Engineering and Science

California Institute of Technology | May-June 1976

In This Issue

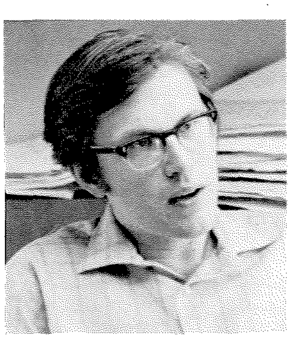
Package Deal

On the cover — parts of two series of nested containers, both from Russia, both more intricate and extensive than they seem, and both — in a way — connected with Caltech's "Lunatic Asylum." In fact, it was the Lunatic Asylum's Gerald Wasserburg who pointed out their similarities, thus giving *E&S* a chance to tell the story of some "Souvenirs from Russia" on page 18.

Shock Talk

Roger Noll, professor of economics, probably had no idea just how timely his April 12 Watson Lecture would be for backing up a colleague. One of Noll's chief points was that earthquake prediction should be a routine and public matter. A week later James Whitcomb, senior research fellow in geophysics, rocked some segments of southern California with just such a "prediction."

Whitcomb said — publicly — that an earthquake of magnitude 5.5 to 6.5, with an epicenter near that of the 1971 San Fernando earthquake, is likely to occur within a year. When Whitcomb's statement made headlines, Noll's words were also on the record. "Defending Against Disaster" on page 2 is an adaptation of Noll's talk at Beckman Auditorium, and on page 8 "Testing a Hypothesis" is a discussion of Whitcomb's "prediction."

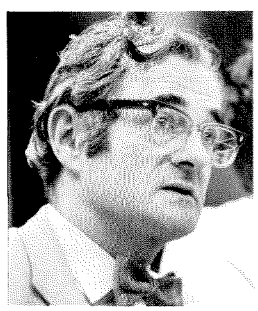


Noll

Brown

Voice of Reason

Not long ago, President Harold Brown was invited to address the Business Council's national meeting at Hot Springs, Virginia. His subject was not Caltech research, nor was it the SALT deliberations — though he could have spoken knowledgeably about either. He did draw on his experience and observations in those two areas, however, to make a careful evaluation of the pro's and con's of nuclear power. "Nuclear Power Plants — Weighing Benefits and Risks" on page 10 is adapted from that talk.



Sinsheimer

Handle with Care

In the last year or so, Robert L. Sinsheimer, professor of biophysics and chairman of the division of biology, has been increasingly concerned with the implications for human welfare of recombinant DNA research, and thus he has been a strong advocate for stringent safety regulations. But this is simply a specific instance of Sinsheimer's long-held general position that science and scientists are responsible to mankind. At a recent conference on Biomedical Research and the Public he spoke again on this subject. "An Inquiry Into Inquiry" on page 15 is a slight expansion of those remarks.

Throdahl

Good Management

The 1975 W. N. Lacey Lectures in Chemical Engineering at Caltech were given by Monte C. Throdahl, group vice president, technology, for Monsanto Company — and a lot of his listeners hoped that what he had to say could be shared with a larger audience. Throdahl was willing, but boiling two talks down to one article takes time, and he doesn't have much to spare for that kind of activity. Nevertheless, he turned to, and "Managing Innovation" (page 20) is the result.

It's a subject on which Throdahl speaks out of both conviction and experience. He has been with Monsanto since he joined the company in 1941 as a research chemist. He served as Director of Commercial Development, of Research, and of Marketing in one of Monsanto's divisions and became corporate vice president in 1964, transferring at that time to Brussels as general manager of the International Division. In 1966 he returned to St. Louis when he was elected a member of the company's board of directors and to its executive and technical committees. He was appointed to his present position in 1973.

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Defending Against Disaster

by ROGER G. NOLL

What we know—and what we don't know—about the adequacy of society's defenses against a major earthquake

PUBLIC POLICY-MAKING about earthquakes is very difficult for a number of reasons. A major problem is simply how to determine the magnitude of the threat. Devastating earthquakes are particularly difficult to plan for because they are extremely infrequent. Rational behavior is especially elusive to define when the threat is a tiny probability of a major catastrophe.

The first step in attacking the public-policy problems related to earthquakes is to state the magnitude of the damage in terms that make the threat of earthquakes comparable to other hazards that we face in everyday life. One such measure is the average annual destruction from earthquakes, which can be calculated by dividing the damage from major earthquakes by their frequency.

Unfortunately, because the earthquakes that we're most interested in — the ones that cause widespread damage — are so infrequent, we can make only the crudest estimates of the likelihood that one is going to happen this year. Probably on the order of once every 100-200 years an earthquake of major proportions, like the San Francisco earthquake of 1906, will strike a major population center in California. We can't be much more specific than that, because we don't have observations over a long enough period of time to say more.

A second part of the problem has to do with the nature of scientific inquiry into earthquakes. The people who study earthquakes, the seismologists and the geophysicists, do not focus their attention primarily on the earthquakes that government officials are most interested in for public policy-making purposes. Naturally, the scientists focus on the quakes that occur frequently enough to enable them to collect enough statistical data to test their hypotheses about the nature of the earth. And the earthquakes that happen most frequently are small ones that do little or no damage. As a result, estimates of the relationship between the frequency and the size of earthquakes are fairly good for quakes that public policy-makers don't care much about, but they're atrocious for the ones that matter most.

Another feature of the problem is that even if we knew how often earthquakes of magnitude 7.5 and up occur, we still would not know enough to estimate the damage they would cause. The relationship between the damage to a building standing in Los Angeles, and the magnitude number of an earthquake on the San Andreas fault is very loose. The exact amount of damage will depend upon the type of ground motion created by the earthquake, the time of day that it occurs, and numerous other uncertainties about the location and design of each building.

One way of getting at the likely damage is to examine the historical trends. The annual property loss from disasters of all kinds, including earthquakes, has risen fairly rapidly since 1900, for quite obvious reasons. We're a far richer society, and far more people live here than in the past. Still, despite the population growth that's taken place, the average annual number of people who are killed by a natural disaster has fallen quite sharply. Crude estimates of the expected annual death rate in California due to earthquakes come to about 30 people a year. That's a remarkably small number in comparison to accident rates from other things. For example, several thousand people are killed every year in auto accidents in the State of California.

The proper perspective to take in making decisions about the allocation of resources for safety is in terms of the relative magnitude of threats, and the threat of death from earthquakes is quite small compared to death by automobile accidents. We focus on earthquakes because a single event is so disruptive, possibly producing a larger number of deaths than numerous other threats that, over several years, claim a much higher toll.

To identify opportunities for improving public defenses against earthquakes it is useful to categorize the kinds of damage that will take place and the cause of each kind. Primary damage refers to the direct results of the quake — a building falls over on some people; a dam breaks, and people are drowned or property is washed away. Secondary damages occur after the earthquake, as a result of the disorganization of society that comes about because of it. For example, the earthquake might reduce the ability of the city fire department to fight fires; consequently, a substantial number of fires might go unchecked. Or it might disrupt the water supply, the sewage system, or medical care facilities, so that a few days after the quake an epidemic or other public health problem arises.

Contrary to the imaginings of the motion picture industry, the principal primary threat is not the collapse of buildings

Contrary to the imaginings of the motion picture industry, the principal primary threat is not the collapse of buildings. Since the 1933 Long Beach quake, building codes have required that structures be able to withstand a major shock. Furthermore, the common architectural style in southern California — one-story frame buildings — is ideal for rolling with the punch of even a major quake. Widespread structural collapse of residences does occur frequently in other parts of the world, and notably in Latin America, but the reason is that houses are typically constructed of adobe bricks. Masonry structures are extremely vulnerable to earthquakes, but are relatively rare in California. Actions to demolish these remaining old structures would relegate structural collapse to the status of a relatively minor problem.

In the Los Angeles area the major primary threat is that dams might break

In the Los Angeles area the major *primary* threat is that dams might break. If a substantial number of dams in Los Angeles or Orange County break and break quickly, either during the earthquake or immediately thereafter, about 30,000 people could, conceivably, be killed. But if no dams break, then, even in the year it occurs, the number of people killed by an earthquake will be fewer than the number killed by automobiles.

Historically, the next most important threat has been the secondary threat of fire. A not-so-commonly-known fact about the San Francisco earthquake is that about 95 percent of the damage was due to the fire that broke out after the quake, which the fire department was unable to fight because of inadequate water supplies. Fires are less of a threat in spread-out Los Angeles, but breaks in natural gas and petroleum pipes could still make fires a serious problem.

Another potentially serious problem is maintaining public health with effective relief programs. In addition to medical help for the injured, immediate actions must be taken in response to broken water mains, sewage pipes, and utility lines. A distribution system for water, food, and portable toilet facilities must be set up within a few hours of the quake, in addition to marshalling forces to get normal systems working again.

One public fear of earthquakes — commonly presumed to be true but actually false — is what might be called the disaster-movie syndrome. Panic and riots are expected to be the common reaction in the wake of a major natural disaster, and some public policy in the past has been predicated on such a belief.

But in fact this does not happen. In general, during a natural disaster and for the first minute or two afterwards, people see the situation in very personal terms. They see themselves as the focus of the disaster, and they see the major threat as personal, affecting themselves and their family and friends.

Their first action is to try to get home, to make certain that their family and friends are safe, and to be comforted by familiar surroundings and people.

The second response is to engage in constructive activities to cope with the damages in a direct sense — to keep busy, to work. Over a fairly long period of time — a few weeks — people try to come to grips with repairing the damage in a quite rational and straightforward way. Violent reactions or depressions that immobilize people come later, if at all.

It's interesting to compare the findings of social scientists with the actual response to the Alaska earthquake. The police department there held the incorrect view that the chief threat was looting, rioting, and chaos. So they strengthened themselves in the downtown area where, because store windows were broken, the potential for looting was high. But this kind of antisocial behavior never happened, and it was several hours before it occurred to someone that the police had nothing to do and could effectively be used to rescue people who were trapped in collapsed buildings.

I think the payoff for investing our resources in disaster relief is substantially higher than for making stricter building codes or taking other preventive actions

Meanwhile, several hours were wasted that could have been used to find and treat injured people.

Aside from assessing the magnitude of the damage correctly, a major policy problem is how to choose the extent to which one wants to be protected — that is, how much, in terms of property and human injury and death, are we going to try to save? The common view among public officials and engineers is, first, that the object is to save human beings from death or injury, and second, that the life-saving benefits cannot be valued in monetary terms. Hence, it is said that economic analysis is irrational or inappropriate for aiding in determining public policy.

This conclusion does not follow from the first two observations. It is certainly true that economic analysis cannot be used to determine an optimum policy; that is, it will not identify the right amount of earthquake defense to undertake, since the benefits of the program (lives) cannot be measured in the same units as are the costs (dollars). So I'm not about to tell you exactly how strict the building codes should be or exactly how much we should spend on disaster relief. But comparisons can be made of the extent to which additional expenditures in various safety programs would differ in the number of deaths and injuries they would avert.

Society has scarce resources to devote to safety, and ought to devote them to activities that are most effective. No one is about to propose that all of the gross national product, or every one of our working hours, ought to be devoted to nothing but protecting ourselves against hazards of all kinds. Because resources are limited, if there is a vast difference between the cost of improving the building code and the cost of, say, making automobiles and highways safer in order to save one more life, a serious public-policy question has at least been raised.

Some estimates have been made about how much money it would take to make buildings sufficiently safe so that the *annual* equivalent number of lives lost due to earthquakes would fall from 30 to fewer than 5 — in other words, to save perhaps 25 lives per year. That is really a big number. Multiply it by 100 or 200 (which provides an estimate of the toll from a big earthquake), and you're talking about several thousand lives.

It turns out that the value of a human life implicit in upgrading building codes, even though it amounts to only a 3-5 percent increase in building costs, is approximately $1 million per person. An intriguing cost comparison is with mandatory airbags for automobiles. It is estimated that airbags would save on the order of 10,000 lives per year at a cost of approximately $300,000 per life saved. Both figures, of course, are subject to considerable debate (just like the figures with regard to earthquake costs and benefits), but the orders of magnitude are probably right.

These figures suggest a strange public-policy dichotomy. Building codes implicitly place a far higher value on human life ($1 million) than do mandatory airbags ($300,000), and the latter is a policy which the nation has thus far been unwilling to adopt. Numerous other examples could be cited that would illustrate the same disparity.

The point of this is not to say that we definitely ought to have mandatory airbags, or that it's problematical whether we ought to upgrade building codes. The point is that we're not doing a very good job of rationalizing safety. We're not allocating resources among these alternatives so as to achieve the maximal saving of human pain and suffering from the resources we're devoting to it.

The third major policy area to be faced in making decisions about earthquakes deals with how to organize disaster relief effectively. Unfortunately there's not enough information to permit estimates of the number of lives that would be saved from more disaster relief than is currently available. As far as I know no one has even begun to ask that question in a sensible, coherent way, but I think the payoff for investing our resources in disaster relief is substantially higher than for making stricter building codes or taking other preventative actions.

Recently, in California, there has been a substantial increase in local and state government planning for disaster relief immediately following an earthquake. This is, it seems to me, quite laudable. Planning doesn't require much in the way of resources, and — by causing people to think carefully about how to use their resources if an earthquake comes — it can avoid grievous mistakes.

In the Los Angeles area the focus of these plans is primarily on the chain of command that will operate if a disaster occurs and the allocation of responsibilities for various kinds of activity. There's also some focus on maintaining communications so that the people who are at the top of the responsibility chain can have adequate information on which to base decisions.

While this is well and good, there are still some problems. First, there is a tendency to rely for disaster relief on highly structured organizations. And there's also a tendency to rely upon existing institutions to take on different and additional responsibilities in case of a disaster. One difficulty of this approach is that the more hierarchically structured an organization is, and the more complete are the rules and regulations governing its behavior, the less flexible it is likely to be in responding to a new, unusual, and unexpected circumstance. Unfortunately, we don't know what's going to happen if a major earthquake hits Los Angeles. We literally don't know exactly what kinds of damages will be suffered or what demands will be placed on public institutions. As a result, a substantial amount of decision-making flexibility will be needed.

In plans for the Los Angeles metropolitan area, much attention has been given to improving communications. Communications are important in a disaster, and a major problem is to maintain them within relief organizations. This is particularly true after an earthquake, when the normal channels are likely to be disrupted and when the scope of the damage cannot be known in advance. But the dilemma resides in the fact that too much information can be as devastating as too little. And if too much of the decision-making authority is at the top of the organization chart, then decision-makers spend all their time receiving information and very little of it making decisions.

This is not to say that organizations should not be hierarchical. It is simply to say that there is value in decentralization of some decision-making. For example, a simple issue is how to cache emergency supplies — medical equipment, water, food. One possibility is to have a relatively few large caches located where they're easily accessible to the people at the top of the organization, who will then order them to be distributed where needed in response to factual statements by people in the field about the extent of damage in various parts of the community.

Few people, in or out of government, know who will do what in case of a major disaster. That is a mistake that should be rectified

Another extreme is to have numerous small caches that are the responsibility of, say, local fire stations, or local police precincts, or even local community organizations (since people tend to congregate around their own neighborhoods). A counterpart to the old civil defense system could be set up, with civilians having emergency disaster relief equipment on a neighborhood basis.

Decentralization of some resources and responsibilities permits communication of less information to the people at the top of the emergency decision-making system. In any given earthquake, most of the Los Angeles area won't have much of a problem. The difficulty of learning who's in trouble and who isn't is substantially reduced if most of these areas can take care of themselves.

To accomplish decentralization of responsibility requires that plans for emergency relief, including those covering the distribution of emergency resources, be made known to government employees in the field and to the general public. As yet, planning information has not filtered down to any appreciable extent. Few

How should public policy-makers deal with this wonderful new technology being invented by the geophysicists and seismologists—predicting earthquakes?

people, in or out of government, know who will do what in case of a major disaster. That is a mistake that should be rectified.

Public policy-makers now face still another mind-bending problem. How should they deal with this wonderful new technology being invented by the geophysicists and seismologists — predicting earthquakes? This is a strange and wonderful witchcraft. Apparently certain kinds of earthquakes are very close to, if not now, "predictable." In this case predictable means that geophysicists can tell us with a reasonable probability what is going to happen. Instead of saying the chance is 1 in 200 of an earthquake of magnitude 8 occurring in L.A. this year, the scientists might be able to detect when the real probability is one in a million, and when it is 1 in 10, or 5, or 2. That's different, of course, from being able to tell you exactly *when* there's going to be an earthquake, but still prediction information is a significant gain over simple historical frequencies.

Of what possible use is this kind of information? How should we disseminate it? How should it affect decision-making. Some have criticized my courageous Caltech colleague, James Whitcomb (see page 8), for being so open about his research on prediction. They argue that this kind of information should not be made public because people will behave irrationally in response to it — they will evacuate the area in masses, or they will do all kinds of nasty things to each other.

History does not support this view. Studies of warning systems for bombings in World War II, and for tornadoes and hurricanes in the U.S., show that people behave calmly and rationally in response to predictions — particularly if they are used to them and know what they should do to protect themselves. All that need be avoided is sudden changes in the kind of information people are given, because they will not know instantaneously how to respond to it. I suggest that an official and regular process — like the weather report — be established. Perhaps once a week or once a month geophysicists would issue a press release on the current state of prediction. Usually the information released would be that nothing new is known, or that an earthquake of very small magnitude is predicted in some uninhabited area. This information is of no value directly, but it *is* useful.

If a geophysicist says that a magnitude 2.3 earthquake will hit eastern Riverside County (which would do no damage, even if anyone lived there), no one is going to change his way of life. But it helps people learn to make independent judgments on the quality of prediction technology, which is really the key to making rational decisions about such information. Right now no historical index is available to enable us to assess how fast scientists are learning to predict earthquakes. Society will start to pay attention as soon as it acquires enough historical information to judge the validity of prediction information.

At the present time there are some dangerous incentives *not* to make public predictions. One reason — the sort of perverse thing an economist would think of — is that the value of the information is far greater if it's private than if it's public. For instance, suppose you were the only person who knew there was going to be a recurrence of an earthquake of the kind that shook the San Fernando Valley in 1971. If you owned property there, it would behoove you to sell that property immediately and buy some somewhere else. That's a simple, straightforward use of the information. If everyone knows the information at the same time, however, it can't be used to take advantage of other people.

When the technology gets sufficiently good, all kinds of people and firms can be expected to want to have the information for their exclusive use. From the point of view of equity as well as efficiency, we should make the information public as soon as possible so that the private uses of it don't have any unfair economic consequences.

The second reason for not providing information is that if one's business is the scientific prediction of earthquakes, one will, for professional reasons, strive for scientific certainty before going public. Conservatism in evaluating experimental data is a necessary component of successful science, and because the data that are used in earthquake predictions are still sparse, the community of seismologists and geophysicists is reluctant even to talk publicly about prediction.

Of course, the requirements of scientific proof are often quite different from what we might want societally. Airplanes were built long before scientists understood flight, and wireless was extensively used before it was understood. More to the point, the earthquake in Oroville in August 1975 was predicted, but outside of a small group of scientists, no one knew it. Several geologists (not at Caltech) observed that some small earthquakes were occurring on a long-dormant fault. The situation was similar to one in which the Chinese claimed that they had been able to predict an earthquake, but the geologists who were aware of this situation had no real explanation for the mechanics of this kind of earthquake prediction. And they didn't feel that they should make a statement either to the scientific community or to the press that they were expecting a quake near Oroville. Several of them did station themselves in the area so they could see if anything happened, and an earthquake did indeed occur.

In this particular case there was no substantial damage that could have been avoided had the general public known the earthquake was going to happen. But if it had broken the Oroville Dam, the ethics involved in withholding this information from the public would, indeed, have been dubious. In fact, there is an interesting principle of liability law to the effect that if you possess information that another person could use to avoid damage to himself, and if you withhold that information from him, you may well be liable for that damage.

On those grounds there's not likely to be a lawsuit against people who predict earthquakes, because the technology is simply not good enough for any reasonably certain prediction. But the point remains that some individuals might well regard themselves as being able to make use of that information — or at least would regard themselves as being better off if they could see for themselves the extent to which prediction technology is improving, and thereby take their own action in response to it.

What can government do with prediction information? First, it can expect a couple of things to happen that will change its responsibilities. For example, the existence of prediction technology will have quite a devastating effect on earthquake insurance. If eventually earthquakes can be predicted accurately, the absence of a prediction will be valuable information, too, because it will mean that an earthquake is extremely unlikely, and so there is no reason to have insurance. If a prediction comes along, then insurance is attractive, but no sensible insurance company would sell it.

If that's the case, the demands are greater on government to act as an implicit insurer in the form of disaster relief programs. One of the consequences of the development of prediction technology is likely to be transferral of part of the responsibility for compensation for earthquake damage from the private insurance companies to disaster relief programs.

In addition, numerous little things can be done if a prediction takes place. Dams can be drained, and engines can be removed from the fire station so the firehouse won't collapse on them and make them inoperative. But the number of things you could or would want to do in response to this information is quite limited compared to the damage that would take place.

Perhaps the most important consequence of accurate predictions will be the stimulus they will provide to take simple precautions, such as bolting bookcases to the wall. Because major quakes are so infrequent, there is not much risk in postponing defensive actions for a little while. But with predictions, the risk will rise. Plans will be taken more seriously, and people will be more responsive to instructions regarding damage prevention and relief. Furthermore, predictions may well reduce the psychological stress caused by earthquakes by eliminating some of the surprise and the sense of helplessness one feels during and immediately after a quake.

No stupendous, all-encompassing response at either the public or private level will come about from the existence of predictability. When people have access to this information, and when most of it turns out to be about minor earthquakes that do not threaten them or require them to make immediately cataclysmic decisions, it seems reasonable to expect them to respond rationally. Thus, there is no particular reason not to make predictions public. Furthermore, there are all kinds of good reasons to make it public *now,* even if the people who could provide this information do not have the proper incentive to do so.

As nice as prediction is scientifically, as nice as it looks as a research topic today, the likelihood that anybody is going to get a substantial amount of benefit from it in the short run is very small. Even in the long run it is not too great. But the fears of giving this information to the public are completely without foundation. The best way to deal with predictions is to make them open and public as quickly as possible.□

TESTING A HYPOTHESIS

A geophysicist makes an earthquake "prediction"

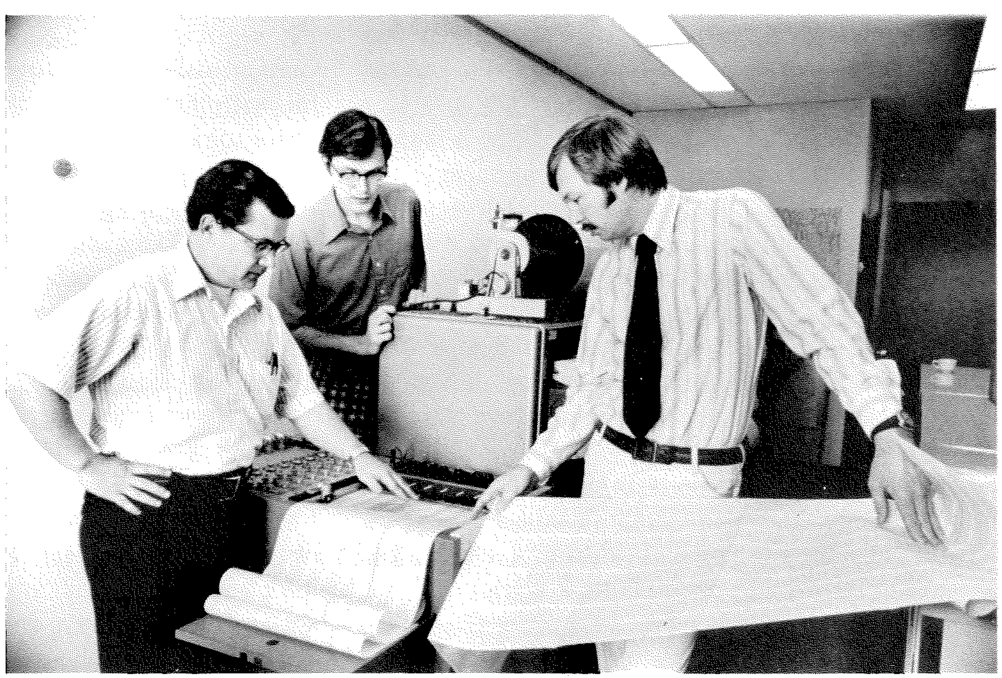
On April 21 it was widely reported in the press, and loudly broadcast on radio and television, that James Whitcomb, senior research fellow in geophysics at Caltech, had predicted that, sometime within the next year, southern California might have an earthquake with its epicenter somewhere near that of the 1971 San Fernando quake. Its magnitude would be somewhere between 5.5 and 6.5, which would be comparable with the San Fernando quake.

Though this news burst upon the general public, especially in southern California, like a small earthquake of its own, it was not at all a sudden announcement by Whitcomb. In fact, the data that led to the prediction had first been presented at a symposium held by the International Union of Geodesy and Geophysics in Grenoble, France, in September 1975, then offered in testimony before a congressional committee visiting southern California in October 1975. Interpretation of the data was presented in a paper before the annual meeting of the American Geophysical Union in Washington, D.C., in April 1976.

It was after that meeting that the press got wind of it, and began moving in on Whitcomb for confirmation and further information — to the point where he decided that the only way to get the information out accurately, and completely, was to make a general public statement.

It may not have been the first U.S. earthquake prediction, but it certainly was the first to get almost universal attention. By way of contrast, hardly anyone paid attention in January 1975 when Whitcomb predicted an earthquake near Riverside, California — even after one really did occur just a few weeks later in the town of Yucaipa. (The quake had a magnitude of 4, rather than the predicted 5 — which Whitcomb admits is further confirmation of the fact that his theory needs a lot more testing.)

Let it be noted that Whitcomb carefully tried to avoid



Checking the records in Caltech's seismological laboratory — Don Anderson, director of the lab; Roger Noll, professor of economics; James Whitcomb, senior research fellow in geophysics.

James Whitcomb appears before the California Earthquake Prediction Evaluation Council to describe the theory and data that led to his earthquake forecast.

describing his work as a "prediction." Again and again, he explains that he is simply testing a prediction hypothesis. Known as the velocity-bay theory, it was first proposed by the Russians in 1962. It is based on the observed slowing of seismic waves (naturally occurring ones, or those from quarry explosions). According to this theory, rocks along an active fault in the earth become strained as the land masses on either side of the fault slowly move in opposite directions. Eventually they develop countless hairline cracks. Seismic waves normally move faster through rocks than they do through air, but they slow down in rocks that have hairline cracks in them. Eventually — either because the cracks close again, or because they fill with water, or for other reasons still not known — the waves resume their former velocities. According to the velocity-bay theory, this is a signal that an earthquake is due. The theory also contends that the longer the period of slowed-down waves, the larger the quake.

Whitcomb's seismic studies showed a reduced velocity in seismic waves for most of 1974 and 1975 in an 80-mile-wide area with Los Angeles on the south, the Mojave Desert on the north, Fillmore on the west, and Mt. Baldy on the east. This area, which also contains the epicenter of the 1971 San Fernando quake, happens to be one where seismic instruments are in operation — and where, therefore, measurements can be made. There are, of course, countless other areas for which no such information is available.

The effects Whitcomb is now observing in this area were present before the 1971 quake, and similar effects have been observed before other quakes as well.

"Our experience in interpreting this kind of velocity anomaly is very limited," says Don Anderson, director of Caltech's seismological laboratory, "but there have been at least six examples where moderate earthquakes have been preceded by a similar effect. As Whitcomb has often pointed out, we have no information on the false alarm rate, or how often such an anomaly occurs without it being followed by an earthquake. And the magnitude assignment is based on limited previous experience. We are still in the learning process."

The California Earthquake Prediction Evaluation Council considered the Whitcomb forecast (the first it ever *had* considered) in a public meeting held on the Caltech campus on April 30. The council is made up of professional earth scientists from public and private California universities, state agencies, and the U.S. Geological Survey. After limited study, the eight-man council "did not conclude that the probability of an earthquake in the area in question is significantly higher than the average for similar geologic areas of California.

"Nevertheless, the data are sufficiently suggestive of such an increased probability as to warrant further intensive study and testing of the hypothesis presented by Dr. Whitcomb."

Further intensive study and testing is just what Whitcomb wants. In fact, his "prediction" is one more reminder of the fact that the development of an accurate and reliable earthquake prediction system is not just a local, or national, but an international concern.□

Nuclear Power Plants—

Weighing Benefits and Risks

by HAROLD BROWN

CONSUMPTION of energy on our planet, and particularly in the United States, has risen at a rate that has become frightening in view of the limited resources of the earth. Despite embargoes and quintupled prices of imported oil, we have not yet focused on the realities of current shortages, the probabilities of energy famines, and the need both for conservation and for developing new or expanded energy sources. I believe that what happens about these energy sources—and to economic development in the U.S. and the world during the next century—will be determined largely by what we do in the next decade about nuclear energy.

No other energy source is subject to the variety and severity of controversy to which nuclear power is currently exposed. The issue is not merely the extraction of raw material, nor even inhibitions on the operation of power plants. In some cases the proposals would virtually forbid nuclear generating plants altogether. California's ballot this month has the best-known proposal, and its provisions are highly restrictive. Twenty-seven other states also have anti-nuclear legislative activities or voter initiatives.

The uncertainty as to what limitations are to be imposed in terms of environmental impact, or of safety, prevents a sensible design, development, and production schedule, greatly interferes with raising capital, and disrupts efforts to foresee needs for transmission and distribution of electrical power. However, during the next decade, decisions are going to have to be taken either consciously or by default on the sources of energy that will have to be used during the following 50 years. A central choice is the degree to which nuclear energy should be employed, and that will depend partly on public attitudes and on the ability to make political decisions.

The situation is extremely complex, but there *are* some key questions whose answers should determine policy in this matter. I believe that only when these are understood can public attitudes be informed ones, and only then can even a courageous leadership make the appropriate decisions.

There is clearly a connection between energy consumption on the one hand and economic and social well-being on the other. Quality of life includes GNP, pollution levels, and many other components. But specifically at issue here, in the observed connection between per capita GNP and per capita energy consumption, is: Which is cause and which is effect?

I conclude that each is both. In particular, there is a considerable waste of energy in homes and offices. On the other hand, for industrial production and probably for agriculture, as well as for much of the service sector, marginal returns on energy use are high. The value and productivity of labor in those areas are highly dependent on per capita energy consumption.

Reduction of consumption in existing residential and commercial structures is feasible, and so is an even greater reduction (perhaps 30-40 percent) through sensible redesign; in manufacturing and agriculture possible reduction is almost certainly less. Taking the economy as a whole, we may have a 25 percent or more

We have not yet focused on the realities of current energy shortages and the need for both conservation and expansion of our sources. What happens will be determined largely by what we do in the next decade about nuclear energy

cushion of waste. By increasing efficiency and by lowering consumption for marginal use, much of what would otherwise be necessary growth in energy requirements can be replaced during the next couple of decades without any substantial loss of economic well-being.

Perhaps we can thus cut the per capita U.S. growth rate in half—from 4 percent to 2 percent per year for total energy, from 8 percent to 4 percent in electrical energy—in the 1975-2000 period. However, even the reduction during 1974-75 in the rate of annual growth of energy consumption per capita to about half its former value is in substantial part responsible for the current—or recently concluded—recession.

There is, I believe, no acceptable way to take care of our economic needs for the next 50 or 60 years through energy conservation alone. We will have to find other sources both to meet increased needs and also to replace much of the present consumption of oil and natural gas, which together now comprise over 75 percent of the U.S. energy consumption mix. To anticipate a bit, I am convinced that the only realistic sources until well into the next century are fission reactors and coal.

In the U.S., and in the rest of the developed world, a rather modest rate of growth of per capita energy consumption can allow or even improve economic well-being. But anything other than a Malthusian solution to the inhuman poverty of the fourth world will require a large increase in per capita energy consumption. And total world reserves of fossil fuel won't even come close to providing the necessary energy base.

Nuclear energy is not a forseeable substantial mobile energy source. For these uses, either natural or synthetic hydrocarbons are by far the most advantageous, but the world's supply of the natural ones will probably be nearly gone within the next 30 to 35 years. Synthetic fuel from coal, shale, or tar sands may begin to be available then or earlier, but it will have to be saved for use in mobile power plants (autos, airplanes, etc.) and for feedstocks in petrochemical production. Indeed, I expect such restriction of its use to take place well before the end of this century.

For fixed power plants, coal and uranium are the principal sources available at least up to the year 2000. Sufficient reserves of each exist within the United States to supply the needs at present rates of stationary power generation beyond the year 2000, assuming that we go to a 50-50 mixture. The U.S. has about one-quarter of the world's supply of coal, enough for more than 200 years supply. Even without a nuclear breeder cycle, a comparable supply of energy exists in native uranium.

At least three other energy sources are possibilities. The first is geothermal energy, which can and should some day provide a small portion of the world's energy.

Solar power is a large potential source, but unfortunately it is at a very low level of concentration. However, it can soon be used effectively and at a reasonable cost to heat water and to heat and perhaps cool homes and offices in sunny portions of the world. The concentration of the sun's rays over large areas to produce high-temperature thermal energy, and hence electricity, through boilers or turbines is more distant in time. Direct conversion into electricity is further off still. I do expect that—by the end of this century or early in the next—solar methods would be able to provide perhaps 5 percent of stationary energy production.

The energy from controlled nuclear fusion is a fair prospect to begin playing a part in stationary power production early in the next century. Only now, I believe, are we within a decade or so of showing the feasibility of a nuclear fusion machine that puts out more energy than it consumes. Therefore, we have only just begun to consider the engineering problems that go with fusion plants. My own judgment is that these difficulties will approach those of fission. The environmental problems (including radioactivity) and even those of international control will also be far from negligible.

Thus, early in the next century, solar energy, and geothermal and fusion energy will just have begun to contribute to stationary power plant generation. Other possible exotic sources of energy also exist, but even the possible ones can have no significant effect during this century. The coming generation's needs will have to be powered either by fission energy from uranium, by coal, or by a combination of the two. These two are therefore a natural pair to compare in terms of economics, availability, and environmental and other hazards. Both uranium and coal are plentiful, but not unlimited, within the United States, so there are advantages to preserving coal for the production of synthetic fuel (liquid and gaseous) and for hydrocarbon feedstocks. To expand the contribution of uranium to central-station electric power production will take a considerable effort, but it is feasible.

We are within a decade or so of showing the feasibility of a nuclear fusion machine that puts out more energy than it consumes

At present about 8 percent of U.S. energy is generated in nuclear plants; this corresponds to less than 2 percent of our total energy consumption. The Energy Research and Development Administration's projection is for 25 percent of electric power to come from nuclear plants by 1985. I think nuclear energy could provide well over half of the electric power generation by the year 2000. Moreover, I believe that this can be reached with little or no operation of breeder reactors.

Nuclear breeders are feasible, but particular designs may well take 15 years or more to prove out, and an operating cycle of about a decade is required to double the usable fuel in a breeder reactor. The price of uranium ore is $24 per pound (triple its recent value), which makes its contribution to the cost of nuclear energy a bit more than 2 mills per kwh. The price could go to $100 per pound without markedly affecting the economics of nuclear power. However, such a price would greatly increase the availability of uranium, which is a key factor in the question of when and whether breeders will be needed to carry us to the fusion and/or solar age.

Space here does not permit a detailed economic comparison between coal-fired plants and fission reactors. Past nuclear capital costs have been close to those of coal-fired plants (but various hidden subsidies were present). The comparison is complicated by: the recent rapid rise in capital costs of coal-fired and especially of nuclear plants; the effect of construction time on the interest costs associated with construction; the difficulty of extrapolating these trends to the future; the uncertain added costs of environmental and safety precautions for both; and the (inverse) linear effect on capital costs per kilowatt-hour of the fraction of the time that a plant operates at full power.

My examination of capital, fuel, and operating cost factors suggests to me that the costs per kilowatt-hour of coal and nuclear power will be fairly close late in this century. The most probable cost for nuclear power appears somewhat lower, but the costs of added safety precautions that may be imposed could reverse that relationship.

In the light of these economic and other factors, I place a high priority on the need to consider various aspects of safety in the operation of power-producing converter reactors.

The crucial issue of safety falls, for nuclear reactors, into four categories: (1) health and safety for workers and surrounding population in normal operation; (2) safety against and consequences of release of radioactivity in some form of reactor accident; (3) problems of processing and of long-term storage of the highly radioactive spent fuel; and (4) the problem of diversion of enriched uranium or plutonium to weapons purposes.

The first of these, the environmental effect of normal operation, is actually far less than it would be for a plant burning fossil fuels at the same site. The chemical pollutants (hydrocarbons, oxides of sulphur, nitrogen, carbon) are absent. Waste heat is comparable. And nearly all radioactivity is contained on-site.

Each of the remaining three categories of safety issues is a serious one, but—with the possible exception of the last—I think they are all manageable.

Extensive (and conflicting) calculations have been made about the probability of reactor accidents that could release into the atmosphere various amounts of radioactive by-products—not by the reactor blowing up, but by its melting down. We are not sure what the effects would be on the population of small doses of radioactivity, administered over many years, resulting from an accident, but there is a generally accepted upper limit.

The most serious problem arising from nuclear reactor power is that of nuclear proliferation. This is a severe threat to the peace and security of the world

For a typical population density in the area extending to 500 miles from the nuclear plant, this upper-limit assumption indicates that a very severe but also very rare accident could produce, over the 50 years following such an accident, as many as 10,000 long-term deaths from radiation if individuals did not move out of the area and if the difficult task of decontamination was not carried out. This is a large absolute number, but it looks rather different if one multiplies it by the one-in-a-million estimates of probability of such an accident per year of operation that some analysts have made. There have been no meltdown accidents at all. Experience of actual operation approaches the one-in-a-few-hundred probability level as an upper limit—none has happened in that much operation, even in terms of much smaller release of radioactivity. It is, in fact, very hard to get a good estimate of the probability of such unlikely accidents. However, the fraction of the radioactivity emitted in any accident can probably be reduced by such actions as placing the nuclear reactor underground.

Should such a very severe but very unlikely accident occur, the probability that an individual who continues to live in the exposed region will die of cancer induced by radiation from the accident is, making a worst-case linear assumption, one chance in a thousand, with the cancer manifesting itself at some time during several decades following exposure. This is to be compared with a present probability of about one in five—200 times as much—of dying of cancer induced by other natural or man-made causes.

It appears almost certain that the sulphur content of burning coal in central power stations, even if desulphurized coal is used, is much more hazardous than the effect of nuclear plants in normal operation. The same relation may apply to accidents at a fossil fuel plant compared with those at a nuclear plant. We can't say for sure because the long-term effects of low-level exposure to sulphur-dioxide (or of the sulphuric acid to which it can be converted in the presence of ozone particulate matter in the atmosphere) are even less well known than the effects of small quantities of radiation. Thus, there are unanswered questions about accidents and safety. But comparison with the effects of other energy sources available during the next 50-75 years suggests that nuclear energy need not be more dangerous.

The problem of finding an acceptable storage method for spent nuclear fuel that will be safe for thousands of years is not a trivial one. Yet there are some methods (including storage in salt domes) that appear very likely to work.

Meanwhile, one should not ignore the possible long-term effects of expanded use of hydrocarbons. Their combustion produces carbon dioxide, which has some risk of producing a greenhouse effect, increasing earth's surface temperature, ultimately melting the icecaps and raising the sea level by 100 feet, with catastrophic effects. This is not demonstrably more unlikely than the biggest nuclear reactor accidents, and would be much more severe in its consequences.

The most serious problem arising from nuclear reactor power is that of nuclear proliferation. By this I mean the acquisition of quantities of weapons-grade fissionable material, sufficient to produce nuclear explosives, by countries not now possessing them, or by non-governmental groups of terrorists, gangsters, or others. This is a severe threat to the peace and security of the world and of each country in it. If the U.S. could eliminate or greatly decrease this threat by foregoing or delaying construction of more nuclear power plants, I think we should seriously consider doing so, using coal, and trusting that pure fusion or solar energy will become economic before U.S. coal is used up.

But when one looks at the rest of the world, this approach appears rather naive. Western Europe has about one-fifth as much coal as the U.S., Japan virtually none. For those nations to delay massive use of nuclear reactors into the twenty-first century is economic suicide—which they won't commit. Therefore, trying to forbid reactors, or abstaining from using them in the U.S., will surely fail to prevent nuclear proliferation. We should work instead on rigid control. In that attempt, I think we have substantial leverage. This is our best real hope—though far from assurance—of keeping nuclear weapons and dangerous radioactivity from those even less responsible than their present possessors.

The "vendor nations"—the Soviet Union, the U.S. and Canada, France, Germany, Britain, and Japan—have common interests in establishing close controls to prevent unauthorized possession. Any solution, of course, will have to be principally a political one. The problem can be solved, if at all, only by cooperation among nations. In cases where the reactors or fuel are made in the U.S. or with our help, we can insist—and put pressure on the other vendor nations to join us in this insistence—on limiting any processing facilities for the spent, plutonium-containing fuel to internationally supervised regional (not national) plants, preferably in countries that already have nuclear weapons. The availability of, and the will to use, very strong political sanctions against countries that refuse to accede will be an important factor.

For this approach to work, it may be necessary to postpone the use of breeder reactors, which depend on recovering plutonium. A small risk in doing so is that we might run out of coal *and* medium-grade uranium ore before we can make available enough power capacity from solar energy and pure fusion. I would incline toward going slowly on the operation of plutonium recycle plants and of breeders while we seek rigid controls against proliferation. But I would go ahead with their development. This would allow breeders to be activated some decades before other fuels run out, if an energy catastrophe is then judged more dangerous than the increased risk of diffusion of plutonium.

Since only technologies now in being (coal, oil, natural gas, solar energy for heating and cooling but not for electricity, converter reactors) can give us substantial quantities of energy by the year 2000, now is the time to pursue strong development programs in a number of other areas. These include fusion, solar energy, geothermal energy, and fission breeders—all for stationary electric power production; and conversion of tar sands, shale, and coal to liquid and gaseous hydrocarbons. The nuclear and other energy industries and the utilities cannot successfully convince the public of the need for such development programs on their own. Their views are understandably discounted as being influenced by their own interests. However, if what I have said is a sensible way of tackling the problem, then explaining it ought to be an educational process that is within the capacity of our business, governmental, educational, and scientific leadership.

We need to pay more attention to the problem of understanding hazards in general. Our society has not reached agreement on how to weigh the benefits against both such general risks as proliferation and the individual risks of serious accidents of very low probability but high potential damage. We have not determined accurately (though we have a fair upper limit on) the effects of low-level, long-time exposures to nuclear radiation and (less well) to chemicals or other environmental products. Most important, we lack an effective framework—academic, business, and political—for the decision-making process itself. Creating it will not be easy, but only when it exists can our arguments at least be about the right questions.

Among these matters, questions of the need for nuclear plants and how (and whether) they can be made safe will probably be the first to be decided. The ballot initiatives that have been advanced strike me as not the right way to make the decision. But the debate that they inevitably initiate is a vital opportunity to influence in a positive way our pattern for deciding future questions of new technology, economic need, environmental health, and other hazards—as regards not only nuclear energy but coal, other energy sources, and other issues as well.

I think that these decisions can be taken so as to be consistent with resource conservation, to give as good assurance of environmental safety and international security as any practical alternative, and to preserve and even raise—slowly—the standard of economic well-being of mankind. □

An Inquiry Into Inquiry

—Some Questions to Discuss

by ROBERT L. SINSHEIMER

For a scientist to challenge freedom of inquiry is akin to heresy — but is inquiry itself to be exempt from inquiry?

THE RIGHT of free inquiry was hard won. It has served us well. Surely, any who would restrain inquiry should give full cause.

Indeed, for a scientist to challenge freedom of inquiry is akin to heresy — if not suicide. But is inquiry itself to be exempt from inquiry?

The stage of history does change. When knowledge was scant and technology feeble and the art of inquiry itself an infant, full freedom of inquiry could be readily championed. Today science and technology — born, to be sure, of free inquiry — have transformed our world and have given us great powers. The art of inquiry itself is now mature and deeply penetrating. And a sober and reflective reevaluation of the purposes and consequences of inquiry may well be in order.

It may be that the highest wisdom is to recognize that we should not trust ourselves to civilize the course of inquiry. Human history from the Inquisition to Lysenko suggests the probability of abuse of such power.

But it is at least instructive to consider the alternative, and it just may be imperative. Restraint can mean guidance and pacing, not eternal prohibition. Curiosity is not necessarily the highest virtue — and science, the distillate of curiosity, may not merit *total* commitment.

To be meaningful, an inquiry into inquiry should provide specific instances. From such instances some generalizations may be possible.

For what specific purposes might we wish to limit inquiry? Do we wish to curb only the means or even the ends of inquiry? I expect each person might devise his own list, but let me advance some suggestions.

One is the preservation of human dignity. We should not do experiments that involuntarily make of *man* a means rather than an end. The ethics of human experimentation are, I think, now rather well understood — even though it must be recognized that such restraints blunt pure inquiry.

Another reason to limit at least the means of inquiry is the avoidance of involuntary hazard, physical or biological. As might be expected, the level of hazard which demands restraint will be arguable. We have already one instance of such limitation in the — not universally accepted — ban on atmospheric nuclear testing. We can all recall the controversy which preceded adoption of this ban.

The field of recombinant DNA research has an analogous potential for widespread, inadvertent danger from the leakout of possibly toxic organisms; a danger even more difficult to quantitate, so that the limited precautions already proposed are certain to be the subject of continued controversy. This hazard — posed by the invention of synthetic biology — has a novel aspect. Unlike fallout or DDT, it is potentially irreversible, for synthetic living organisms are, by definition, self-reproducing.

We may be lucky; Nature may again protect us from our ignorance. I personally dislike to leave such a grave consequence in hostage to fortune.

Yet another reason to limit inquiry may be the sheer cost of a research project. This issue introduces a new perspective. In the previous instances we were concerned only with the means of inquiry and did not question the ends. The constraints suggested to be imposed upon inquiry derived from commonly accepted ethical principles, although, as always, one might quarrel with their specific application.

By introducing the element of cost one asks if the primary consequence of the inquiry — the knowledge to be gained — is worth the expenditure of talent and time and resource. Decisions as to the allocation of resources are usually, and properly, left to the political sphere. However, scientists are also citizens — and despite our enthusiasms we should endeavor to be at least dimly aware of the realities of competing concerns. For instance, I would find it difficult in today's world to justify, in terms of the benefit to science, the expenditure of 100 billion dollars to land a man on the Martian planet. Such extreme enthusiasms might at least be tempered with common sense.

Is it useful to be able to predict the latent consequence of genetic defects if we cannot avert or mitigate their effects?

Progressing ever deeper into controversy, one may extend inquiry into the ends of inquiry to question, in particular instances, whether we want to know the answer in any case — whether the secondary consequences of such knowledge (given the nature of man and of human society) are, on balance, likely to be beneficial.

Here it may be that the highest wisdom is to recognize that we are not wise enough to know what we do not want to know — and thus to leave the ends of inquiry unrestrained. Indeed, I expect there are only a few instances where prudence would be in order. But the set may not be null; let me present a few examples for consideration.

I would suggest that the temporal order of scientific inquiry deserves some thought. It is usually conceived that the stream of scientific advance follows a linear course, dictated by the internal logic of each discipline — that is, by the availability at any time of knowledge and technique. To which I would add, also by the available inspiration, which in turn is closely coupled to motivation. And to this extent I conceive that the pattern of development of science is not wholly innate or preordained — that we are not tracing out in an inevitable web.

If so, then in particular it would seem to me desirable to keep some proportion between our predictive capabilities and our deflective capabilities. Is it useful to be able to predict the latent consequence of genetic defects if we cannot avert or mitigate their effects? Is it useful to be able to predict the approximate date of an earthquake if we cannot appreciably spare its consequence? And reciprocally, we have need to be able better to predict the aftermath of our interventions into Nature before they become too gross — as with fluorocarbon propellants or perhaps with recombinant DNA.

But some directions of inquiry carry within themselves the seeds of what I would label social hazard or, perhaps, just plain mischief. In this case the inquiry itself is not really hazardous, but the almost certain social consequence most assuredly is. And this hazard would seem likely to far outweigh any foreseeable social benefits.

Now there is a hallowed and traditional point of view that it is the business of the scientist to inquire, to discover new knowledge. It is not our concern but society's what use, if any, to make of that knowledge.

In my view, in our world as it is, such a position is very largely a cop-out. In general, our society lacks both the means and the will to avert the development and use of the products of scientific discovery.

We live in a free enterprise society. Any development that provides gratification or can yield a profit or is deemed to strengthen the national defense will most often be adopted, frequently with remarkable speed. And I am not sure we would care for, nor science thrive in, a society which had the extensive control system necessary to prevent such applications.

At the same time, we do live in a strained society of uncertain elasticity. It is a part of rationality to recognize that mankind harbors always the potential and the reality of irrationality. There *are* arsonists and assassins, terrorists and tyrants.

Let me give four selected illustrations of research whose likely consequences would seem to me to be major and to be at this time in our society of appreciably less advantage than harm. It may be that these are but personal crotchets. But I believe these merit discussion, *before* the experiments are done. One example is from radio astronomy, one from physics, two from biology.

We have heard many proposals that we should attempt to contact presumed "extraterrestrial intelligences." I wonder if the authors of such experiments have ever considered what might be the impact upon the human spirit if it should develop that there are other forms of life, to whom we are, for instance, as is the chimpanzee to us. Especially devastating, it seems to me, would be the impact upon science itself, once it were realized someone already knew the answers to our questions. We know in our own history the shattering consequence of the impact of more advanced civilizations upon the less advanced. In my view the human race has to make it on its own — for our own self-respect.

Research upon improved, easier, simpler, cheaper methods of isotope separation. Result: slightly cheaper power, far easier bombs. Is that, on balance, in anyone's best interest?

Research upon a simple means for predetermination of the sex of children. Result: some boon to animal husbandry; boys or girls upon parental request — and the potential for a major imbalance in the human sex ratio. Is this disruption of a balance already provided by Nature really a desirable advance?

Indiscriminate research upon the aging process. What is the long-range purpose of aging research? The purpose of cancer research is clear — the eradication of cancer. Is the purpose of aging research the eradication of aging? None would quarrel with research to relieve the infirmities of old age. But in the section "Purpose of Legislation" in the House Committee statement accompanying the Research on Aging Act of 1974, it is stated, "This Institute (the National Institute of Aging) will provide a natural focus for the research necessary to achieve the great goal of keeping our people as young as possible as long as possible." Is this on balance, a desirable goal?

By now I have probably cited enough instances to have trod on at least one toe of every reader — thereby proving the truth of my earlier cautions. But more seriously, the point is that the role of science — which is our principal organ of systematic inquiry — the role of science in society has changed in the course of the 20th century, although our perceptions have not kept pace. It has changed because of the success of science itself. In the nucleus of the atom and the nucleic acids of the cell we have discovered the core of matter and energy and the core of life. These discoveries place in our hands immense powers, far beyond human scale and experience.

In consequence, I think there are limits to the extent to which we can rely upon the resilience of Nature or of social institutions to protect us from our follies and our finite vision. Our thrusts of inquiry should not too far exceed our perception of their consequence. There are time constants and momenta in human affairs. We need to recognize that the great forces we now wield might drive us too swiftly toward some unseen chasm.

The very success of science has ended its pleasant isolation. The impact of science and the increasing coupling of science to human affairs do encumber us with new responsibilities. Yet at the same time we do not wish to shackle inquiry with the bonds of responsibility. Somehow we need to find a way to be doubly responsible, both to mankind and to science, as one of man's finest creations. That will not be easy.

Afterthought

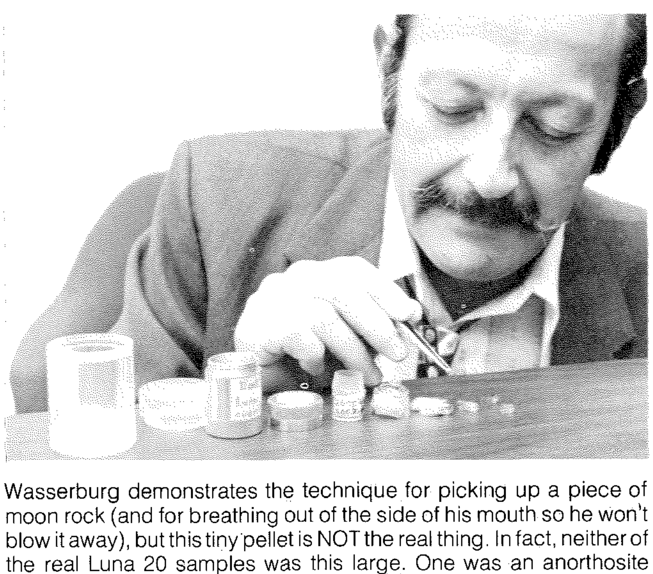
A far more pervasive (and insidious) rationale for the restraint of scientific inquiry will likely derive from the phase transition from the spontaneous to the planned society, from past loose-jointed self-reliance to future tightly integrated interdependence. Planning is invasive; once begun in one sector, it tends to expand inexorably to adjacent sectors of the social enterprise, lest their unplanned fluctuations perturb the adopted plan.

In the fully planned society, change and innovation must be regulated, and thus science itself — as the fountainhead of change — will be carefully channeled and metered.

Spontaneity (essential to the scientific enterprise) and crystallinity (essential to the planned society) can only coexist within narrowly determined conditions. It may become a most important task for scientists to help define those conditions. □

Souvenirs from Russia

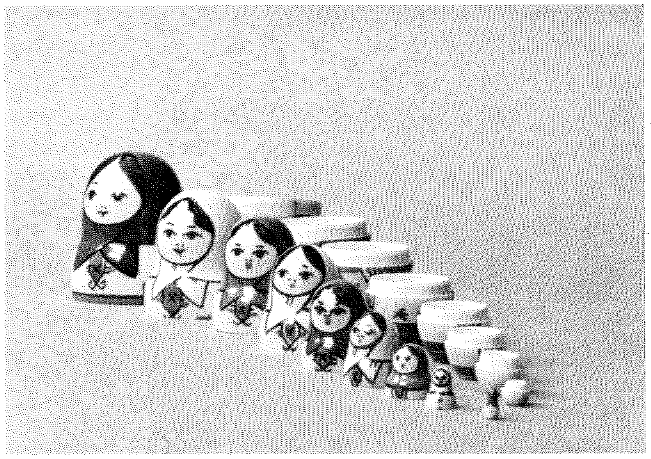
One of the central tenets of the search for scientific truth is that knowledge is to be shared — a fairly complicated process when moon rocks and international boundaries are involved. Take transmitting samples from the Soviet Luna missions to the moon to Caltech's surgically clean laboratory known as the Lunatic Asylum, for example.



Wasserburg demonstrates the technique for picking up a piece of moon rock (and for breathing out of the side of his mouth so he won't blow it away), but this tiny pellet is NOT the real thing. In fact, neither of the real Luna 20 samples was this large. One was an anorthosite particle that weighed approximately 42.6 milligrams, and the other was a piece of basalt that tipped the scales at 36.8 milligrams. "This is really at the bottom of the line in size," says Wasserburg. "If you make one slip, you can lose everything. It's not impossible technically; it's just very, very difficult."

For the group of geologists, geophysicists, geochemists, and planetary scientists of the Lunatic Asylum, working with lunar rocks began several years ago with the acquisition of samples from the American Apollo missions. Their work on those stones made them known as one of the world's prime laboratories for the analysis and age-dating of lunar materials. And in the eyes of NASA's Lunar Sample Analysis Planning Team (LSAPT), it also made them logical recipients for the prime "boulder" samples (see far right) from the two Soviet missions to the moon—Lunas 16 and 20—which were obtained via the USA-USSR exchange agreement.

In September 1970, Luna 16 brought back to the earth from the northeastern section of the moon's Sea of Fertility about 100 grams (3½ ounces) of lunar regolith (surface rock fragments and soil). The Lunatic Asylum's share was a basalt pebble that weighed about 62 milligrams, and two small pinches of soil. Using their own special skills and techniques, the Lunatic Asylum inmates were able to determine both the age and the composition of the samples, relate their findings from this very different lunar locality to those from earlier Apollo missions, and add a few more pieces to the lunar jigsaw puzzle. Their results pleased and impressed the Russians enough to have the reports translated and reprinted in a book along with some of their own findings.



Each of these *matreshki* nests inside the next larger one, and each represents a separate generation. A traditional Russian toy, this set is a souvenir of G. J. Wasserburg from a trip to the USSR in September 1973, when a group of Caltech faculty, President Harold Brown, and members of the Board of Trustees and their wives visited the Soviet Union as guests of the Soviet Academy of Sciences of the USSR. Wasserburg now has a modern version of the *matreshki* (right).

This series of containers once held souvenirs of another kind of Russian trip — moon rocks from the Luna 20 mission. A member of the USSR Academy of Sciences handed the assembled package to a representative of the U.S. Embassy in Moscow. From there, it was hand-carried to Houston and placed in the care of Caltech alumnus Michael Duke, curator of lunar samples for NASA. Duke brought the unopened container to Caltech.

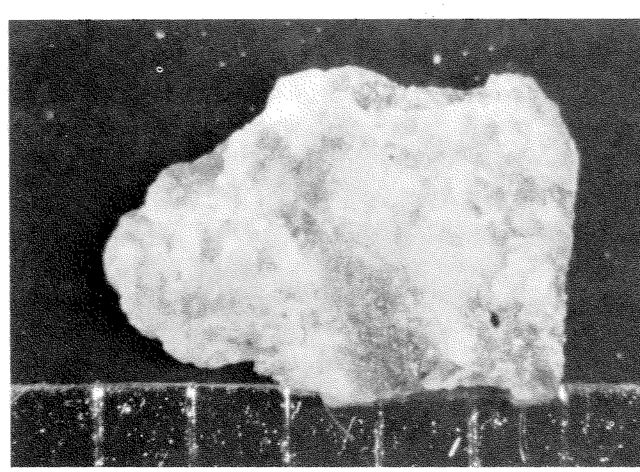
After Luna 20 returned from the moon in February 1972, the Caltech group received a small portion of the 50-gram core from its landing site in the highlands to the north of the Luna 16 site and some distance from the mare basins, which are flooded with basalts. The highland areas represent the largest fraction of the lunar surface, but so far have not been extensively sampled.

This first Luna 20 sample was dated by John C. Huneke, senior research fellow in planetary science, and co-workers at close to 4 billion years (3.90 AE). Study of that material, which was mostly moon dust and only a few pebbles, whetted scientific appetites for another generation of experiments, preferably with bigger rock samples. Recently, after more than a year of negotiation, two additional fragments were transmitted to the Lunatic Asylum.

Once the rock samples actually reached Caltech, a series of painstaking steps were followed. Michael Duke, curator of lunar samples for NASA, Gerald Wasserburg, professor of geology and geophysics, and D. A. Papanastassiou, senior research fellow in planetary science, opened the plastic bottle, that enclosed the aluminum case, that held a glass-weighing vial, that cradled two gelatin pill capsules, which contained smaller gelatin pill capsules, in each of which rested a fragment of lunar rock.

Each capsule was carefully slit with a razor blade, and the rocks were lifted out, weighed, inspected under a binocular microscope, photographed, cleaned of moon dust, photographed again, and chipped. Joseph Brown, associate research engineer, made a polished thin section. A grain mount was made of dust washed from fragments.

A preliminary chemical investigation was then made over a period of several weeks. Finally, a detailed experimental plan was submitted to Michael Duke and LSAPT. It was approved in mid-February. Since then the rocks have been further divided and subjected to scores of tests, including analyses of micro-thin sections by Arden L. Albee, professor of geology. All these investigations have almost totally destroyed the material — but they are expanding our understanding of the moon. □



Lined up against a millimeter scale, a piece of lunar anorthosite measures just about 2½ millimeters — about half the size of a grain of rice. This is the larger of the two Luna 20 rock samples, each of which has now undergone analysis and age-dating procedures by a consortium of scientists at Caltech, Oregon State University, and the University of Chicago.

Managing Innovation

by MONTE C. THRODAHL

In large organizations the management of innovation is not an option— it is an obligation

MANAGING INNOVATION may appear to be a contradiction in terms. "Managing" implies dealing with known quantities, following procedures, observing rigid orders. "Innovation" suggests ventures into the unknown, techniques that are unconventional, breaks in known patterns.

Management in large organizations is frequently accused of a lack of innovation, yet it is precisely in these organizations that high-technology innovation is most frequently found. Therefore, despite apparent contradictions, managing innovation is not a management option—it is an obligation. Management must deal effectively with innovation, and innovation must be channeled toward profitable ends.

The joining of management and innovation, like any union, requires definition, understanding, and often adjustment by both profit-minded managers and technology-oriented innovators. Management, comfortable in the day-to-day arena of the familiar and the predictable, must learn to deal with *undefined* problems, where decisions and solutions are not always based on convenient rules developed through experience.

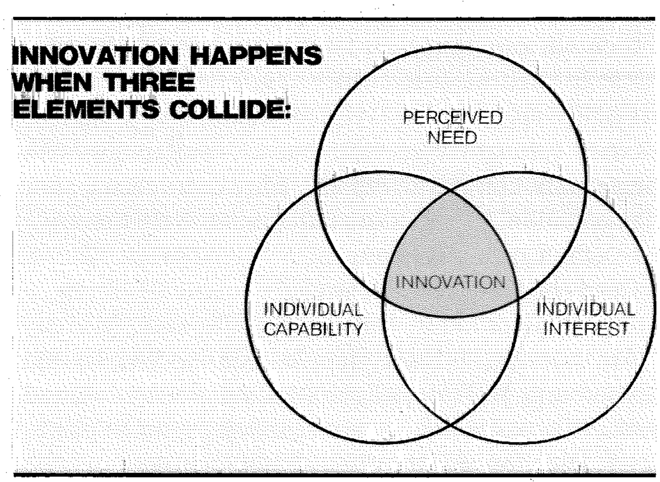
Management must face these problems in such a way that the problem itself is solved, not a symptom of the problem. Both managers and technical innovators must have the flexibility to work out alternate solutions, and be ready to modify these solutions as conditions change.

A clear understanding of innovation is critical. Innovation is not simply an increase in efficiency that results in only limited, short-term growth. Nor is innovation the same as discovery (invention), which is, for the most part, unmanageable. A discovery happens unexpectedly and may or may not be useful.

And neither is innovation synonymous with technology, although the two are closely related. Technology takes new knowledge, places it in usable form, and then mixes combinations of new and existing knowledge to isolate and solve problems.

Innovation is a diffusion of discovery. It applies technology to societal needs, and exerts powerful influences on the future. It is intentional and purposeful, and contains a wide spectrum of activities—searching, selecting, incubating, developing, commercializing, and diversifying.

Innovation is not an accident. Its existence depends



on the interaction of three elements. This interaction can be a managed process, with each element a complex entity in its own right.

Needs usually arise from the convergence of societal and technological trends. At the present time, to take an obvious example, cheap, abundant energy is a prime societal concern. Experiments in biology and biochemistry are hinting at previously unknown methods of producing energy. When these two trends are understood and related, a need is perceived. The process of perception—not always this obvious or easy—involves the highest order of individual management skill. Most simply stated, it is the description of bona fide and highly desirable results which we do not know how to achieve, but which—if we had them—would fill important needs.

Capability is not merely the existence of technical expertise. Needs must be stated in many disciplinary and technical "languages" and from a variety of viewpoints. Electrical engineering and biology can approach the same need, but they will do so from different directions and because of different motivations. These directions and motivations must be clearly understood by individuals who expect to manage the innovation process.

Interest, and its corollary, involvement, comes from many individuals. Since it is not possible to predict who will produce an innovation, the widest possible variety of potential innovators must be approached. These innovators will be found in diverse disciplines, from pure science to marketing. They must be able to sustain their interest over long periods of time, often in the face of extreme discouragement.

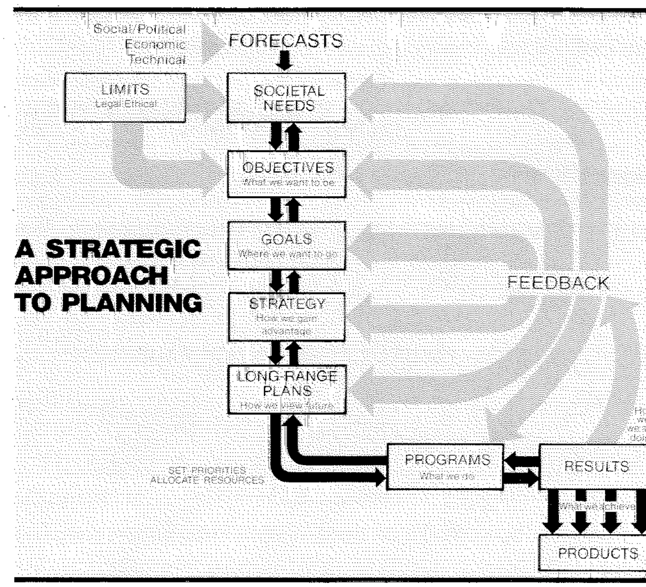
An organization interest is also required. It is rare for an organization to develop enough pressure from within its ranks to produce an important innovation that may bring uncomfortable change. "The priesthood seldom initiates its own reform" is a common slogan we must manage to disprove.

A collision of perceived need, individual capability, and individual interest does not guarantee that innovation will occur, but it does increase the probability.

In order to manage innovation once it has been ignited, a strategic approach to planning is needed to observe, correlate, evaluate, and test innovative ideas. This strategic approach is cyclical.

The elements of the collision that produces innovation are now modified by specific trends and limits. A projected technical capability may be overshadowed by economic prohibitions. Certain areas (human genetic experimentation, for example) may be related to some societal needs, but investigation in these areas is subject to a host of constraints.

Once societal needs and objectives have been defined and judged desirable, an orderly procedure is required if the end results and products are to meet the needs as originally perceived. Systematic planning is not an attempt to eliminate risk. It is an attempt to assess future probabilities when large risks are taken.



Feedback is the key that permits the intelligent altering of strategies. Without feedback, there is no measurement, no evaluation, no way to judge progress—or the lack of it.

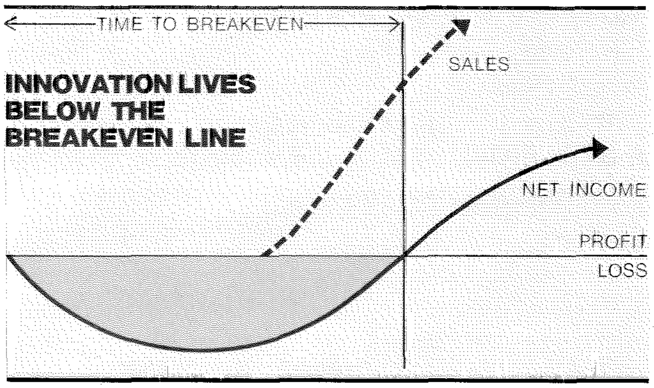
This strategic approach to planning is what makes innovation possible in large, technology-oriented organizations. Without it, innovation will disintegrate into a haphazard pursuit.

Given the intricate interrelationships required to make innovation happen and the complexities of strategic planning, it seems remarkable that a rationale for innovation can exist at all. Particularly in large organizations, there is a basic, psychological conflict between the demands of long-range strategic planning and the constraints of day-to-day thinking.

But not only must large organizations do innovation and planning, they must do them well. The difference between doing and doing well is the difference between remaining consistently competitive in a market, and earning and exercising market leadership. The latter requires a higher level of effort, dedication, timing, flexibility, and, above all, the ability to deal with uncertainty.

Enterprises which achieve positive results because of technology share common features during the innovative process: Technical programs are intimately meshed with clearly defined business goals. Funding is consistently more than adequate for program needs, regardless of corporate profit results or laboratory findings. A proprietary technical position is secured and exploited to develop a proprietary market position.

Even when all these optimums are present, those in the innovative process—as well as those who support it—must not only be willing to exist below the profit line but must also vigorously defend their fair share of the red ink on the corporate balance sheet.



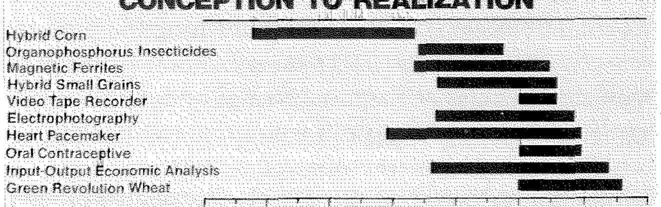
The shaded portion of the illustration above contains more than the research that led to the first faint "Eureka!" and to the development activity that proved it out. There are also plant construction costs, commercial development costs, manufacturing, and management costs.

An activity that only lives below the profit line is suspect, even in the best of times. When total corporate net income is threatened, such activities are truly an endangered species. It takes individual tenacity to continue in the face of inhibiting circumstances, major obstacles, and the outright death wishes voiced by "the experts" ensconced on the top side of the profit line.

To support this individual commitment, there must be a corporate commitment based on the prospect of future profits which will more than offset the losses incurred during the innovation process.

While a prime objective of strategic planning is a reduction in the time needed to bring an innovation to realization, this process takes far longer than most managers realize. In the illustration below, the average time from first conception to first realization is 19.2 years, a time span longer than many managers would tolerate financially if they understood the duration before they became involved.

CONCEPTION TO REALIZATION



SOURCE: *Interaction of Science and Technology in the Innovative Process: Some Case Studies.* Final Report. Prepared for the National Science Foundation by the Battelle Memorial Institute, March, 1973.

If the time to break even from the first research and development explorations to true profitability is forbidding, so too is the delicate and precise timing needed for market introduction of an innovation. Timing is frequently the most important factor in the ultimate commercial success or failure of an innovation.

Technology must be ready at about the same time as the market—never early, and better late than never. History is replete with mismatches. Technology was ahead of the market with Chrysler's Airflow car, with coal hydrogenation, and with protein-enriched foods. The market is still ahead of technology with a cancer cure, a heart disease preventative, and low-cost, effective water treatment.

Management's ability to pace the development of an innovation, particularly in the later stages required in the strategic planning cycle, can mean the difference between success and disaster. The importance of this fine tuning cannot be overstated, but it is often underrated.

Predictability is a complex factor from which management cannot escape, and it requires a high degree of flexibility. Laboratory predictability is another way of saying experimentation. We accept the cost of laboratory failure not only because it is a traditional part of the innovation process, but also because it is a private failure.

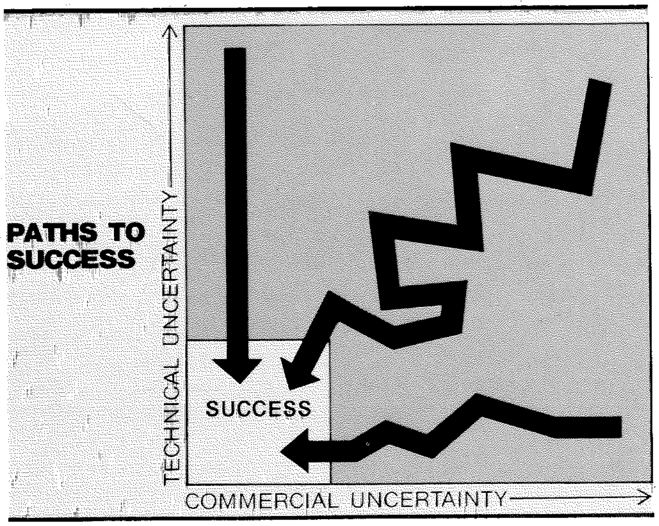
Marketplace failure involves public experimentation and its cost is measured in prestige and opinion as well as dollars. This public cost can take on such importance that it is avoided entirely. Thus we tend to rely on the private technique of market research instead of early selling trials to discover market trends accurately. Statistical extrapolation can have validity, but never certainty, and inaccurate market predictions can throw off timing, confuse long-range plans, or misdirect products.

It is probably impossible to make any market/product interface completely predictable, however hard we try. The most profitable outlet for an innovation is frequently not the one first projected or practiced.

Artificial turf surfaces now common in professional and collegiate athletics were initially developed as portable playground coverings. Xerography failed initially in office copying and was used instead to make multilith masters and engineering drawing enlargements from microfilm. Only much later did it reenter office copying as a success.

The lack of complete predictability, which is always present, requires flexibility, and it is wise not to program application research so tightly that the wild idea never has a chance. If it had not been for some sophisticated developments in silicone polymers, the children of today would not have Silly Putty.

Uncertainty adds another dimension to the process of innovation. It is a condition requiring action but defying risk analysis. Of all the functions large enterprises do not do well, dealing with uncertainty heads the list. Uncertainty can be defined by type and exists in time, permitting construction of a matrix, below.



Projects that are the result of the innovation process are located throughout the shaded area, and the arrows indicate typical paths to success. The farther to the right and the higher a project is, the more uncertain it is of reaching success. The more complex the path to success, the longer the time period will be. In actuality, paths to success are much more complicated than those shown on this chart. They often double and redouble back on themselves many times before reaching the lower left.

Progress becomes smoother and more direct when a program is located close to one of the axes of the chart. When only technical uncertainty is involved, the path is usually direct, since the problems to be overcome are well defined—although not necessarily easy to achieve. When there is technical certainty but commercial uncertainty, the path is somewhat more complex because technical adjustments are required to achieve commercialization.

The coordinated effort each project necessarily involves becomes increasingly expensive as the project progresses from the outlying corners toward the lower left of the uncertainty chart. At the lower left, it has accumulated the four required skills of a successful business venture: technology, management, manufacturing, and marketing—all in proper balance.

Every well-known product or business arising from innovation was once in the shaded area of the chart. So too were many products which have never seen the light of the marketplace. Knowing when to stop a project from moving about on this business chessboard requires as thoughtful a management decision as the first one which encourages an innovation. Projects eliminated from one corporation's uncertainty chart have appeared on another's and have been carried through to success.

The management of innovation is one of the most demanding tasks faced by a large organization, and it is in groups of this type that most technology innovation occurs.

In the beginning, selection must happen not by accident, but with perception of a mixture of technical and commercial needs.

Management becomes more uncertain and difficult after need identification, when a project begins its unique—and unpredictable—route through the uncertainty chart. Without total commitment over an extended period of time, projects cannot survive in this environment, and this becomes a management responsibility.

Then, as a project nears the end of its exploratory journey, management must become critical and rigid before massive numbers of dollars and large numbers of individual careers are infused into the program.

Without unduly restricting individual capability and interest, a method or frame must be provided in which the ultimate goal of the innovation activity is considered. This frame is the strategic planning process, which must be adhered to whether business is good or bad, whether there is an obvious need for such planning or not.

The innovation process must exist within a system which encourages product conceptualization, development, testing, growth, and finally, maturity. This system must reduce the time from first conception to first realization, within the constraints of market introduction timing.

If the management of innovation is to be a success, those concerned with planning must relate the organization to the larger environment in which it exists. They cannot close their eyes to the fact that the long range is becoming shorter all the time. All too often, the management responsible for an organization cannot trust itself to be objective. Most managements of most organizations spend most of their time on yesterday's problems or in striving to maintain the status quo.

This does not alter the fact that senior management in a large organization holds the prime responsibility for making innovation happen, for approaching planning strategically, for dealing with predictability and uncertainty. No other group has the required overall perspective. No other group can manage innovation. □

Retiring This Year

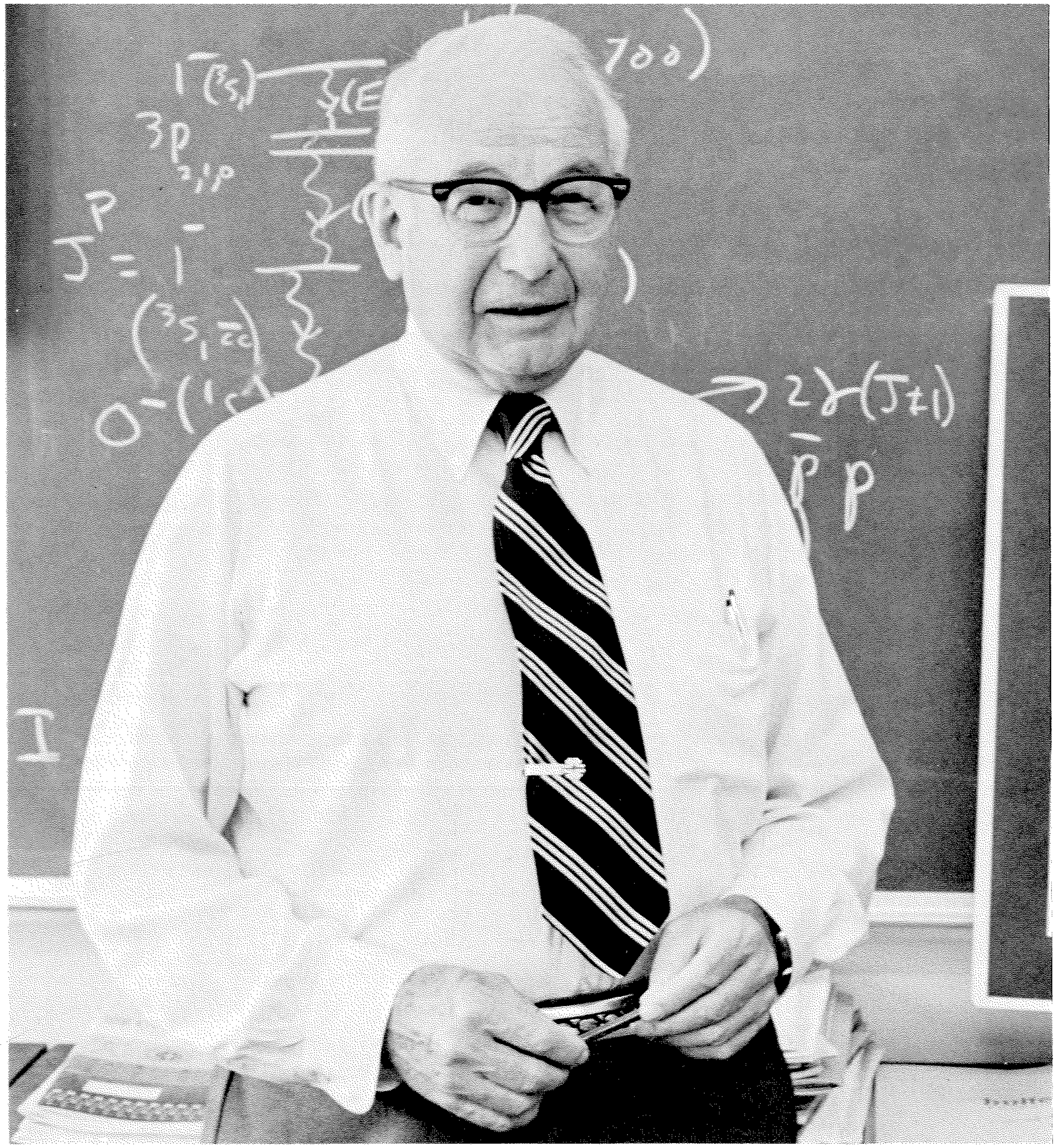
- Anderson
- Bacher
- Wiersma
- Sechler

Carl Anderson, during his more than 50 continuous years on the Caltech stage, has played every academic role from that of undergraduate student on up to division chairman. After receiving his PhD in 1930, Carl set out to investigate high-energy cosmic rays by watching their progress through a cloud chamber placed in a huge (for those days) magnetic field. By 1932, with Carl now playing the role of research fellow, he had obtained cloud chamber photographs clearly proving the existence of the positively charged electron.

The discovery of the positron is one of the milestones in the history of physics and was recognized with the Nobel Prize in 1936. Continuing his cloud chamber studies of cosmic rays with his students and co-workers, notably Seth Neddermeyer, he soon discovered the muon, the first particle of mass intermediate between the electron and the proton.

The pursuit of cosmic rays took the cloud chamber, and Carl along with it, to a variety of altitudes and latitudes, but Pasadena and the Institute always remained home base. During the early 1940's, his research was interrupted for a stint in the service of the National Defense Research Committee, Office of Scientific Research and Development, in Pasadena and briefly in Germany, but as soon as hostilities ceased, Carl was back at his favorite pastime, this time flying the cloud chamber in a converted B-29 bomber. The productive postwar period saw the discovery or confirmation of V-particles, lambda particles, and the beginning of a long stream of subnuclear particles — called with some optimism at the time, elementary particles. This evidence of the richness and complexity of the subnuclear world, first glimpsed in the cosmic rays, led to the production of the large modern accelerators that are turning out such exciting discoveries today.

Though any review of Carl's achievements will necessarily emphasize his research and his contributions to physics, this represents only half his professional concern, and he has taught 53 years of physics students at Caltech, give or take a few. From 1962 to 1970, he served as chairman of the Division of Physics, Mathematics and Astronomy. His many friends and colleagues on the campus look forward to catching Carl Anderson in his new role of Professor Emeritus.



Robert F. Bacher, professor of physics, retires this month after 27 years at Caltech as professor, division chairman, and provost. He attended the University of Michigan, receiving a BS degree in 1926 and a PhD in 1930. His early work was in spectroscopy, and while working in that field he wrote a book with S. Goudsmit entitled *Atomic Energy States*.

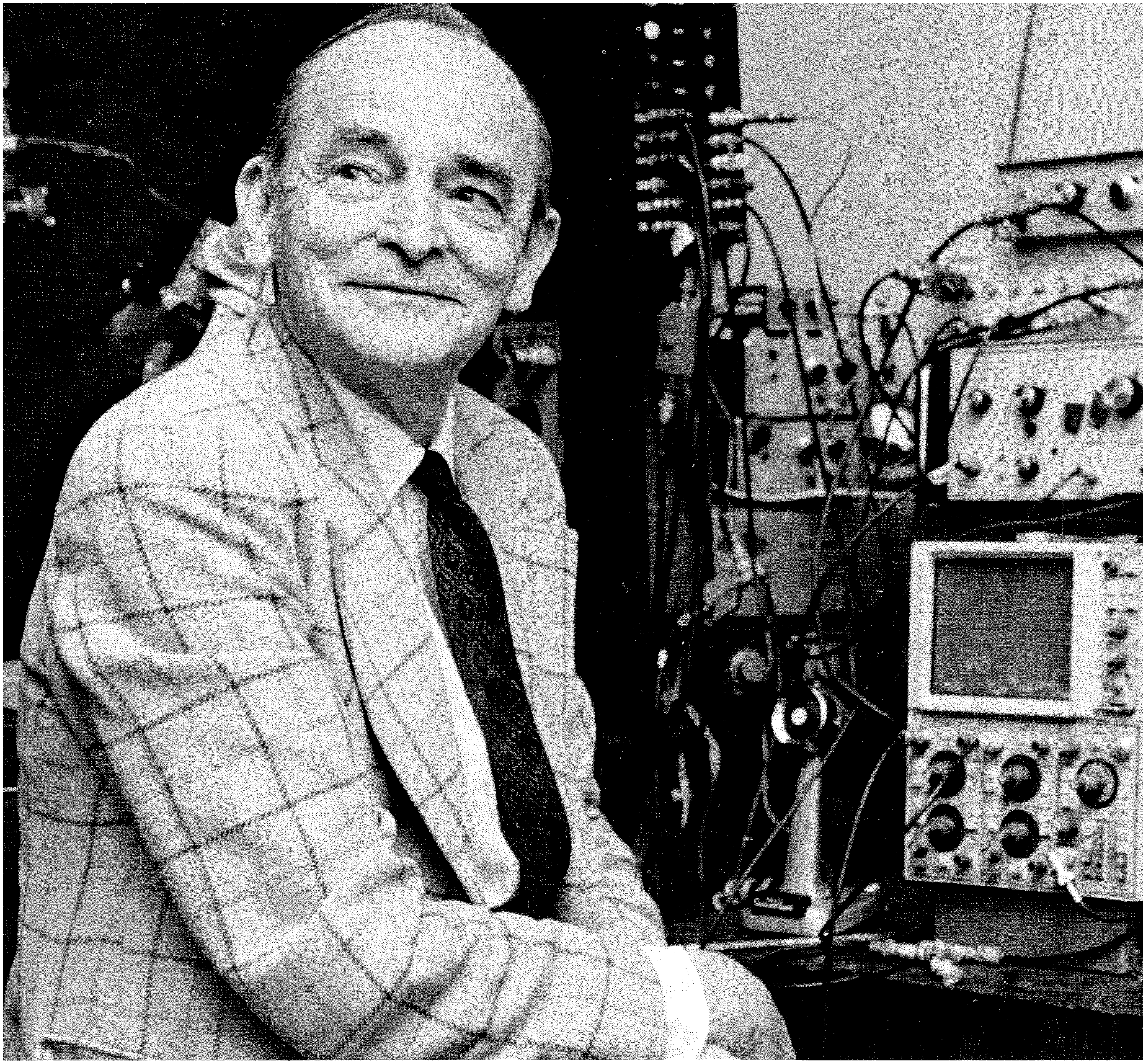
His first stay at Caltech was as a National Research Council fellow in 1930-31. In 1935 he joined the faculty at Cornell, where he worked in nuclear physics until 1949. During this period he was co-author with H. A. Bethe and M. S. Livingston of some famous articles on nuclear physics which, published in the *Reviews of Modern Physics,* remained for many years the major "textbook" in that subject.

During World War II, he worked first in the radar program at the MIT Radiation Laboratory, under Lee DuBridge. In 1943 he moved to Los Alamos to work on the atomic bomb, serving as head of experimental physics (1943-44), then head of bomb physics (1944-45).

After the war he returned to Cornell as professor of physics but soon thereafter moved to Washington to serve as one of the first members of the new Atomic Energy Commission.

In 1949 he came to Caltech as chairman of the Division of Physics, Mathematics and Astronomy, which position he held for 13 years. While division chairman, he initiated or promoted several programs of considerable importance to the Institute. One of these was the program in high-energy physics — based in the beginning on construction and use of a new electron synchrotron — which he directed. He is also largely responsible for the group in elementary particle theory, which he raised to a preeminent position by bringing Professors Feynman, Gell-Mann, and others to Caltech. And he initiated and encouraged our program in radio astronomy with the creation of the Owens Valley Radio Observatory.

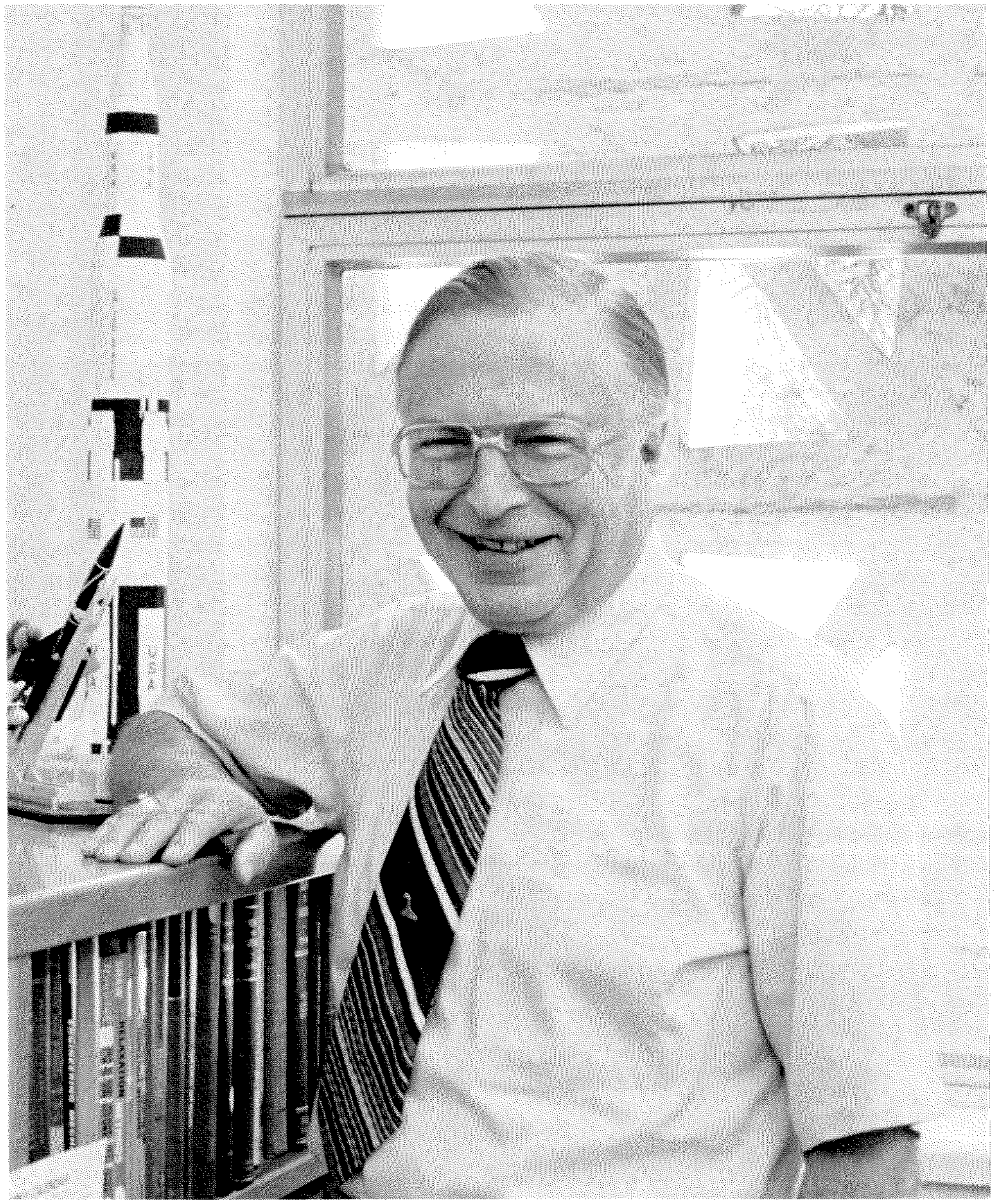
In 1962 he became Caltech's first provost, and in 1969 he was appointed vice president in addition. These positions he held until 1970, when he retired from administrative duties.



C. A. G. Wiersma, who has been a member of the Caltech faculty for almost 42 years, retires in July, acquiring emeritus status. Born in The Netherlands, he studied at the Universities of Leiden and Utrecht and was invited by Thomas Hunt Morgan to join the California Institute of Technology in 1934 to represent the then relatively new science of comparative physiology.

Wiersma obtained his doctoral degree in 1933 from the Utrecht University with a thesis on the nerve-muscle system of crustaceans. He has maintained an interest in this class during his entire scientific life, performing pioneer work first on the neuromuscular system, then on the central nervous system and, during the last years, on the visual system — sometimes traveling to exotic places to pursue his studies. Over the years Wiersma has acquainted many students — undergraduates, graduates, and fellows — with his favorite objects of research. His former co-workers commemorated his retirement recently with a symposium in his honor.

Wiersma is a member of numerous scientific societies including groups such as the American Physiological Society and the Society for Neurosciences, and he became a correspondent of the Royal Netherlands Academy of Arts and Sciences in 1956.



Ernest Edwin Sechler never expected, when he entered Caltech as a freshman in 1924, that he would spend his entire career here and become Professor Emeritus almost 52 years later.

In 1929 he received an MS in mechanical engineering, with an option in aeronautics. When the Guggenheim Aeronautical Laboratory was officially inaugurated a year later, the first MS degree in aeronautics was conferred upon Ernie Sechler, putting him at the head of a long list of renowned graduates from this laboratory. He received his PhD in 1934, and became full professor in 1946.

Ernie has nurtured a lifelong interest in teaching the design of safe, lightweight structures. And this interest was not confined to the design of aeroplanes. He played a decisive role in the design of the large shell structure that covers the 200-inch telescope on Palomar Mountain and in the analysis and correction of the gravity-induced surface deformations of the 200-inch Palomar mirror. He was instrumental in designing the shell structure of the Cooperative Wind Tunnel — the big Caltech-operated test facility that, in operation, drew 50 percent of Pasadena's on-line power output. His work on the buckling strength of thin shells has influenced the design of missiles and boosters that are the backbone of our space effort.

His technical knowledge has found application by way of many consulting contacts throughout the aerospace industry, and he has provided guidance to the Air Force and NASA through chairmanship of various national advisory committees on structural research.

Beyond his technical interest Ernie has always had a warmhearted concern for his colleagues and for the students of the Graduate Aeronautical Laboratories. Since the late 1930's Ernie has performed most of the admissions work for GALCIT, a task that was and is vital to the success of this option, and after Clark Millikan's death in 1966 he directed the Aeronautical Laboratories as executive officer until 1971.

Since retirement from the position of executive officer, Ernie has been active in teaching, design, and furthering the development of windmills as a power source. It seems safe to say that when the speeches are made and this note gathers dust, Ernie will continue to operate in his familiar style, oblivious to the meaning of emeritus status.

Speaking Of . . .

True Confessions

At the 50th anniversary celebration of the founding of the California Institute of Technology Associates, held at the Athenaeum on May 3, Lee A. DuBridge, president emeritus, and chemistry professor Harry B. Gray engaged in some reminiscences of Caltech which, not so incidentally, revealed for the first time how Harry Gray happened to join the Caltech faculty.

GRAY: I'd like to tell you about my first meeting with Lee DuBridge — and I'd like to tell Lee at the same time, because he's forgotten. I was here visiting from Columbia in the spring of 1965. I enjoyed the place very much — indeed I sensed a special atmosphere here as soon as I arrived. I was asked here to give some lectures in my special field of chemistry. I gave two or three lectures and then Jack Roberts, who was chairman of the Division of Chemistry and Chemical Engineering at the time, called me into his office and said, "Harry, we like what you're doing, and we're impressed with your work . . . We'd like you to come out permanently."

DuBRIDGE: You must have been pretty old — 37 or so?

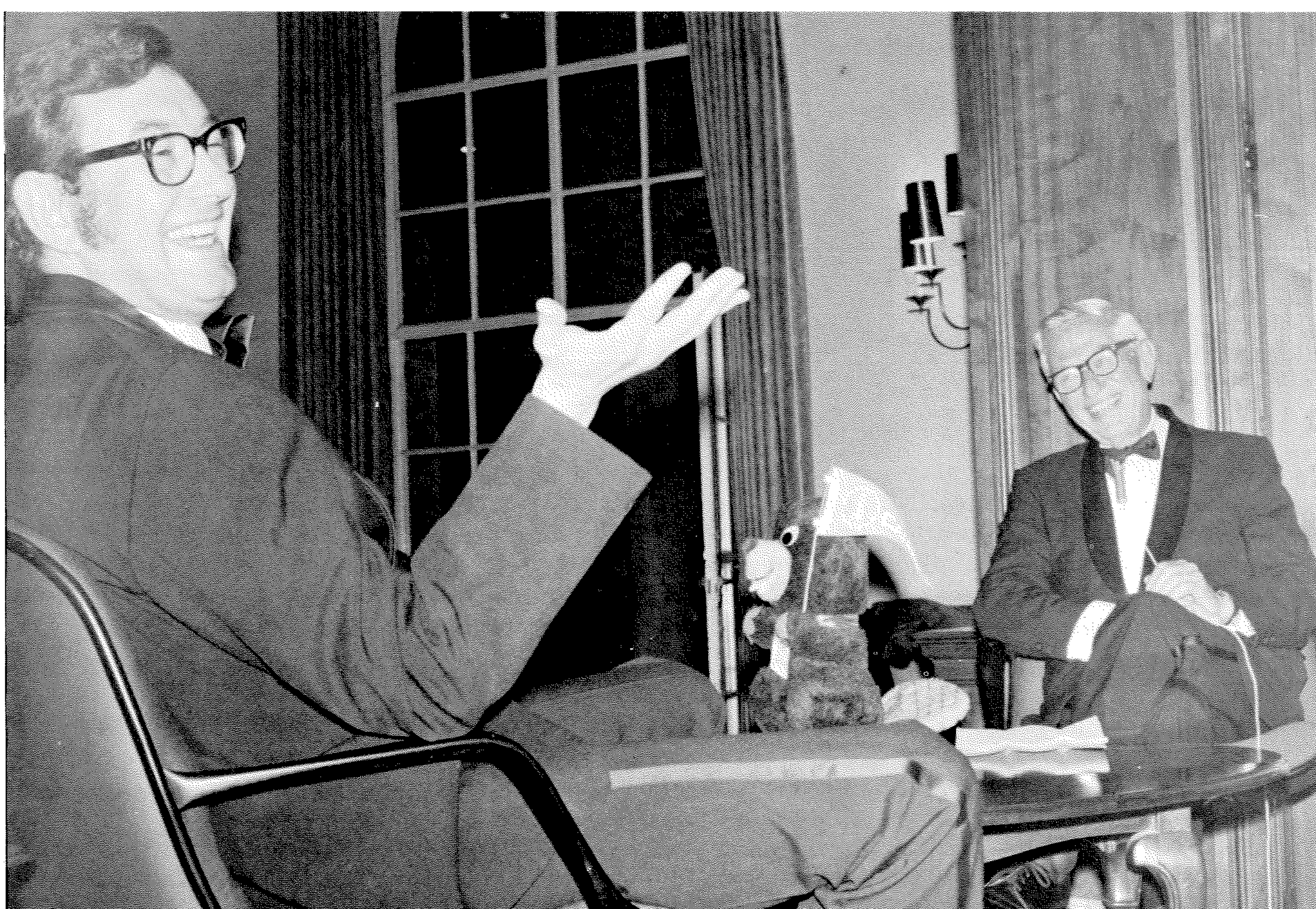
GRAY: I was 29. (Don't extrapolate!) And he said, "I'd like you to go talk to Lee DuBridge about coming out here." I started shaking at this point and said, "Isn't he president?"

"Yes, he's the president of Caltech."

I said, "I'm sure I couldn't get an appointment with Dr. DuBridge. After all, I've been at Columbia for five years and I've only seen Grayson Kirk once. And that was at long distance."

He said, "Well, Lee DuBridge is waiting to see you now."

So I ran out of the office and from Crellin to Throop — quite in shock — and I ran into your office, and there you were, looking much as you do now.



Remember when? Chemistry Professor Harry Gray and President Emeritus Lee DuBridge reminisce for The Associates.

And you welcomed me and said, "Harry, we've heard very good things about you. What can I do to get you out here from Columbia?"

Then I was in *total* shock, and couldn't think of anything, and so I blurted out the first thing that came to mind, which was, "Dr. DuBridge, you know that Grayson Kirk has just decided to build a physics building on the four beautiful tennis courts at Barnard College?"

And Lee said, "No I didn't know that."

I said, "Well, let me tell you about it."

This is the only thing I could think of at the time. Maybe he'd do anything to get me out here. So I said, "Yes, these are the last four good tennis courts at Columbia"—and since I've always been a tennis enthusiast, I described to Lee how the tennis courts were between my apartment and my laboratory, so I could play going to my lab and on my way back home as well.

Lee was taking all this very well, and I said, "You know, several of us in chemistry who also play tennis wrote a letter to Grayson Kirk in which we said,

Dear President Kirk: We are very concerned that you decided to build a physics building on top of the four beautiful tennis courts at Barnard College. In our opinion as chemists and tennis players, one good set of tennis is worth at least 12 physicists.

I realized at this moment that I was talking to a physicist, and I looked around and Lee was still smiling. It was at that moment that I knew Caltech was my kind of place.

So I said, "Dr. DuBridge, if I come to Caltech I want you to promise me that you'll never build a physics building on the three Athenaeum tennis courts."

And he said, "Harry, I'll be happy to promise that. We're desperate for space, but we're not that desperate. And *I'll* promise you that — but I can't promise you that my successors won't build on those three beautiful tennis courts."

So I went away happy and I came back to Caltech, and, since I've been here, of course, I've made sure that any physicists appointed as administrators also play tennis, and you see the result — there's not a trace of a physics building on the tennis courts."

Where They Go from Here

What happens to Caltech graduate engineering students after they receive their degrees? Well, everybody knows that most of them go into academic life; quite a few probably go into research organizations to continue advanced research; and a few unfortunates find themselves in industry. After all, industry really wouldn't be attractive to Caltech's graduates. Or would it?

To find out, Ernest E. Sechler, professor of aeronautics, recently reviewed the postgraduate careers of approximately 1000 alumni of the GALCIT (Graduate Aeronautical Laboratories of the California Institute of Technology) from 1939 to 1973.

Sechler found that 51 percent of these alumni (514) are in industry, some in very high positions. At least one is president of a very large corporation, many are research directors, and others have risen to management positions after making significant engineering contributions as members of technical staffs. As might be expected, of those in industry 69 percent (353) are in aerospace-related companies; the remaining 31 percent (161) hold positions in 104 widely diversiform industrial organizations.

In the years since GALCIT's founding in 1928, the armed forces have increased their use of technologically complex systems, and the military have found it advantageous to have a few of their officers receive GALCIT's high-level engineering training. This was true in particular during and immediately following World War II and resulted in the second largest group of alumni (208) in Sechler's survey. Of those in the military, 137 are in the Navy (20 having made Admiral rank), 56 are in the Air Force (2 Generals), 8 are in the Army, and 5 are in the Marine Corps. The Royal Canadian Air Force boasts one GALCIT alumnus who is a Brigadier General and one who is a Colonel.

In the third place numerically are the 18 percent (179) of the graduates who hold positions in 92 academic institutions. Caltech with 14 alumni on its faculty leads the group; Stanford is next with 10; and 9 are at UCLA.

A total of 59 different research laboratories and government agencies employ 75 graduates, or 8 percent. Of these, 40 are at the Jet Propulsion Laboratory, and 21 are connected with foreign agencies or governments.

Seven GALCIT graduates have formed their own consulting companies, and there is an interesting group of 16 who might be classed as in "unusual positions." These include a rancher, a lawyer, a retired guitar maker, a Peace Corps teacher in Malaysia, a car wash owner, a bowling alley proprietor, and two physicians.

Depending upon the changing demands of the outside world, certain cyclic trends appear in an overview of the careers of these 1000 graduates over the 34-year period. Obviously, new industrial interests such as fusion, high-energy lasers, new constructional materials, and new forms of energy have taken some of the more recent graduates. But the diversification is found in essentially every graduating class.

The GALCIT faculty see this as verification of the soundness of the underlying philosophy of their program. It is often said that modern technology demands narrow specialists. This is true only in the sense that a very detailed knowledge of a specific subject is required at any given time. Unfortunately, a short time later the emphasis may shift to another field. Consequently, engineering schooling should be anything but specialized; instead, an appropriate education should result in a broad and deep foundation in science and engineering to enable the professional to adapt rapidly and to change specialization with ease.

Letters

Dear Who?

San Francisco

Gentlepersons:

I enjoy your magazine but the address label covers too much of the cover to get any effect from backing off or squinting at Professor Pierce's picture in 84 characters. Except for the low brow, it could just as well be me.

My calculator shows that your error re 1/273 gives a true repeating decimal — 0.003663002663, etc., while Feynman's 1/243 is imperfect (0.004115226337448559677081).

In response to the terminal question in your letters column (What is the proper salutation in this modern age?), the above is my suggestion for a modern salutation to be used by one who doesn't know you well enough to use "Dear Ed and Jacque."

JOHN DASHER

Thanks – and we're sorry those address labels got slapped right into John Pierce's face last month. Our mailing service is still *arguing with the Post Office official who made them do it.*

Caltech

How about "Dear Mr/s" as a salutation when you don't know the sex of the person, e.g., Lynn? The addressee can read it to suit the occasion: Mr., Mrs., Ms.

JOHANNA TALLMAN
Director of Libraries

Washington, D.C.

Dear Sir or Madam, as the case may be:

I too would like a copy of Feynman's article on cargo cult science, which you published a couple of years ago. The above salutation is in response to your query in the March-April issue.

EDWARD A. FINN '60, Deputy Director
Lunar and Planetary Programs, NASA

Books

RACISM AND EMPIRE: White Settlers and Colored Immigrants in the British Self-Governing Colonies, 1830-1910.

by Robert A. Huttenback
Cornell University Press........$17.50

Reviewed by Edwin S. Munger

Impeccable scholarship is often covered by the barnacles of pedantry, resulting in a book that is unreadable and unread. Here is a lucid exception. In examining Joseph Chamberlin's assertion that the British Empire "makes no distinction in favour of, or against any race or colour," the book informs and entertains.

Dr. Huttenback, who has spent his academic career at Caltech since taking his PhD in history at UCLA, has done research for this study over a period of years in Britain, Australia, New Zealand, Canada, and South Africa. The result is a fascinating, fair, and devastating refutation of Chamberlin's doctrine.

The author obviously relishes the tidbits of history that serve to garnish his imperial roast. Thus from New Zealand he quotes the *Otago Times* in 1871, which decried Chinese market gardeners as "Mongolian Filth": "We are free men, they are slaves! We are Christians, they are heathens! We are Britons, they are Mongolians!"

In Canada, the Victoria Trades and Labour Council contended that "the Hindoos by reason of their caste prejudices, peculiar religious convictions, loathsome habits and obnoxious manner of living, can never assimilate with white people or perform the duties of desirable citizens of this country." Nor were Indians more generously described in Natal, where in 1897 they were called "black vermin," and common phrases included: "A thing black and lean and a long way from clean," or "the Asian dirt to be heartily cursed."

Sexual fears and stereotypes were common in the white colonies. The Canadians attempted to save white women from the allegedly vile and unspeakable habits of Chinese by prohibiting Asians from employing white girls. A communication from the Vancouver branch of the Trades and Labour Congress declared that the Chinese "are also adept druggists in their own way, and as servants they have ceaseless opportunities of adulterating food with drugs unknown to white men, thus placing the female members of the household at their disposal and unscrupulous will."

The result of all this denigration and of the widespread, if less pejoratively expressed, views by white colonists was the passage of various acts to exclude Asians from settlement. British Columbia and California followed similar paths. All kinds of tricks were employed, which included asking a ship officer of Austrian and Egyptian antecedents to take Greek dictation to prove that he was "civilized."

The author shows repeatedly that the authorities in London did try to secure an even break for non-white British subjects, but they were usually overridden by the local white settlers.

The situation hasn't changed all that much today, when we find successive British Prime Ministers all but impotent in seeking an end to the decade of independence in Ian Smith's Rhodesia. But whereas the British government is today equally as powerless as in the nineteenth century, it has not acquiesced and will not acquiesce to the continued domination of a white minority outnumbered by blacks probably thirty to one.

The book describes how Indians lost their jobs in Natal when they couldn't keep books, as required, in English. A few years ago this reviewer heard Indians in Mauritius justifying a bill designed to exclude Chinese from being bookkeepers by requiring them to know Tamil. Racism will never be a monopoly.

Professor Huttenback recognizes that it is often "much too easy to judge the past through the eyes of the present," and that "the British Empire of Settlement was not alone in denying the brotherhood of all men." And although he sees validity in the view long presented to American schoolchildren of Britain as a birthplace of liberal ideas, in this study of prejudice and deception he rattles a major skeleton in the British imperial cupboard.

Edwin S. Munger is professor of geography at Caltech.

INTERNATIONAL ECONOMIC CO-OPERATION AND THE WORLD BANK

by Robert W. Oliver
The Macmillan Press Ltd ... $20.80

Reviewed by Horace N. Gilbert

A book review should not be a summary of its contents, but because few of the readers of *E&S* probably will come across this book, there is reason to depart from this convention. Professor Oliver has written a book in a highly readable style on one of the most important subjects of the century: the development of means, formal and otherwise, to bring about international economic cooperation.

The story begins with the end of World War I when the economics of most European countries were in disarray, some extremely so. Country by country emergency measures were taken. Varying degrees of success were attained largely because of the cooperation and personal leadership of Benjamin Strong, governor of the Federal Reserve Bank of New York, Montague Norman, governor of the Bank of England, Hjalmar Schacht, governor of the Reichsbank, and Professor Charles Rist, representing the Bank of France. The postwar recovery, the expansion of foreign investment, and the complications of war debts and reparations gave rise to widespread concern regarding the improvement of ways to handle international economic transactions.

Oliver's presentation of the discussions that took place during this pre-Bank period are detailed and enlightening. His ideas of John Maynard Keynes during these years are set forth in considerable detail. Keynes stands out prominently for his creative thinking and wisdom regarding the steps that should be taken to bring about international economic cooperation. It was a relief to hear Keynes cited on broader matters than deficit financing, the main theme of his 1936 classic book. In 1930 Keynes offered a plan for a Supernational Bank which anticipated much of his plan for an International Clearing Union drafted in 1942 and presented as the British position at Bretton Woods. Oliver describes the limited but significant role played by the Bank for International Settlements.

World War II called for a new American foreign economic policy, and the Treasury Department played a leading part in its formulation. In the Treasury the person most concerned with the assignment was Dr. Harry Dexter White. Oliver tells the dramatic story of White's career, his dedicated work preparing the American draft for the World Bank and the Monetary Fund, his leadership representing the U.S. position at the Bretton Woods Conference, his appointment in 1946 as the first U.S. Executive Director for the International Monetary Fund, and in 1948 his being accused of association with a Soviet espionage group.

Bretton Woods was ratified by the U.S. Senate on July 18, 1945, and signed by President Truman on August 4. By the end of the year enough nations had joined to make the Bank and the Fund realities. There were the usual problems and delays in staffing and becoming operational. A serious difficulty was the definition of the relative authority of the president of the Bank and the executive directors. The first president, Eugene Meyer, resigned over this issue; the second, John J. McCloy, accepted the appointment after rewriting the bylaws giving the President clear top authority.

The emphasis in the book up through the successful launching of the Bank and the Fund is on careful history. Oliver gives a lighter treatment to the years that follow, with little attention to the operating record over the years. This information is readily available in the annual reports of the two institutions. He does note a common criticism of the Bank, that it has been too much of a "bank" giving prime consideration to the credit worthiness of borrowers rather than to reconstruction and development. Perhaps this policy has been just as well. It has established the good credit of the Bank's bonds sold in the international money markets to raise funds for the making of many loans. Also the Bank's operations must be judged in the context of other institutions and programs set up to promote reconstruction and development.

Much has been written about the subject of Oliver's book. His differential contributions are a balanced weighing of the circumstances attending the efforts to find and develop formalized ways to bring about international economic cooperation, and a scholarly account of the conception and birth of the World Bank and the International Monetary Fund, institutions that have proven themselves as valuable for the achievement of important goals. Caltech can be proud of Oliver's significant contribution in this field.

Robert Oliver is professor of economics at Caltech, and Horace Gilbert is professor of business economics, emeritus.

MALRAUX'S HEROES AND HISTORY

by James W. Greenlee
Northern Illinois University Press $12.50

Reviewed by Annette J. Smith

Outside of France and a small circle of French literature aficionados, the name André Malraux probably brings forth the picture of an intellectual man of action, surrounded, somewhat like his Anglo-Saxon counterparts T. E. Lawrence and Hemingway, by a mysterious personal legend. Tourists of Paris in the 50s and 60s know him as the Minister of Culture under DeGaulle who undertook the inconceivable and seemingly sacrilegious task of sandwashing the monuments back to their original freshness. To readers of

World Masterpieces, he is the author of *Man's Fate*, a landmark of modern literature. Students who have lived through the spring of '68 passionately resented hearing that Malraux responded to the news of 450 wounded Parisians lying on the street with a dry, "Nobody killed yet. Rather interesting, isn't it?" while proceeding to chat with a friend about *his* passionate years in the Spanish International Brigades.

This is all to say that M. Malraux is not an endearing man. But James Greenlee's study of Malraux reminds us what a central position he occupies in contemporary literature.

The fact that Malraux's writings span 50 years of our time works both for him and against him. For him, because he is a better mirror than almost any other French writer of the successive philosophical trends in the twentieth century. Not only is he a pre-existentialist, but one might say he also is a post-existentialist, or an anarchist. On the other hand, many critics wonder how the same man can have been, within one lifetime, a cynical adventurer, a devoted Marxist, a militant Gaullist, and finally the apolitical Grand Priest of the philosophy of art. Greenlee's main merit is precisely to provide the reader with a valid rationale for these changes and to follow it concretely and painstakingly through the complete body of the works.

Malraux's own philosophical crisis was precipitated by several sojourns in the Orient and the confrontation of the passive but cosmic Oriental point of view with his Western concept of personality in a dynamic opposition to the world. This antagonism between the Westerner and the world was denounced by Malraux as "absurd" (two decades before Camus made the adjective famous) and summed up in subsequent works in the word "destiny." Destiny must deal with history as the only transcendental framework left by the disappearance of God from modern literature.

The overall path followed by his heroes reflects Malraux's changing views of the relationship between the individual and history in the course of his life. While not much different in substance from other existential solutions, Malraux's responses offer the advantage of changes within an organic continuity.

From 1926 to 1933, Malraux's works reveal a tragic concept of history. In Oriental contexts and plots heavily tinted with colonialism, his heroes seek mainly personal gratification, and so remain disconnected from history, which refuses to respond to their demands and turns against them. From *Man's Fate* (1933) on, the main characters succeed in transcending their own lives by serving a collective goal (here the Chinese Revolution). Imprisoned, tortured, or forced to suicide, they still fail from a historical point of view, but find redemption and meaning in the fraternity of militants. *Man's Hope* (1937) carries the same theme to a more optimistic conclusion as we witness a hero of the Spanish International Squadron evolve from an abstract Marxist ideology to a realistic and efficient military leadership, or, as Malraux puts it, "lose his virginity of command." Thus, individual destiny and history have found a way to coincide, each affecting each other.

However, a still later stage, dialectically illustrated in *The Walnut Trees of Altenburg* (1948), then reworded in the first person in the *Antimemoirs* (1967), proposes to find the meaning of life no longer by identifying with history — whether tragic or hopeful — but by identifying with civilization; that is, those creative human endeavors which both preexist and survive the boundaries of history and national cultures. Thus it is through the mediation of art and out of time that Malraux finally sees a possible reconciliation of man with the cosmos. This "concept" of "Antihistory" or "Antidestiny" underlies several massive volumes on the philosophy of art: *The Psychology of Art* (1949-50), *The Voices of Silence* (1953), and *The Metamorphosis of the Gods* (1960-).

This sweeping trajectory which goes beyond the existential solution and fuses being and doing, East and West, is perhaps what makes Malraux (and Greenlee's study) more relevant to younger readers than other existential writers. They too seek, unfortunately by less arduous and more superficial means, a reconciliation with the universe. They too often believe in a disengagement from politics and in being rather than doing.

Do they read Malraux? I do not know. If they do, I doubt, nevertheless, that they would recognize themselves in him. Even when giving in to the collective, Malraux remains obsessed with the individual achievement or quest. His world is fraternal but could never be communal. It is tense, solemn, occasionally accompanied by the accents of Palestrina or Beethoven, and foreign to the kind of universal "letting go" and inner space that appeal to the young. He is too wordy for an era which so much distrusts language.

Finally, as a woman, I should add that his is a monolithically virile world where women appear at the least as sexual outlets for a masculine libido frustrated by history, and at the most as sexless militants. Vice versa, action is viewed by Malraux as a higher form of libido. This personal reaction would be irrelevant if Malraux were not in conflict with the present general suspicion of machismo.

But beyond the debt a literature teacher will have to Greenlee's useful and illuminating panorama, some lines of Malraux will speak for and to all men and women who might feel constantly depassé — or only bypassed — by a technological, elitist, quantitative, and sometimes merciless society: "An individual is more than his biography, or at least, than the sum of his acts." And, again, "We hear the voices of others with our ears, our own with our throats. Yes, And our lives, too, we hear with our throats . . ."

James Greenlee, who was assistant professor of French at Caltech from 1967 to 1973, is now teaching at Northern Illinois University. Annette J. Smith is lecturer in French at Caltech.

Joe Nemchik battles distance and resistance...

to help provide better rural telephone service. Bell Labs electrical engineer Joe Nemchik, shown checking the performance of a circuit board, was one member of a team that tackled a major problem: telephone signals are weakened by electrical resistance in the copper wires that connect remote communities to switching offices. Up to now, reducing the resistance required costly large-diameter wires.

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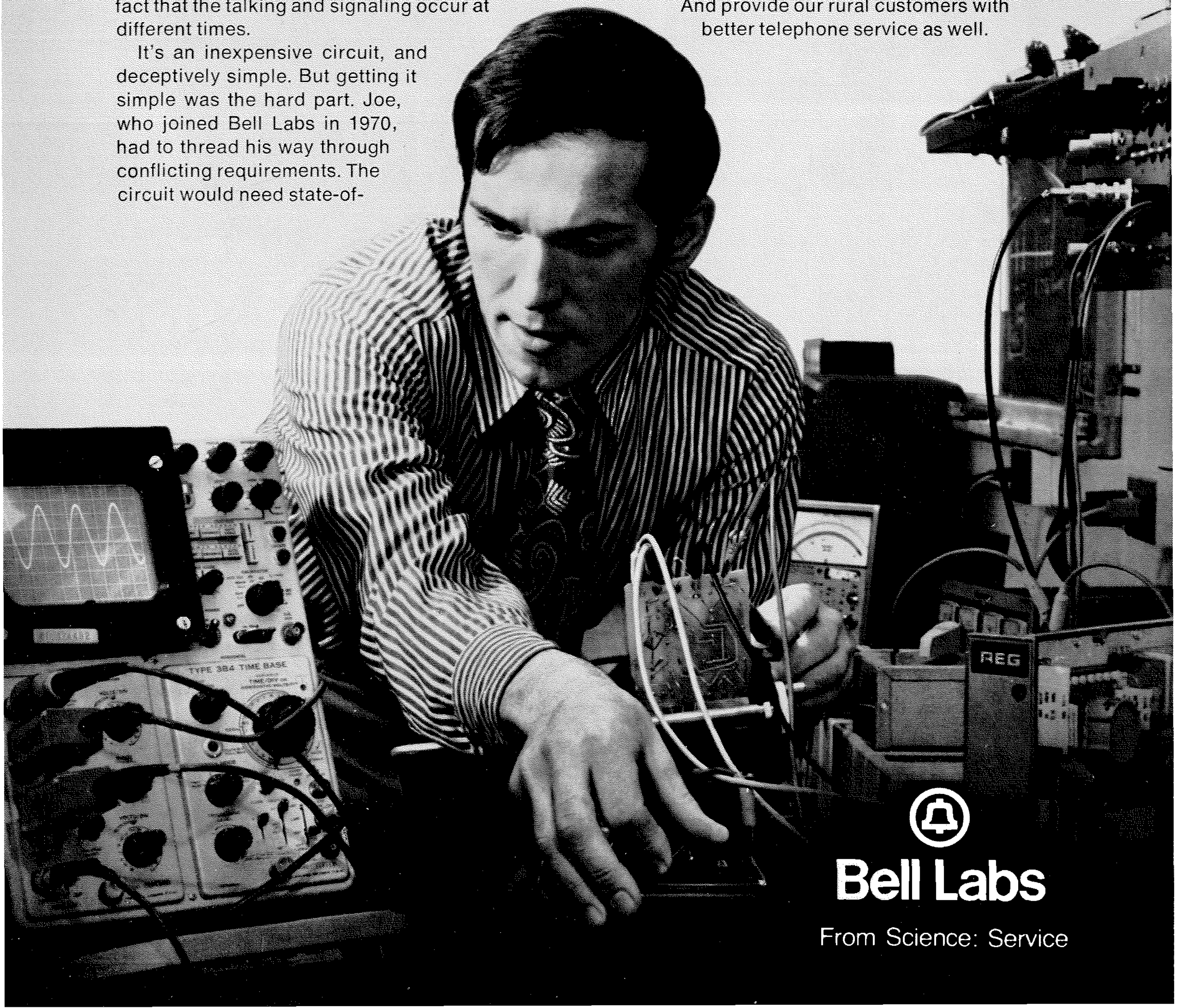

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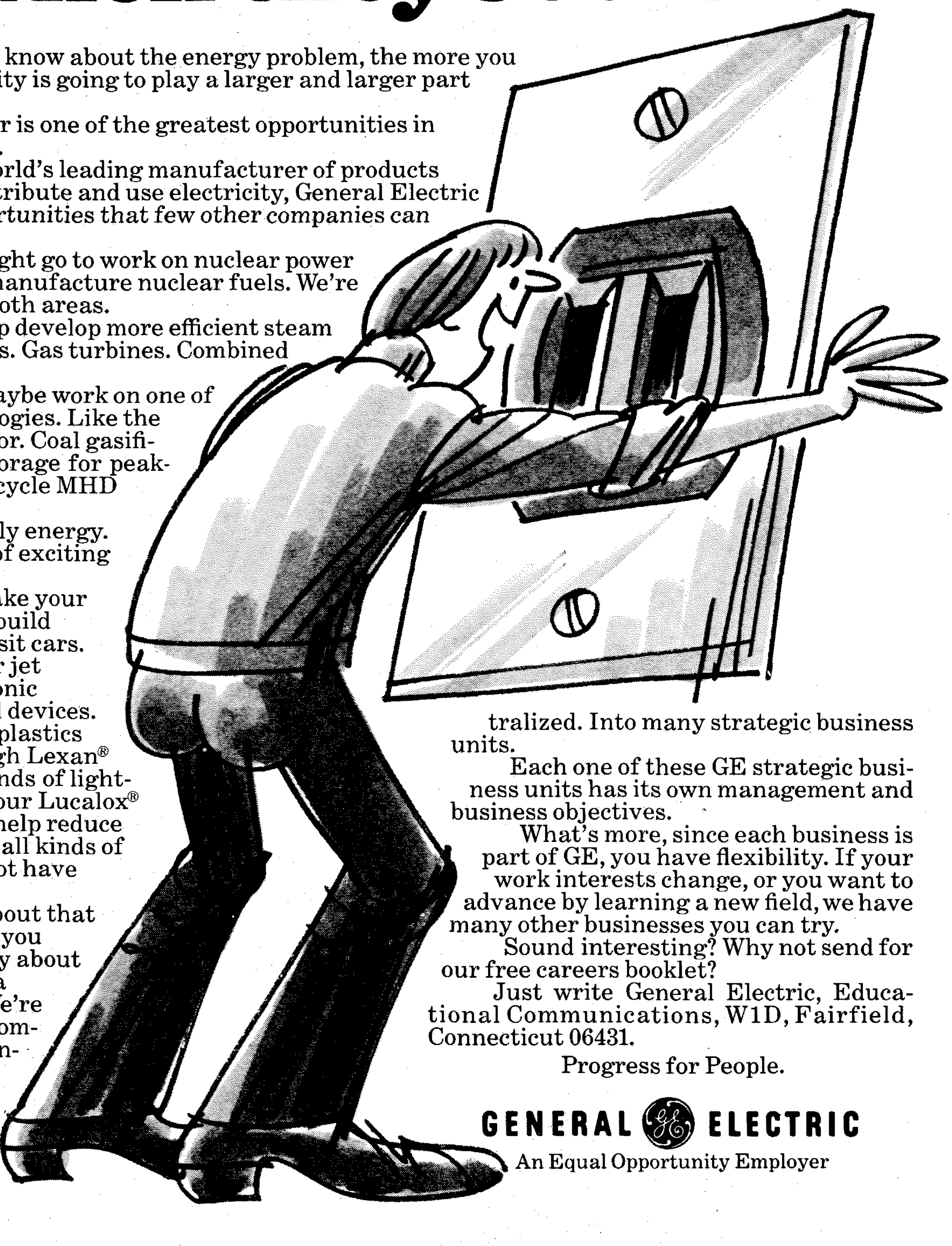

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