand, will not be very useful at spending the $3 billion which it is estimated will be spent by the federal government over the next ten years for water purification. And indeed, the universities won't be getting any of that money. That program turns out to be one of matching funds to help support municipal and water district construction costs. It does not contain money for research or for training, though minor amounts may be found for these elsewhere in the federal budget.

Thus, activities of all the universities taken together will be swamped by the total needs—and the total expenditures—on environmental protection and control.

"I would be the first to acknowledge that Caltech's technological contributions of the past bear some of the responsibility for the environmental problems which we face."

This is probably as it should be, because research and training costs are likely to be a tiny fraction of the over-all cost of programs which are concerned with altering, avoiding, or suppressing the undesirable by-products of large-scale manufacture and consumption. Still, it is worth noting that research and teaching costs are small. And even in the league of total funding for university research and training, the size of the activity on the Caltech campus will be small, simply because we are a very small institution.

But despite this, we decided to take a look at some parts of the environmental problem. We wanted to educate ourselves to the current state of knowledge. We wanted to probe more deeply into the situation so as to be able to decide what we ourselves might be able to do. And we picked, as is natural to the inhabitants of the Los Angeles Basin, the smog problem. As a result we believe we understand better the smog problem and some of the things that need to be done. We also are somewhat reassured that some of them apparently are being done.

In going at this problem at Caltech, we had the advantage of having a number of people who were enthusiastic about tackling it. In particular, about three months ago Professor Carver Mead of our engineering and applied science faculty volunteered to enlist other faculty people to spend a fraction of their time, small or large, to take a look. They would examine not only the scientific and technical situation but also, so far as they were able, the economic, social, and political context in which that situation exists. About 25 other people contributed from 10 percent up to almost 100 percent of their time during the past three months. The group included three or four people from the Jet Propulsion Laboratory who were able to make use of the facilities there for computation and design. Many of the participants were professionals who already are deeply in this matter, such as faculty in our chemistry and chemical engineering division who have, over the past couple of years, put together a model for the purpose of forecasting smog production and distribution in the Los Angeles Basin. The study was funded from Caltech's own funds and from the JPL Director's Fund set up by NASA to allow JPL to engage in a small amount of this kind of research.

The group examined the various kinds of atmospheric pollutants and their sources. It looked at the projections of what is likely to happen to each of the pollution components as a function of time in the future. This was done for various assumptions about the controls to be imposed, made possible by technology which is now available or in sight. As I said, the group also looked briefly at the larger context of these technical questions.

Without going into all of the details—the group will be issuing a final report shortly—here are their conclusions and some of the possibilities which they think are feasible as a result of future development.

First, the breakdown of pollutants according to source and the future projections (based on specific assumptions) made by the Air Pollution Control District of Los Angeles County last year in *Profile of Air Pollution Control in Los Angeles County* have been generally confirmed by the Caltech study group. This should not be a surprise to anyone who has followed the work of the APCD or followed the available literature in this field. But I know that APCD was glad to have an independent group confirm their calculations and analyses in this regard, and I think this points out the value of an independent critical group which can be provided from the talents available at a university.

To summarize these results, about 90 percent of the carbon monoxide and high reactivity organic gases (hydrocarbons) in the Basin come from motor vehicles, as do about two-thirds of the nitrogen oxides, and 40 percent of the particulate matter. Forty percent of the sulphur dioxide (which has a rather small emission in Los Angeles compared to most eastern industrial cities) comes from the chemical industry, with smaller fractions coming from motor vehicles, from petroleum evaporation, and from stationary power plants. These latter also contribute about one-quarter of the nitrogen oxides and

"Environmental and ecological studies are more like chess than checkers. You must play two or three moves ahead."

about 10 percent of the particulate matter. Organic solvent usage contributes about 40 percent to low reactivity organic gases, and aircraft about 10 percent to particulate matter.

The reactive hydrocarbons and nitrogen oxides undergo photochemical reaction in the atmosphere in the presence of sunlight and oxygen to produce eye-irritating smog and ozone which, although not the producer of eye irritation, has its own bad effects. Because ozone is produced and moves with eye irritants, it is a good measure of their presence.

Sulphur dioxide emission in the Los Angeles Basin, as a result of the use of low-sulphur fuel, is actually less than it was in 1940. Carbon monoxide increased by a factor of four from 1940 to the mid-1960's, but has now begun to come down again as a result of the recent controls on carbon monoxide emission from motor vehicles. With the present control program it should keep falling, until in 1980 it will be near the 1940 level. Hydrocarbon emission from motor vehicles almost quadrupled from 1940 to the mid-1960's but has now begun to come down again as a result of the control program. With the present control program it projects to reach the 1940 level again in about 1980, despite the estimate of increasing auto use in Los Angeles. Nitrogen oxides rose by about a factor of five from the end of World War II to the mid-1960's. Then, as a result of the methods used to control exhausts of hydrocarbons and carbon dioxide, mainly consisting of introducing more air at a point of high temperature in the combustion cycle to burn the carbon and hydrogen more completely, the nitrogen oxide emissions began to increase still more rapidly, so that in 1970 they are over seven times the end of World War II level. The control program now being introduced should begin to bring that down, but even with the present control program the nitrogen oxide emissions will by 1985 still be about two and one-half times that of World War II.

This increase in nitrogen oxides has prevented the decrease in eye-irritation and ozone which would otherwise have resulted from the decrease in reactive hydrocarbons. But the projected drop in nitrogen oxides in the early 1970's should pay off in eye-ease.

All these improvements are feasible, the Caltech study group concluded, under the new standards to be imposed on piston engines through 1972, and they can be achieved with currently available technology. So, probably, can the more stringent standards which the ACPD has proposed through 1975, which could reduce auto emissions further into the 1980's.

The group suggests that the addition of catalysts to the fuel to break the repeated photochemical cycle, which allows nitrogen oxides to produce eye-irritant by-products more than once, should be considered again. Like so many ideas in the control of air pollution, it was originally suggested as a possibility by Haagen-Smit 15 years ago. The additive originally suggested, iodine, has the disadvantage that a small fraction of the population is sufficiently sensitive that it would probably cause much more damage than it would prevent, but there may very well be other such additives which do not have such effects.

An additional conclusion of the group, which was simultaneously (or even earlier) reached by others, as can be seen by the actions they have taken, involves the removal of lead from gasoline. The direct health effects of lead are arguable, but its elimination makes it easier to take other pollution reduction steps in the engine and exhaust. Both the auto manufacturers and the gasoline producers have made it clear that this is not only feasible but will begin to be done in a year or two.

Environmental and ecological studies are more like chess than checkers. You must play two or three moves ahead. Let's do that in this case. By the early 1980's, nearly all autos then in service will have incorporated what is now seen as the limit of economic technology available to reduce pollution in piston engines. But numbers of people and automobile use will continue to rise, so pollution will start up again. The more restrictive standards projected for the mid-1970's could delay this upswing by a few years. Unless a different cycle—

"Unless Caltech can contribute something unique, we don't want to add another element to the activities that have been generated by the enthusiasm to protect the environment."

electric, steam, gas turbine, or some combination—is employed for vehicles, the air will start to increase in pollution contamination again in the early 1980's. It is time to think about new power cycles—and new transportation systems—and to begin some development work.

These projections also assume that as the nitrogen oxide from automobile emissions is reduced, parallel efforts will be made to install equipment in the old stationary power plants burning fossil fuels to reduce their nitrogen oxide emissions. Otherwise, by the mid-1980's they would be producing as many tons (250 per day) of nitrogen oxide as would the motor vehicles. And in addition, these projections assume that essentially all of the new stationary power plants in the area will either be nuclear or be outside the Basin itself. My personal opinion is that because in southern California they can be sited on the ocean where the thermal problem will be less severe than it would on rivers, practically all new stationary power plants should be nuclear in this area. I recognize that such plants have by-products which must be carefully controlled or else they can present a hazard too. But one must weigh various alternatives and choose what seems to be the least damaging. Another alternative, still less feasible, is to forego added stationary power generation entirely. No one seems to be willing to do this for himself, although perhaps he is for the other fellow.

I noted that the projection is for emissions to begin rising in the mid-1980's. This will occur because the population will continue to rise.

Perhaps the most important conclusion of the Caltech study group is that there are other factors which are as important or more important than the technological ones. If the Los Angeles Basin environment is improved, as the APCD and other studies show that it can be, demographic forces will come into play which have a strong effect. The population is likely to increase in this Basin more rapidly. So you see that not only technological and economic, but also political and social factors enter.

Furthermore, one must consider the fact that it will take regulatory agencies of various sorts to produce the improvements that we foresee.

Why? In simplest terms, suppose a $200 increase in cost per automobile is involved in the changes in design that will reduce its smog production by a factor of ten. If the individual consumer is offered a choice, he will probably conclude that spending the $200 extra will bring him almost no benefit—the smog level will be reduced by one four-millionth in the Los Angeles Basin, and reduced no more for him than for his neighbor who buys a car without the smog-reducer. Result—no action.

Shouldn't the auto manufacturer then take the initiative? One could say that if all of his cars have the smog reducer and cost the extra $200, the consumer will have no choice. The catch is simple to understand. Suppose there are no governmental regulations which require all new cars (and later, all cars) to meet the anti-smog standards. In that case the auto manufacturer who puts the smog reducer on all his cars and charges the $200 will eventually go broke, since the consumer will buy his competitor's cheaper but smoggier vehicle.

Thus governmental regulation—local, federal, or both —is required in such forms as legislation, taxation, setting of standards, and inspection. But this regulation itself can create problems.

In the past, as some of the social scientists who have participated in the study pointed out, the regulatory agencies have had unforeseen and not always beneficial effects in the long run. They have tended to identify in one way or another with what they have been regulating. Therefore, new ways of going at the problem which involve substantial changes, in terms either of technology or organization, have tended to be suppressed. This has happened in communication; it has happened in transportation. It can well happen in the case of the ecology and control of the environment.

What lessons have we drawn from our study at Caltech? First, the technique of gathering an interested group of which not all members are expert on the specific problem

"We are thinking about a laboratory concerned with air pollution, solid waste disposal, urban mass transit, water use, noise reduction—in short, an Environmental Laboratory."

to be examined, but some, instead, in the disciplines which are its constituents, can well be applied to other environmental problems. Second, all environmental problems will benefit from the critical look that can be given by a nongovernmental, nonindustrial group. A university group can offer critical and expert appraisal of complex problems in a way that perhaps no other organization can. This can, I think, be of particular value to government agencies and to industries which want information but have learned to be a bit wary of the judgments or solutions they themselves will produce, given the institutional bias which they will tend to have toward those problems about which they know the most.

Finally, those who did the study at Caltech have one very strong cautionary note, and I agree with them. If such a study group consists only of scientists, technologists, and production people, it will almost certainly overlook some of the most important factors and come to the wrong conclusions. Unless expert social scientists are available—and I mean not only economists to examine the economic balance, but political scientists, sociologists, psychologists, and so on—the study will be done in too narrow a context. Although it will give the right answers to its own questions, it will prove to have overlooked questions more important than those which it asked.

What does this all mean to Caltech's own plans with respect to activities connected with the protection of the environment—not just air pollution, but other aspects as well? A group of Caltech and JPL people, headed by Professor Lester Lees of our engineering division, has been looking at this question. Unless Caltech can contribute something unique, we do not want to add another element to the near infinity of activities that have been generated by the universal enthusiasm about the protection of the environment.

What we are thinking about is a laboratory which would be concerned not only with air pollution, but with solid waste disposal, planning and development of a rational urban mass transit system, water use and reuse, reduction of noise emitted by stationary and moving sources—in short, an Environmental Laboratory. A reasonable size to aim for would be 25 to 30 professionals, plus part-time activity by interested Caltech faculty and student. Its staff would need to include economists, systems analysts, social psychologists, and other social scientists.

The social science capabilities would be added to the expertise already present at Caltech and JPL in photochemistry, combustion and chemical kinetics, instrumentation, atmospheric modeling and fluid mechanics, bioengineering, and systems development (especially the problems of interfaces in complex systems). These talents suggest that a most important function would be to provide advice to government agencies and legislative bodies, including independent and objective evaluations of various proposed technical solutions.

I could give a long list of technical questions that need examination by such a laboratory. It would range from work on the fluid mechanics and chemistry of the internal combustion engine to the dynamics of the atmosphere of the Los Angeles Basin; from instrumentation for measuring emissions from pollution sources and the atmosphere, to fuels and additives to inhibit the emission of nitrogen oxides, hydrocarbons, and carbon monoxide. But I want to note that it is easy to find problems in technology, economics, systems design, psychology, or planning—problems which need solution if we are to minimize environmental pollution. Finding the appropriate organizational structure and the necessary funding support is more difficult.

For these reasons, Caltech has not made a decision on whether and how to proceed with such a laboratory. But organizations—more than one—that can handle these disciplines and produce fresh answers to these changing problems will be needed if the human race and its environment are to remain compatible. Only a few years ago, most of us would have assumed that if the two became incompatible, we could easily change the environment to restore a safe balance. Few of us believe that any longer.

The Great Soviet-American Extragalactic Investigation

The cosmos yields ever so slightly

When looking toward the ends of the universe for answers to cosmic questions, little earthly problems like finding out what time it is, making a long-distance phone call, or deciphering an operating manual can mean the difference between success or failure. That's what a trio of Caltech radio astronomers discovered when they took part in the first Russian-American joint investigation of extragalactic objects last fall.

The Caltech people participating in the investigation were Marshall Cohen, professor of radio astronomy; and Kenneth Kellerman and Barry Clark (PhD's at Caltech in 1963 and 1964), currently at the National Radio Astronomy Observatory (NRAO) in Green Bank, West Virginia. The other members of the team from the U.S. included David Jauncey or Cornell, and John Payne from NRAO.

The experiment consisted of making simultaneous observations at two widely separated telescopes—NRAO in West Virginia and at the Crimean Astrophysical Observatory on the Black Sea in the USSR. The technique used has been called "very long baseline interferometry" (VLB). It gives angular resolution 1,000 times better than that obtainable with the largest optical telescope. Cohen, Clark, and Kellerman, with Jauncey, developed the VLB technique only three years ago.

In the Russian-American investigation, the 140-foot radio dish in West Virginia and the 72-foot dish in the Crimea were used like a pair of eyes, 6,000 miles apart in a straight line through part of the earth. When focused on the same object at the same time, the two radio "eyes" eliminate much of the fuzziness of standard radio telescope observations and, in some cases, give the actual size of the object. The system was designed to resolve a radio object as small as a beach ball a quarter million miles away.

Long baseline interferometry is made possible by atomic oscillators used to synchronize the observations. The atomic oscillator, which must be stable to within one part in a hundred billion, is attached to the radio dish and drives a clock. The clocks on the two dishes must be synchronized to within a few millionths of a second.

The purpose of these studies is to learn something about the mechanics of explosions that seem to be associated with such objects as quasars, and to find out something about their structure. The best way to do this is to look at the objects, which apparently are born small and expand with age, when they were very young.

Astronomers hope these kinds of observations, continued over a period of several years, will reveal changes in the configurations of the objects. Such information would help them develop a model and then an explanation of the exploding source.

But a technique that had worked well in an earlier joint investigation between the U.S. and Sweden ran into trouble in the Soviet Union because of a succession of ordinarily minor problems that mushroomed to critical proportions.

Kellerman and an engineer, John Payne, were scheduled to go to Russia three weeks before the start of the observations to set up and test the VLB equipment. Three Russian astronomers were to come to Green Bank to work with Cohen, Clark, and Jauncey on the American end of the base line.

The problems started almost from the moment Kellerman landed in Russia. He describes it this way:

"On September 10 I arrived in Moscow with my wife and was met at Sheremetevo Airport by Leonid Matveyenko of the Soviet Academy of Science. After retrieving our luggage, Matveyenko told the customs man 'Akademy Nauk,' and we passed through customs without any inspection or formality. We later found much use for the phrase 'Akademy Nauk,' which indicated that we were guests of the Academy of Science and would open any and all doors from looking at the crown jewels to getting last-minute reservations on a Russian airplane.

"Our equipment was scheduled to arrive September 15, and Matveyenko was dispatched to the airport to collect it. He returned that evening and reported he needed the 'baggage ticket.' I tried to explain that you don't get a baggage ticket with freight and that in any case I was already in Moscow when the shipment left the United States, and I could not possibly have any of the papers prepared when the shipment left. This appeared to cause some concern among the Russians.

The 72-foot dish at the Crimean Astrophysical Observatory on the Black Sea—the eastern end of the 6,000-mile interferometer base line.

"The following day we obtained a letter from the Academy of Science to the cargo and customs people saying that it was OK for me to collect our equipment. Matveyenko and I drove to the airport in a car and were followed by a truck which was to carry everything back to Moscow. Following about an hour of being sent from one office to another and several heated discussions, we were led to a shed that contained the equipment (or as all Russians insisted on calling it, the 'aperture'). Matveyenko appeared to be a little surprised at the size and weight of our 'aperture,' which consisted of three large wooden crates plus 25 boxes of magnetic tapes weighing a total of 3,000 pounds.

"It had been pointed out to us that the boxes were too big fit in the cargo door of an Aeroflot airplane to be flown to the Crimea. I argued that since they had come from London by Aeroflot they must fit. But my reasoning proved incorrect since the planes flying from Moscow to the Crimea have smaller doors than those flying from Moscow to London.

"There then followed a big discussion (Russians seem to like big discussions) as to whether the 'aperture' should be shipped by truck or railroad. To complicate the situation, it was necessary to send the rubidium clock and VLB control unit first to Leningrad to synchronize the clock with the German Loran (long-range navigation)

He took a quick look, saw a few glowing lights, looked with astonishment at the clock ticking loudly, and said OK.

station, while the tape recorder, front ends, and 25 cartons of magnetic tape were to go directly to the Crimea. It was finally decided to send the shipment to the Crimea by train and the one to Leningrad by truck.

"That evening the whole VLB party—John Payne, Matveyenko, Leonid Kogan (a Russian engineer assigned to the project), my wife, and I flew to Leningrad aboard an Aeroflot TU 104 jet. We also carried in the airplane our atomic clock that we hoped to set with the aid of Loran, which we were told was easy to receive in Leningrad. On our arrival, we were met by a delegation from the Pulkova Observatory where we planned to set up our Loran equipment.

"Lacking a proper Loran antenna, we strung a wire across the floor and promptly received what appeared to be the Loran transmission from Sylt, Germany. Only the Sylt station was supposed to transmit with a 79.6-millisecond period, and the signal we were receiving had an apparent period of 80.0 milliseconds.

"After wasting two days and convincing ourselves that everything was working properly, and rejecting the unlikely possibility that Loran had changed its period without announcement, the light finally dawned—we were not receiving Loran at all but an unadvertised Russian copy. The real Loran appeared to be buried in interference from the most powerful transmitter in the USSR, located only a few miles away, which broadcast entertainment to Soviet ships all over the world.

"It was rumored that at various odd hours of the night on certain days of the week the interfering station would temporarily stop broadcasting, but this never materialized, and it was becoming clear that we were getting nowhere fast.

The discussion degenerated into the difference between electrical + and physical + and electrons and holes.

Imagine a Russian trying to get on a flight from Miami to New York with a strange, ticking box, and having only a voltmeter and pliers for luggage.

"We had previously explored the possibility of flying a running clock into the USSR, but our Russian colleagues in Moscow indicated that this would be 'impossible.' But in Leningrad they were more optimistic and thought that it might be arranged. On September 21, I telephoned Bert Hansson, one of our collaborators on previous VLB experiments in Sweden, to see if he could arrange to synchronize their clock in Stockholm and send it to Leningrad. But I was told that (1) it was a weekend, and there was no one around to prepare a proper box; (2) they had no batteries, and it was not possible to buy batteries on the weekend; and (3) Sweden had just experienced a major storm which blew down an antenna at their observatory and had damaged the director's yacht. Nevertheless, Bert promised to 'see what he could do.'

"Meanwhile, the first observations were only about a week away, and we hadn't even been to the Crimea site yet. So John decided to go alone to the Crimea to set up the VLB equipment, install the front ends, and check out the TWX machine that was supposed to be installed. I stayed behind in Leningrad to struggle with the Loran receiver and await the clock from Sweden.

"Having no success with the Loran receiver and not hearing from Sweden, I found things looking a bit grim, but on the night of September 24 we went anyway to the Leningrad airport to meet the Aeroflot flight from Stockholm. To our pleasant surprise, there was a heavy wooden box addressed to me, strapped in a first-class seat with a safety belt. Of course, the customs man wanted to see what was inside. We handed him some official looking papers of explanation and opened the box. He took a quick look, saw a few glowing lights, looked with astonishment at the clock ticking loudly, and said OK. We quickly left before he could change his mind.

"We synchronized the NRAO clock to the Swedish clock, attached the Russian batteries in case of power failure, and left it to run at Pulkova. We had the nickel-cadmium batteries supplied with the Swedish clock recharged and set off for the airport to fly to the Crimea. We also carried two 6-volt car batteries and an inverter to supply 230 volts. This combination gave us a battery capacity that was good for about 25 hours—more than enough (so we thought) for a two-and-a-half-hour plane trip. The whole load weighed about 200 pounds, and it took some explaining to get it on the airplane.

"The flight was uneventful, and upon arriving in Simferopol, the capital of the Crimea, we were met by Dr. Ivan Moiseyev, director of the radio astronomy station in the Crimea, and set off on a two-and-a-half-hour winding drive through the mountains to Yalta. (This was the first of ten such trips I was to make.)

"In Yalta we were greeted by John with the news that (1) the TWX machine could not be connected because the lines were not good enough; (2) the 50 ohm, 10 dB loss cable they promised us was 72 ohms and had 20 dB loss, which meant we could not get enough signal from the control building to the antenna; (3) he could not receive the Loran timing signals from Turkey.

"But the real blow came when the box was opened: The clock had stopped on the airplane halfway between Leningrad and Simferopol. The batteries had lasted only about an hour.

"This was the low point of the expedition. We had no time synchronization, not enough local oscillator signal to the telescope focus, no way to communicate with Green Bank by TWX, and the first observations were only five days away.

"Someone had to carry the Swedish clock back to Leningrad and synchronize it with the NRAO clock that was still running (we hoped) at the Pulkova Observatory. Only in the USSR, you do not just go to the airport and buy a ticket for Leningrad, particularly if you happen to be carrying an atomic clock (size about 4 x 2 x 1 feet, weight 150 pounds) with you. To make matters a bit worse, it was the end of the tourist season, and the planes leaving the Crimea were booked solid.

"Since there was so little time remaining, John and I decided the only way to get enough local oscillator signal to the mixer was to move the VLB equipment from the control room to the telescope. It took some courage to announce this decision because the control room was on the second floor of the building and it had taken the better part of a day to get the two heavy VLB racks installed. But our Russian friends took it in good spirit, and in order not to damage the equipment insisted on repacking everything in crates. Getting the crates down the stairs was a formidable task, but not nearly as difficult as trying to get them up to the operating room of the telescope structure, which was about 15 feet above the ground and accessible only via a narrow staircase. For this task a crane was summoned from the Crimean Astrophysical Observatory about 100 miles away, and the VLB equipment was ceremoniously hoisted into place in front of 15 to 20 spectators.

"The following morning I departed by car with Kogan and our rubidium clock to catch a 9 a.m. flight to Leningrad. The plan was to arrive in Leningrad about noon, set the clock and recharge the batteries, and return on an evening flight to the Crimea. When we arrived at the airport, we found that the plane was full, and we would have to wait until 5 p.m. before leaving.

"Just before plane time, the local chief at the airport, a rather formidable looking Russian lady, wanted to know what was in our box, and why it couldn't go in the baggage compartment, and also where our personal luggage was. We tried to explain, carefully avoiding the use of the term 'atomic clock,' and had some difficulty in convincing her that we had no personal luggage because we were coming back in a few hours! Imagine a Russian trying to get on a flight from Miami to New York carrying a strange looking box (ticking, of course) with wires and batteries, and having only a voltmeter, a pair of pliers, and a large screwdriver for luggage; and you get the picture.

"When we arrived in Leningrad at 8 p.m., it was cold

During flight we took turns running to the rear every 15 minutes to check batteries. To look inconspicuous, I pretended I was going to the toilet.

I finally got worried enough about it at six o'clock to wake John Payne and ask him where the manuals on the thing were.

"At the end of the three-centimeter run we were planning to break out some vodka to toast the Russians at about 140030 UTC, but we never made it because they broke out the cognac at 140020. It was quite a party."

and raining (it's always cold and raining in Leningrad), and there was no car to take us to the observatory. After getting a good soaking in the rain, we finally hailed a taxi and got to Pulkova. We resynchronized the clock, charged the batteries, and were ready to return to the Crimea. Unfortunately, since it was the end of the tourist season, not very many people were flying to the Crimea this particular Saturday night, and so to save some rubles, Aeroflot had cancelled our return flight.

"The next morning we made it to the airport, only to find that there was a mistake on our ticket and that actually the plane would leave at 11 o'clock, not 10 o'clock. Just to be safe I suggested that since we had to wait an hour we should plug the clock in and not drain the batteries. Upon opening the box to get the power cord, we found that the clock had stopped again!

"It was easy enough to cancel our flight, but a major problem to book a new flight. In Russia you can't buy airplane tickets at the airport, only at your hotel or at the Aeroflot ticket office in Leningrad. However, we were not staying at a hotel. To further complicate the situation, it was Sunday. And in Russia no one works on Sunday, including the airline office. Nevertheless, I told Kogan to go into Leningrad and get us a ticket for an afternoon flight while I went back to the observatory to 'reorganize' the clock and batteries. (I was beginning to have blind faith in my Russian colleagues' ability to overcome all bureaucratic obstacles.)

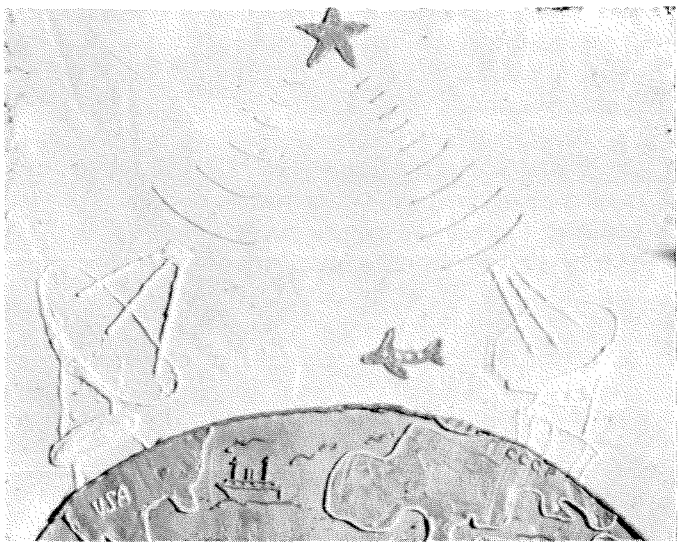
"It was becoming increasingly clear that we were doing something wrong with the batteries. Someone suggested that maybe they were being charged backward, and perhaps the Russian definition of + and − was not the American or Swedish definition. The discussion then degenerated into the difference between 'electrical' + and 'physical' + and electrons and holes, etc., which was clearly all nonsense.

"We later realized that our problem was that each night the line voltage, which was nominally 230 volts, would drop to about 190 volts and slowly discharge the NiCd batteries that were attached to run the clock in case of power failure. Since NiCd batteries were sealed, there was no way to measure the state of charge until the terminal voltage began to drop. This occurred only as the batteries were nearly discharged.

"'Kogan telephoned that he had managed to get us two plane tickets for a 4 o'clock flight. Following another big send-off and round of handshakes and good wishes, we departed once again for Leningrad airport.

"This time we didn't take any chances on the batteries running out. During the three-hour flight Kogan and I took turns running to the rear of the plane every 15 minutes to check the batteries with our voltmeter. In order not to look too conspicuous, I pretended each time to be going to the toilet. But after a few sessions we realized that this was even more suspicious looking.

"Toward the end of the plane ride, the NiCd batteries



To celebrate the experiment's success the Russians had a cake—appropriately, showing the object being observed to be a red star—made up in Yalta.

began to fail, and we switched over to the auto battery we were carrying. We made it to the Crimea and managed to transfer everything to the car without mishap. Halfway to the observatory we had to go over to the battery that was running the car. With this we made it to the observatory and got the clock attached to the 230 (more or less) volt line.

"The first observations were planned just as a test for the main run two weeks later. The plan was to run a few tapes on 3C 273 and 3C 454.3, the two strongest sources at 3 cm. The run on 3C 454.3, unfortunately, came about 3 a.m. local time in the Crimea. So when it was finished, John and I looked about for a ride back to our hotel and some badly needed sleep. We found our Russian colleagues upstairs breaking open the vodka and cognac. After completing 2 percent of the scheduled observing, they decided it was clearly time for a celebration. Following two hours of eating, drinking, and declarations of Soviet-American friendship and cooperation, we were finally taken to our hotel.

Late the next morning we were met by Moiseyev and had a leisurely lunch, after which he told us 'Oh, by the way, a telegram arrived this morning.' It was from Barry Clark. The frequency had been set wrong at Green Bank, and he wanted to repeat the run on 3C 273 in about two hours. We made a quick trip out to the observatory, arriving with about an hour to spare and managed to run the tapes on time.

"The plan was to immediately return the tapes to NRAO for processing before the next run two weeks later. This might be considered wishful thinking, but it had all been carefully arranged in advance. Immediately following the second 3C 273 run, the tapes were quickly packed up and driven to Yalta, where a Russian astronomer was waiting to leave for Moscow. He would deliver the tapes to the foreign office of the Soviet Academy of Sciences where they would be collected by a driver from the U.S. Embassy, who would bring them to the U.S. Scientific Attache, who would then give them to a returning American geologist who was flying to Washington that afternoon. Having been alerted by our telegram, Barry would be at the Washington airport to collect the tapes when they arrived. It seemed like a 'sure-fire' scheme, and we could relax until the next run."

But the problems had just begun. Here's how Clark tells of his part in the odyssey:

"It so happens I never did get the telegram that told me about this whole arrangement. I was just sitting around Green Bank for some days wondering where the heck the tapes were. Then finally this guy in Washington rang up and said he had a couple of tapes there. One of our people went to Washington and picked the things up.

"We spent three or four days trying to process the tapes, but we were unable to make the interferometer work. Well, it was strongly suggested that something was wrong somewhere.

"Many, many things can go wrong in an experiment like this, but one of the more likely possibilities was that something had happened to the clock on the way to the Crimea. So it was decided that I should go over there with a little crystal clock, which wasn't really that good but could furnish an order of magnitude check on things.

"When I arrived at the observatory in the Crimea, we found the time on the clock I had brought was dramatically different from the clock that Ken had brought down from Leningrad. Luckily, the interference cleared up for a few hours, and we were able to pick up the signal of the Loran station in Turkey. That signal confirmed that the clock brought from Leningrad was correct, and that something had happened to the crystal clock.

"Communication was still one of our biggest problems. Before the main experiment we never had a phone conversation in which somebody who knew what was happening was on both ends of the phone.

"The usual thing was to ring up the operator and say, 'I wish to talk to this number in the U.S. I would like to book this call for tonight at eight o'clock.' At eight o'clock

We really attacked the communications problem. We sent a telegram, booked a phone call, and tried to send a TWX all at the same time.

the phone would ring, and the operator would say, 'It is impossible to reach the U.S. at this time. We'll let you know when we can make contact.' And then, at some random time in the next three or four days, the phone would ring in Green Bank, and the phone would ring in the Crimea, and the operator would say, 'America, speak!'

"We had such trouble that our Russian friends finally called up the National Minister for Communications of the Soviet Union and said we had to be able to contact Green Bank by telephone. Thereafter, we had reasonable luck using the telephone. By this I mean we could place a telephone call and have it go through in three or four hours.

"We set up for the first of the main observing sessions at six centimeters wavelength. This was scheduled to be a two-day observing session. Since we got no results from the previous test experiments, we were also going to send a few more tapes very quickly back to Green Bank for further processing. Kellerman was going to Moscow with the tapes to take them directly to the American Embassy to get them shipped off as fast as possible.

"At about five o'clock of the morning Ken was to leave for Moscow with the tapes, I was sitting there staring at the equipment and worrying about things as usual. It finally occurred to me that there was one switch on a piece of fancy commercial equipment that didn't seem to be in the right position. It was confusingly labeled 'switch.' I had, indeed all of us had, looked at the thing and worked through the symbolism and concluded that was a reasonable way to have the thing set. But I finally got worried enough about it at six o'clock to wake up John Payne and ask him where the manuals on the thing were. I looked it up and indeed the switch was in the wrong position.

"This meant frequencies were in error, probably on the order of a kilocycle, which was totally disastrous unless it could be measured. And there was no equipment around capable of measuring it to the necessary precision. By that time I concluded the observations on the tapes were useless.

"My first concern was that we should extend the observing period. To do that we had to let the people in Green Bank know, since it doesn't do much good to observe on only one end of the base line. So we really attacked the communication problem on that. We sent a telegram, booked a phone call, and tried to send a TWX all at the same time. We intercepted Kellerman at nine in the morning. He came back, picked up a couple of good tapes, and went back to the airport. On arriving in Moscow, he also sent a telegram to Green Bank. We finally reached Green Bank by telephone and then ran through the six-centimeter observations again. We were able to get the tapes back to Green Bank, have them processed, and find out the interferometer was working before we started our observations in the three-centimeter series. This made us a great deal happier."

There is one thing about experiments in long baseline interferometry. They end on time. And no one had to tell the Russians when to start the celebration.

Clark says, "On that equipment you know when things are going to happen. At the end of the three-centimeter run we were planning to break out some vodka to toast the Russians about 140030 UTC, but we never made it because they broke out the cognac at 140020. It was quite a party."

Meanwhile, back in Green Bank, the crew of astronomers just turned off the switches and went to bed, too exhausted to think about celebrating.

Although the sensitivity at three centimeters was not as high as expected, and no major breakthroughs were made in the six-centimeter observations, the astronomers were satisfied that the investigation had contributed to the body of data that will help scientists to understand the nature of extragalactic objects.

The investigation also revealed something on earth that may prove far more significant than the results of the observations of universe—that genuine scientific cooperation between the Soviet Union and the United States is possible.

The Month at Caltech

New Provost

Caltech will have a new vice president and provost next fall, and even though he has been a member of its faculty for 24 years, he looks forward to having six months to learn about the job before he has to start doing it. Robert F. Christy, professor of theoretical physics and chairman of the faculty, will hold the second highest administrative post at the Institute when he succeeds Robert F. Bacher, who retires after having served as provost since 1962.

As Caltech's chief academic officer, Christy will have over-all responsibility for faculty appointments and promotions, and for academic planning and research. He feels that the fact that he has been at Caltech for a long time, that he knows the faculty, and that they know and trust him will be very helpful.

Christy decided to accept the job of provost partly on the basis that he has changed the nature of his work occasionally in the past and has found doing something new very stimulating. "I'd rather try new things than get into a rut," he says.

In fact, Christy's career shows very little evidence of time spent in a rut. During World War II he worked at Los Alamos on the development of the atom bomb. In the immediate postwar years he concentrated on theoretical and nuclear physics, and for the last nine years he has been doing astrophysics, with the result that his calculations on variable stars won him the prestigious Eddington Medal of the Royal Society of London in 1967.

His research and teaching contributions are only part of his service to Caltech.

Provost-to-be Robert Christy

He has been on the faculty board, the academic policies and the academic freedom and tenure committees; he was also a member of the presidential selection and the aims and goals committees. He became executive officer for physics in 1968, and in 1969 was elected chairman of the faculty. He says he spent a good deal of time in that newest job interacting with faculty members, discussing their problems, and trying to see sensible approaches to helping them. It was an interesting experience, and it led him to believe that what a provost would have to deal with would be both challenging and rewarding.

The time Christy has spent on the chairman's job in the last six months has interfered considerably with his research, so he is prepared for the fact that being provost will interfere even more. What makes it worthwhile, however, is the opportunity to oversee the activities he feels are the very heart of the Institute—its academic program. (He stresses that he means "oversee," not supervise or control.)

Because the provost is supposed to be aware of and sensitive to faculty views, reactions, and problems, and because the provost has over-all responsibility for academic matters, Christy believes that in the long run the actions of the provost will have a major cumulative effect on the Institute. However, Caltech's small size and the accessibility of its administrative officers to anyone who wants to speak to them make all activities and problems somewhat interrelated.

"At this juncture in the Institute's history," Christy comments, "there are some very interesting changes in the works. In an administration where many positions are filled by persons who have held them for a long time, a newcomer to a job is constrained by traditional habits and attitudes. We have a new president and many other new people in important positions or being sought for them. We are likely to be bound much less than normally by the past. This is a situation that invites expression of new ideas and gives freedom for new ways of doing things."

SALT

As one of the six members of the U.S. delegation participating in strategic arms limitations talks (SALT) with the Soviet Union, Caltech president Harold Brown attended the first session in November and December in Helinski, Finland. Though limited in the amount he could disclose regarding these first meetings, he made some comments in a speech to the Los Angeles World Affairs Council on February 9 (and repeated his remarks to the Caltech community in Beckman Auditorium on February 18).

"In these discussions," he said, "we seek to slow down, to halt if possible, and perhaps eventually even to reverse the continual cycle of development and deployment of new strategic weapons systems which constitutes the strategic arms competition between the United States and the Soviet Union." Both countries now have the ability to strike back and destroy, despite any defenses, the civilization of the other. The security of each side thus rests on this assured destruction capability, which deters the other from launching a thermonuclear attack.

Brown pointed out that "this is a dismal sort of safety, resting as it does on the good sense of another government, but so long as nuclear arsenals exist it is the best we are likely to know." Continued developments and deployments are not likely to make either side any more secure, and it may well turn out that both will be less secure.

The talks at Helsinki were not for the purpose of negotiating agreements but to see if the foundations could be laid for subsequent substantive and detailed negotiations. Brown said that each side wanted to explore the thinking of the other and to probe its seriousness of purpose.

The Soviet motives in strategic arms talks are probably quite complex, but Brown felt that among them might be, first, a desire to damp down the arms race, improve the security of both sides, and save the money that would otherwise be spent on strategic forces. A second possible Soviet motive might be to prevent U.S. moves in the strategic arms competition that they fear might tilt the balance in our direction. A third motive could well be political, perhaps to stabilize one of the many fronts on which the Soviets find themselves engaged or to place restraints upon the U.S.

Whatever their motives, Brown said it became clear to him that the Soviets were quite serious in these discussions. In the first place the Soviet delegation had individuals who were very knowledgeable about the Soviet weapons programs —a new phenomenon in such discussions. Second, there were no polemics and few if any examples of ideology overriding sound analysis. Though clear differences in view emerged, both sides kept the talks private, serious, and constructive in tone. The third evidence of the Soviet attitude was that their presentations showed that they had been thinking long and hard about strategic arms.

Brown felt that a program of work was agreed upon as a result of the discussions that will allow flexibility for subsequent negotiations that will begin in Vienna in April. He cautioned Americans not to expect too much, too fast, since in such matters as national security both sides can be expected to exert great care and caution. At the very least, it is his opinion that we can expect a continued productive dialogue about the weapons systems of both countries. Such a dialogue cannot help but provide information for both sides that may reduce the most extreme concerns that have led to overreactions and increased rates of strategic arms buildup.

It is also possible that a specific agreement may be reached; this would require complex arrangements, patience, and give-and-take on both sides. Thus, even if the talks are successful, they may go on for a long time.

Brown concluded that no international enterprise is more important than damping the strategic arms competition. "The coming phases of SALT may determine whether each of our countries is given the chance to turn to the nonmilitary problems which concern us all—the population explosion, environmental pollution, domestic tranquillity, the problem of the developing countries—or whether, instead, the risk of turning this planet into a thermonuclear inferno will become greater and greater, and perhaps overwhelm us all."

Computer Addition

Within a year a $1.2-million addition to the Booth Computing Center will more than double the space for computer processing at Caltech.

The new facility is being built in response to rapid growth of Caltech's work in information science—the understanding of the complex relationship of science, technology, and social systems through new mathematical concepts and computers. Also, more than 1,200 Caltech faculty members and students now use the computing facilities, with physicists, biologists, chemists, and engineers being especially heavy users.

With the 30,000-square-foot addition, the computing center should give Caltech adequate space and facilities for at least five years. The addition will consist of laboratories for research in the information sciences and biosystems, an extra large laboratory for use in information processing in social systems, keypunching facilities for computer use, a keypunch service, space for card deck storage, customer cubicles, conference rooms, and offices.

Lacey Lecturer

Rutherford Aris, professor of chemical engineering at the University of Minnesota, was the third recipient of Caltech's annual W. N. Lacey Lectureship in Chemical Engineering. Aris presented two lectures in February at Caltech, one on "Mathematics and the Elucidation of Chemical Concepts: The Notion of a Chemical Reaction Mechanism," and the other on "Mathematics and the Modeling of Chemical Systems: The Transient Behavior of Chemical Reactors."

The Lacey lectures bring experts currently active in chemical engineering or related disciplines to the campus. They are made possible by a fund established at Caltech by friends and former students of Lacey, who became professor emeritus of chemical engineering in 1962. He came to Caltech in 1916, and served as dean of graduate studies and dean of the faculty in addition to his teaching and research. Lacey is widely recognized for his research on the behavior and properties of hydrocarbons.

Aris is known for his contributions to the mathematical analysis of chemical reaction systems and to the analysis, control, and optimization of chemical reactors. Among his awards are the E. Harris Harbison Award for Distinguished Teaching from the Danforth Foundation, and the Alpha Chi Sigma Award for Chemical Engineering Research from the American Institute of Chemical Engineers, both won last year.

Digging In

It was Arbor Day, so what could be more appropriate than for the new Caltech Environmental Action Council to plant a tree (two, in fact, near Millikan Library). The Council, a student organization with offices in an unused part of the Campbell Plant Research Laboratory, claims the participation of about 70 students, staff, and faculty in their activities. CEAC's purpose, stated in its monthly newsletter Ecology, *includes "catalyzing concern about ecological problems, cultivating ecological awareness, encouraging political and social action, and conducting educational programs."*

Letters

A Meeting of Minds—Bump, Bump, Bump

The following letter to Robert Sinsheimer concerning his article in the January E&S ("The Brain of Pooh: An Essay on the Limits of Mind") is reprinted, along with Dr. Sinsheimer's reply, with the permission of the correspondents.

DEAR DR. SINSHEIMER:

I have read with great interest your recent article in *Engineering and Science.* Clearly the article might have been entitled *The Brain of Pooh: An Essay on the Limits of the Mind of Robert L. Sinsheimer.* Perhaps you feel that external manipulative control of other human beings is a legitimate aim of science. I do not—nor do I feel that within my own lifetime will I ever fully reach the limits of my mind. Articles such as yours confirm the popular view of scientists as controlling, manipulators of other humans—ostensibly for their own good—but actually to satisfy the desire of the scientist for immortality and power. You will of course recall Dr. Frankenstein and perhaps you will also contemplate his doom as parallel to that of a science that has so little humanity that it must be destroyed by a popular uprising.

Our selfs were not constructed by evolution to deal only with the immediate external world. Our minds also enjoy an unlimited potentiality for imagination, creative feelings, hope, and care. Scientists who wish to deny these facets which free the mind of the narrow constrictions imposed by Aristostelian logic and linear thought resemble Pooh-Bear and will continue to bump-bump-bump down the stairs until the end.

Fortunately, each of us is also a Christopher Robin: a finite center of possibility, of knowing, and of willing that tends to be infinite—because we can imagine not only the possible and conditional—but can conceive the impossible, the unconditional, the infinite, the whole and the nothingness of being. These unlimited possibilities reside in each of us—and can be tapped by each person willing to take the risk, without recourse to DNA or injections of other drugs.

I believe that man is capable of being human now; your view seems to be that man is the missing link between apes and machines.

JAY M. SAVAGE
Professor of Biology
Associate Director,
Allan Hancock Foundation
University of Southern California

The reply:

DEAR DR. SAVAGE:

Your letter raises important questions which deserve discussion. (They also deserve a better advocacy than trite reference to Frankenstein and power-obsessed scientists.)

I believe the essence of our disagreement lies in your assertion that there are no limits—"unlimited possibilities"—to human capacities. I could wish that I could command the conceit to believe that.

As biologists we are well able to recognize the limitations of other species (relative to our own) in the performance of varied mental functions. What reason then is there to believe that our capacities, derived in a sequence of evolutionary steps, are not also subject to limits?—though it is of course more difficult for us to perceive these limits. That we can extrapolate in certain dimensions to zero or infinity is hardly an answer. Indeed for a certain few functions we can already begin to define these limits, as I attempted to describe.

It is not my aim to "manipulate" man. Quite the contrary. I believe that these limits—still largely undefined and unrecognized—to human capabilities are in fact basically responsible for much of the woe of the world. And I would hope—when change becomes feasible—to use this knowledge to free us from these inherent and crippling constraints which we have had for so long simply to accept.

This is not manipulation—it is the enlargement of human freedom.

ROBERT L. SINSHEIMER
Chairman
Division of Biology
Caltech

Millikan Speaks For Himself

EDITOR:

Please refer to the article by Arthur Laufer in the February 1970 issue ["The Sponsorship of Basic Research"], page 12, 2nd paragraph. Mr. Laufer states that in the 1930's Dr. Robert Millikan, "in answer to an English bishop," referring to atom energy, said:

> " . . . 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."

Now I heard Dr. Millikan say some things about the atom in either 1921 or 1922 (certainly not later). He carefully described the enormous amount of energy in the atom. Of this I am sure because it really made an impression. Then he talked about the future. And as I remember, he said that it would take a *long time* for scientists to find a way to get this energy.

We can all agree that Dr. Millikan missed on his estimate of time. But I strongly doubt that he believed that it remained locked up.

My memory is open to question as to some of his statements. But surely there are men still around that knew him well and can comment with greater certainty.

A. M. WHISTLER, '22
Los Angeles

Robert Millikan made the statement in an article he wrote entitled "Alleged Sins of Science" in the February 1930 issue of SCRIBNER'S *magazine.*

Un-Friendly

EDITOR:

It is indeed unfortunate that you do not appear to understand what biased reporting is or how it works!

If you had published the speech given by V. P. Agnew AND that given by Mr. Friendly in the January 1970 issue of *E&S* ["Some Second Sober Thoughts on Vice President Agnew"], careful reading would have shown you and all readers that in almost *all* instances Mr. Friendly, in disagreeing with Mr. Agnew, has distorted or otherwise misquoted from context, thereby completely changing Mr. Agnew's intended meaning.

The message which my study has shown that Mr. Agnew was trying desperately to convey is apparently beyond the comprehension of those who *today* call themselves "liberals." Mr. Agnew was in effect calling for Dr. Millikan's "Scientific Mode of Approach," not the spouting of a personal ideology without the speaker making any honest effort to develop an *understanding in depth* of the full spectrum of philosophies important in a given field.

To be specific, the complaint he made about Mr. Harriman was against the use of Mr. W. Averill Harriman and ONLY people who believed in Mr. Harriman's ideologic philosophy as critics of Mr. Nixon, choosing NO ONE NOT AGREEING WITH MR. HARRIMAN.

Remember: Mr. Harriman had a hand in the loss of China to the

Communist juggernaut, in the early activities in the loss of North Korea, in the sell-out of Laos to the Communists which made the Vietnam nightmare possible, and most recently he has been of course in Paris. In the face of these and his other activities, how can you say other than that his ideologic orientation is soft on Communism?

I care not what his or anyone else's *personal* philosophy is if he understands his subject to such a depth that he CAN IN FACT LOGICALLY DEFEND ANY OF A FULL SPECTRUM OF VIEWPOINTS. *AND DOES!*

I just wish I could find more than one major mass media group or group of individuals, signed, or unsigned, who understood in depth the fields they discuss, and made an honest effort to treat their subjects objectively.

It would appear that to get ahead in the mass media you have to be a graduate of the London School of Economics or believe strongly in their socialist philosophy.

You see, Mr. Friendly apparently equates analysis with ideologic bias. I doubt that this is intentional, but he probably knows much more than he understands in these subjects. So he sways with the "crowd."

There is a big difference between objective "analysis" after a presidential speech and using "analysis" time for counter-briefing (as President Johnson and President Kennedy both found). In fact, only a few days ago, CBS News teamed up with the Democratic Party to put on a so-called "NEWS Special" which was really either a continuous commercial disguised as news or demagogic propaganda all the way through. Yet they almost completely hid the fact that the Democratic Party had anything to do with it, to the point that I thought that CBS News had hit an unbelievable new low. Normally CBS News insists on keeping complete control of content of a special news program. Unless they are teamed up with socialist-thinking Democrats? That was a perfect example of why Mr. Agnew felt obliged to speak out.

Can't you return *E&S* publication activities to following at least partially Dr. Millikan's "Scientific Mode of Approach" by making certain that the full story relating to such events as Mr. Friendly's visit is presented for readers? Not all readers had access to the text of Mr. Agnew's speech for study in addition to hearing it. Only through detailed comparisons can the insidious effect of Mr. Friendly's misquotes be brought out clearly. (You probably would not have published his speech if you had done it objectively yourself!) Objectivity is a vital criterion for all activities at a school like Caltech. I feel that even the Caltech YMCA should show more objectivity than it does by selecting a broader spectrum of speakers. But I have yet to see an even half-way objective presentation of any viewpoint in *E&S* where a politico-economic bias is possible.

Our society is heterogeneous, not monolithic. Otherwise Caltech could not exist at all. Isn't it time you recognized that it takes heterogeneity and independence of thought and action and intellectual laziness on the part of people, particularly in the politico-economic sphere, to maintain our "unstable" but highly productive economy in the face of efforts to crowd us into socialist stagnation?

The fact of the matter is that without man's creative instability and his desire for independence, we would still be in the cave.

KEATS A. PULLEN, JR., '39
Kingsville, Maryland

Hadley Ford Is Never Bored

Hadley Ford, in charge of university relations at Caltech's Jet Propulsion Laboratory, found his life changed considerably when, during a plane trip last December, he noodled out some little poems.

Once started, there was no stopping. He took to carrying pad and pencil all the time because he couldn't ignore the poems' demand for liberation. He has since turned out more than 250, and says it hasn't been easy to live with all those things tumbling about in his brain.

He wonders if there will ever again be a time when he won't see the world in poetical groupings such as those on this page.

The universe is not
As we know it
Our perceptions
And projections
Grow it

Life results from the Atom's insistence
On Learning
Of its own existence

We will soon
Compose our hymns
Entirely
Of acronyms

Let me make a simple
Assumption
Success depends on the square
Of one's gumption*

*Another assumption
I'd like to share
Our nation's success
Depends on the square

Things may get better
before
They get worse
but not if you consider
The whole universe

Here's a question
On which you might brood
Is the air you breathe
Any worse than your food?

The best management tools
Come
From bending the rules
Some

Proposed ideas
Are seldom gem'nal
If they start with
An "Ahem"nal

Some errors are little
And some are much less
But it doesn't take many
To end in a mess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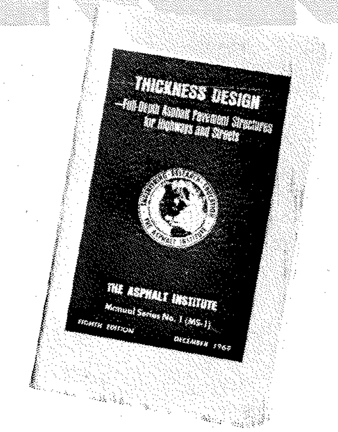
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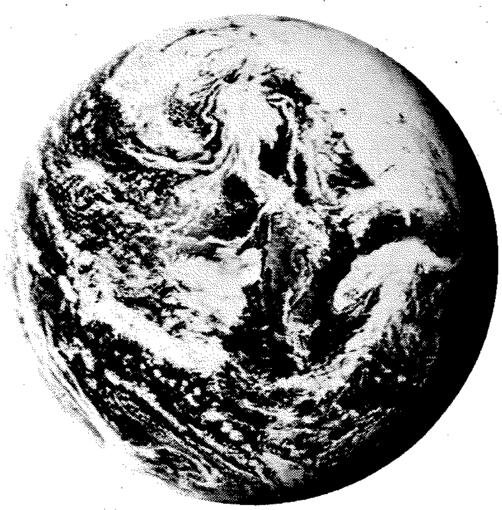

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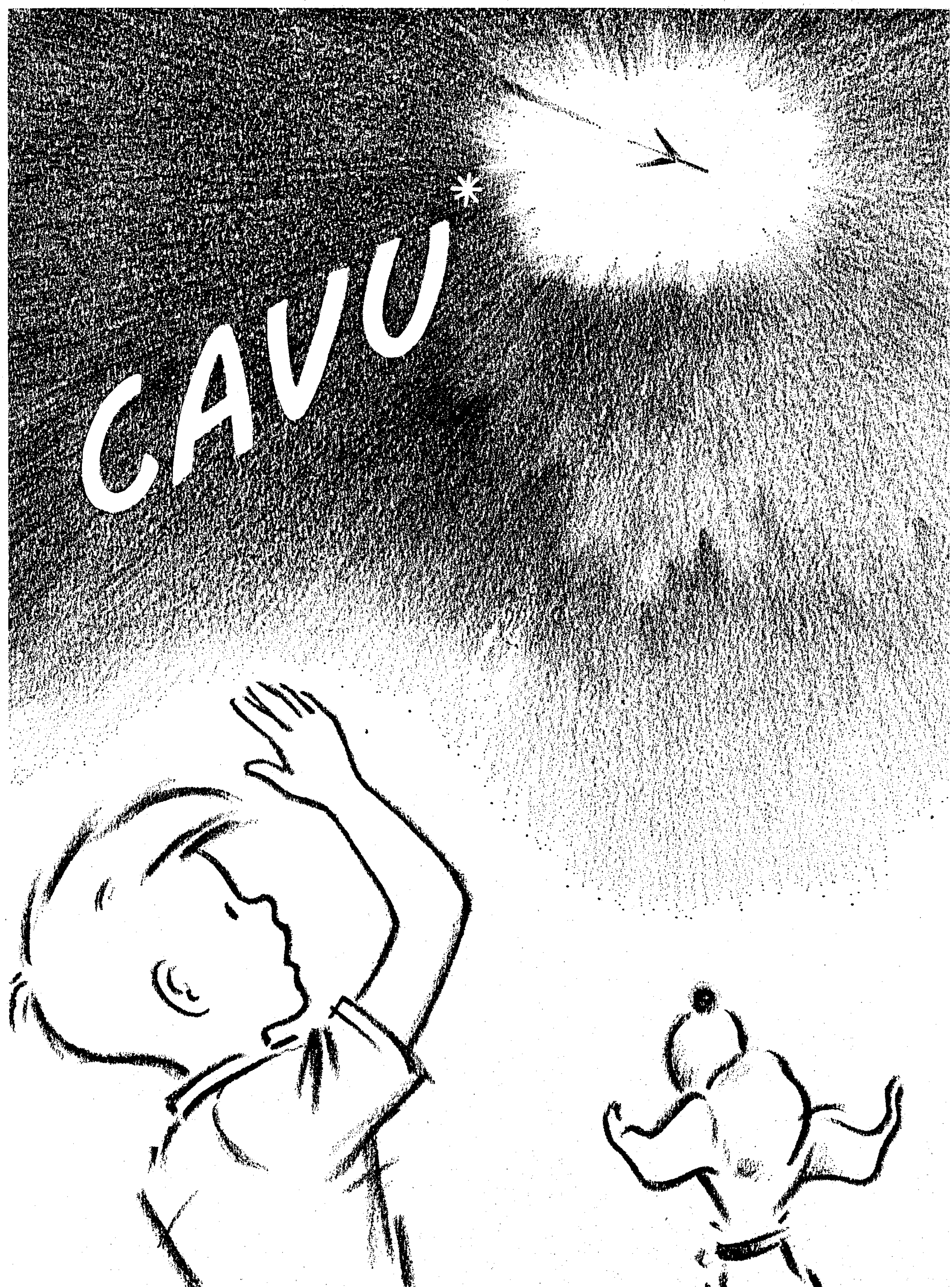
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