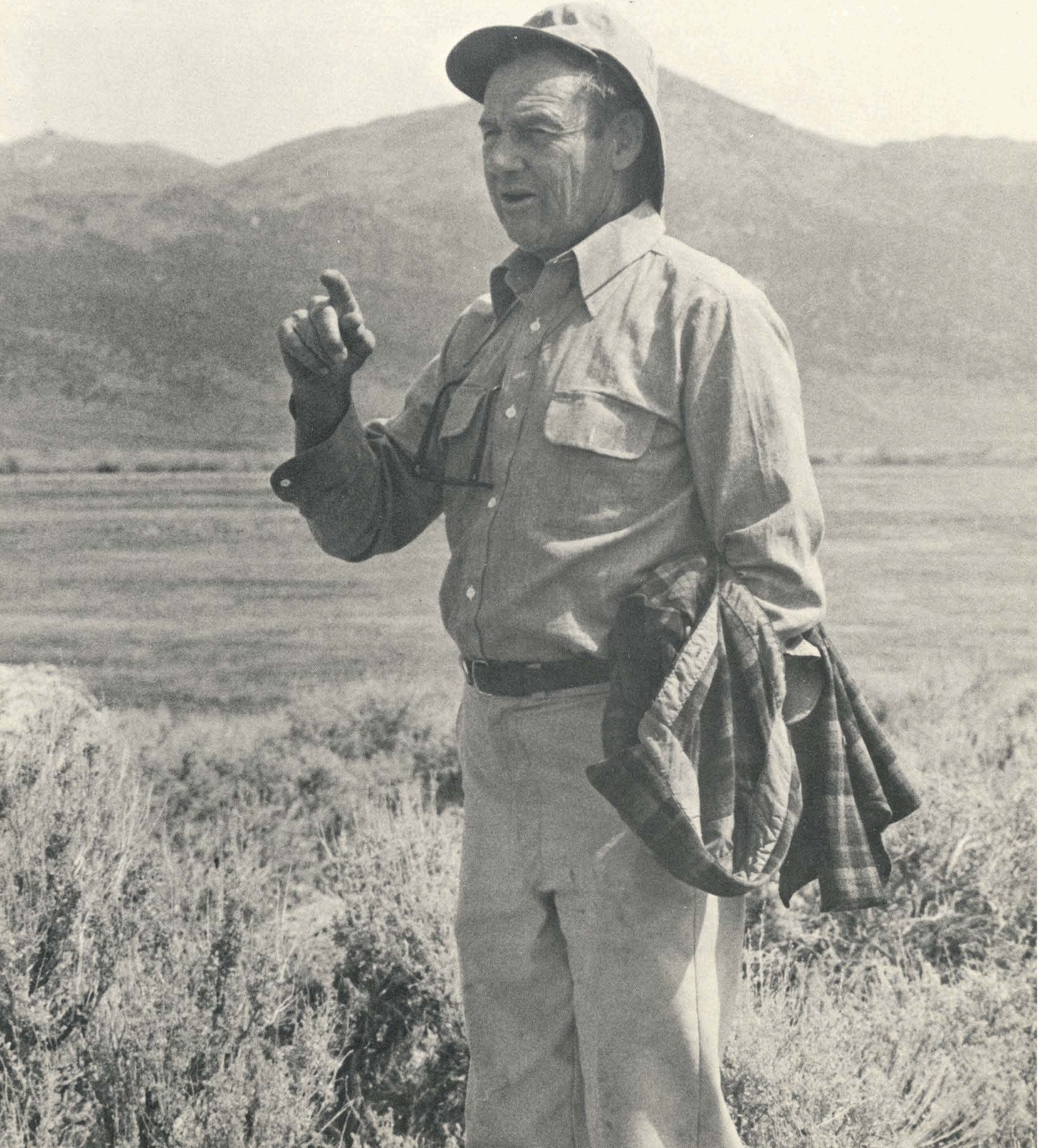
Engineering & Science

California Institute of Technology | October-November 1977



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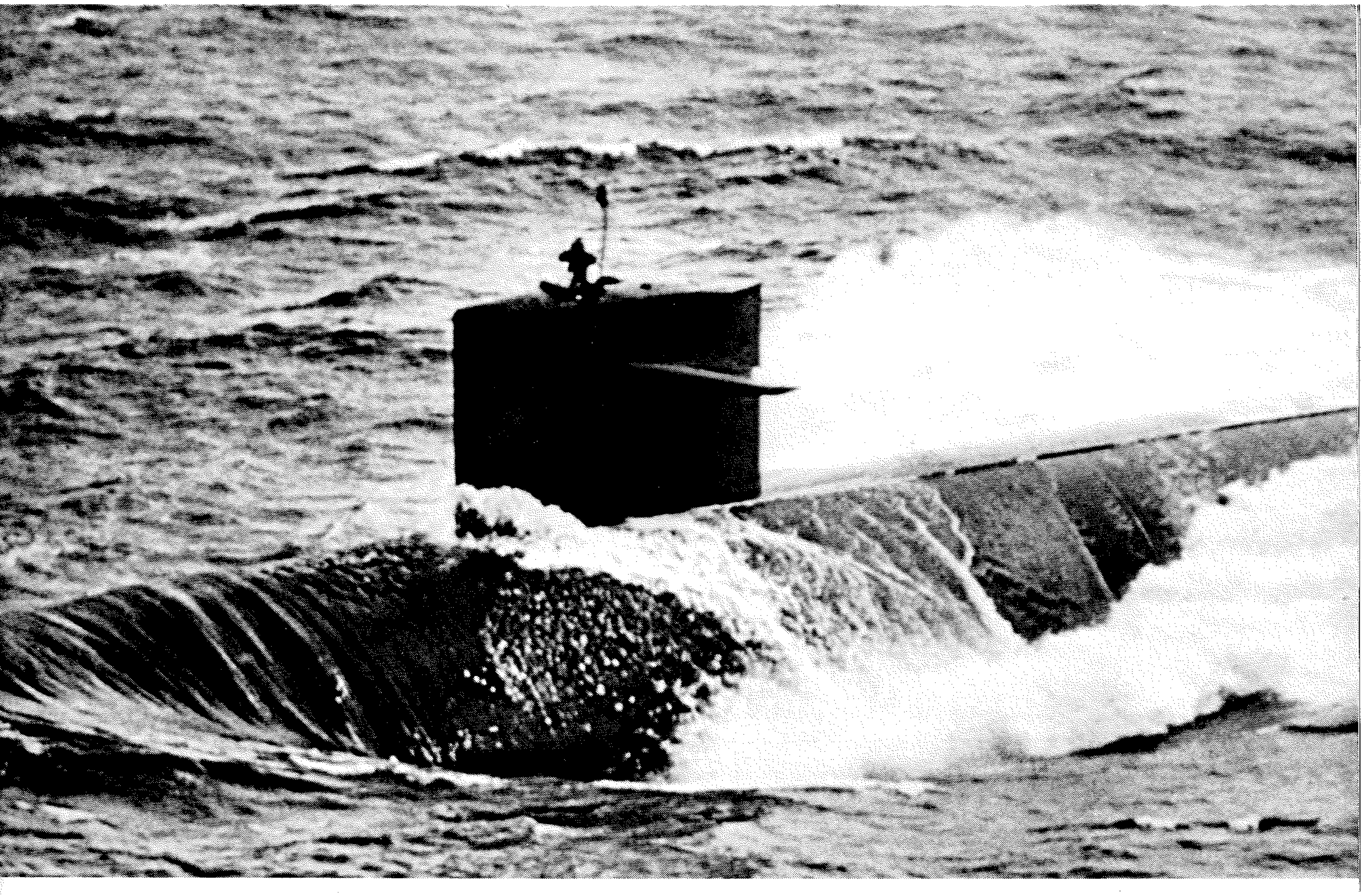

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Engineering&Science

October-November 1977/Volume XLI/Number 1

ON THE COVER: Robert P. Sharp, professor of geology, in his favorite classroom—the field (page 8).

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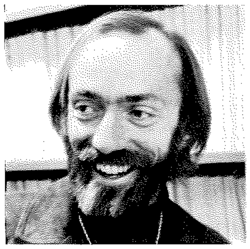
In This Issue

Familiar Figure
With Lee DuBridge behind the podium, commencement last June was in some ways just like old times, the main difference being that on this occasion he was the speaker of the day instead of the presiding officer. Nevertheless, he was still doing what he has been doing ever since 1946 when he became Caltech's president, namely, his best for the Institute. Though DuBridge, who has been president emeritus for nine years, claims that he has given up his "addiction" to public speaking, "Some Dilemmas in Science" on page 5 demonstrates that he hasn't lost any of his well-remembered ability to communicate.



Black Holes
Understanding the universe around us is partly a matter of understanding the deaths of stars, among which are those stars that die to form black holes.
Black holes and the waves of gravity produced by their birth throes are major preoccupations of those scientists whose research is called "relativistic astrophysics." One of the Institute's leaders in the field is Kip Thorne, who recently spoke on the subject at a Symposium on Science and the Future of the Navy, held to observe the thirtieth anniversary of the Office of Naval Research. "Probing the Universe: Big Bang, Black Holes, and Gravitational Waves" on page 17 is adapted from that talk and published here by permission of the Naval Studies Board of the National Academy of Sciences.

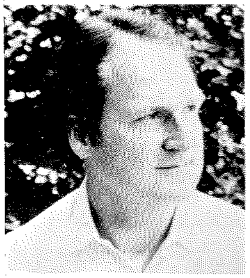
Thorne received his BS at Caltech in 1962 and his PhD from Princeton in 1965. He returned to the Institute in 1966 as a research fellow in physics and is now professor of theoretical physics. Since 1971 he has also been an adjunct professor of physics at the University of Utah. A distinguished scientist, Thorne is an articulate interpreter of science as well. In 1969, for example, he was winner of a $1000 prize from the American Institute of Physics - United States Steel Foundation for the year's best science writing in physics and astronomy.

Model Scientist
To discuss the potentials and limitations of computer simulation of the industrial society is no small assignment. But it didn't stump Donella Meadows, associate professor of environmental studies at Dartmouth College, who recently did just that at Caltech's conference on The Next Eighty Years. What is more, she brought a refreshing objectivity and a straightforward vocabulary to the task. "Computer Modeling: How Good Is It?" on page 11 is adapted from that talk.

A graduate of Carleton College, BA '63, and Harvard University, PhD '68, Meadows began her career as a biophysicist specializing in enzymology and spectroscopy. She has since become more and more interested in and occupied with systems analysis, and she has a considerable list of publications that bring both of these competencies to bear.

In 1972 she was a co-author of a highly controversial book, *Limits to Growth,* which presented a computer model of the world. Partly as a result of that controversy and also to compare other world models as they have been developed and elaborated, the International Institute for Applied Systems Analysis has been set up in Laxenberg, Austria, by the United States, the Soviet Union, and several eastern European countries. Meadows is currently a research scientist on its staff.

The Rolling Stones
According to alumnus John D. Bush, '55, his interest in primitive technology developed after he bought an abandoned granite quarry. Bush wanted to move some of its stones, but they weighed up to 200 tons and he didn't have a crane. Assisted by his BS in mechanical engineering, professional experience as a machine design consultant, and a look into such historical records as he could find, Bush was able to devise his own methods.



In fact, not only did he move some of those stones, but he came up with a theory of how the ancient Egyptians built the pyramids, using much less manpower than is generally assumed. In "The Rolling Stones" on page 23 Bush tells how it may have been done. "It's too bad," he says, "that Cecil B. DeMille didn't know my theory before he made *The Ten Commandments.* He could have saved a lot of money on extras."

Our Man in Bucharest
When a devastating earthquake struck Romania on March 4, Caltech alumnus Frank Lamson-Scribner, '46, was there. "Bucharest '77—Richter 7.2" on page 24 is his on-the-spot account of the experience, and we have illustrated it with a photograph of the devastation that we got from one of Caltech's noted authorities on earthquake damage to structures, George Housner, Carl F Braun Professor of Engineering.

Lamson-Scribner happened to be in that place at that time as a member of the World Bank's Economic Development Institute (EDI), an organization whose objective is to promote a more efficient economic development by training officials from the developing countries. Since 1973 he has helped with the training of over 100 senior Romanian officials in industrial project analysis. He uses a course that he designed especially for centrally planned economies as a cooperative effort of EDI and the Romanian Communist Party's institution for training senior officials.

Home for Lamson-Scribner is a house near Annapolis, Maryland, that he built almost entirely by himself, "putting my engineering education to good use," he says, "but occasionally wishing I had had a good shop course."

COMMENCEMENT 1977

Some Dilemmas in Science

by LEE A. DuBRIDGE

It has been nine years since the last time I stood here. Probably none of you students have seen me here before, but the older members of the faculty can tell you that I appeared 22 times in succession between 1947 and 1968.

On most of those occasions I did not have to give the principal address. I did that only when we were unsuccessful in finding someone else who would come. That may the reason I am here today.

In any case, I thought that if I were a graduating student at Caltech, I would like to hear from an old-timer—especially a *very old* timer—something about what the world of science and technology has been and *is* all about. What have been its successes and its failures? What are its prospects and its problems?

Let's start out by asking the question, What is the *status* of science today? What is the status of your own field of science—physics, mathematics, chemistry, biology, geology, astronomy, or whatever; and what is the status of any of the many fields of applied science and engineering?

Now the vague term "status" can mean many things. It can mean how a given field is progressing. Is it continually turning up exciting new discoveries or important new applications? Or is it at a plateau where new things appear ever harder to come by?

Status can also mean the relative place that a particular scientific or technological endeavor has in the hierarchy of science as a whole. Is too little or too much attention being given to applied science in comparison with basic science? Are the various fields of basic or applied science being supported in proper relation to each other? Are we under- or over-emphasizing those areas which are of current social importance—such as energy, the environment, prevention and cure of disease?

Status can also mean the *social* importance of science. Does it have a high or low priority among the many other fields of human endeavor? Is it adequately supported by society, and do active scientists and engineers have a respected place in the community?

Please notice that I am asking these questions—not giving or implying any answers. I don't even know the answers! As one gets older, one seems to be less positive about answers to tough questions. All of us have probably given too many wrong answers in the past. Also, we know that answers acceptable today may be obsolete tomorrow. And answers acceptable to me may appear quite wrong to you. However, these *are* questions that you should ponder.

One problem in answering questions about the present or future of science is that it is not a predictable or programmable enterprise. It is, rather, an exploration of the unknown. And by definition, the unknown is the unpredictable.

You are all familiar with the unpredictable results that have emerged from research in basic science, such as the discovery of the electron, of X rays, radioactivity, nuclear reactions, relativity, the quantum theory, the elucidation of the structure of organic compounds, the nature of genetic material, the expanding universe, the motion of tectonic plates, and all the rest. How would *you* have placed your bets on which area or sub-area of science would be the most productive in, say, 1910—1960—or 1977? My advice is: Don't put your money too heavily on any assumption of just how or when the next mysteries of nature will be discovered, or how they may be used.

Even many fields of *applied* science are not predictable. When I was called to MIT in 1940 to explore the possible military applications of microwave radar, our ambitions were very modest. We were told of one or two simple devices that it seemed practical to develop—and this might take the efforts of 30 or 40 physicists for three to six months. Five years later, 4,000 of us were at work, and over two billion dollars' worth of microwave equipment had been ordered by the military services for use in every theater of war. A whole new era in the application of radio and electronic technology had been introduced. We never dreamed that some day a highway patrol officer equipped with a tiny radar set would arrest you for speeding, nor that radar measurements would some day tell us about the surface structure of Mars and Venus and allow us to track a tiny spacecraft more than 200 million miles away.

On the other hand, 30 years ago, many nuclear physicists were convinced that nuclear power reactors were the final and immediate answer to our need for cheap and abundant energy. Fossil fuels would soon be unneeded. Well, it hasn't turned out to be so easy.

Again, 30 years ago, when the transistor was first being introduced, I was told emphatically by an electronics expert that the transistor could never replace the good old vacuum tube. It was too expensive and too unreliable. Well, take a look at your little pocket calculator now and see how wrong *that* was.

Does all this mean that if most *any* field of pure or applied science has a chance, even a seemingly remote one, of turning up something new and startling and important some day, therefore *every* scientific project should be given all the support it says it can use?

That is just one of the dilemmas I want to talk about. The dictionary says that a dilemma is "any situation involving a choice between unpleasant alternatives." I have not found a word to describe a choice between *pleasant* alternatives—although sometimes that isn't easy either. It would be pleasant to have more money for research in astronomy, and also in, say, chemistry. The unpleasant part is that we may not be able to do both. It is still more unpleasant if we can do neither. And I assert those are still dilemmas.

Life would be much more pleasant at Caltech and many other places if more money, and more good people, were available in many areas of teaching and research. But, with limited resources how do we make the unpleasant decision of how much goes to each? And *who* makes that decision?

In Caltech's case there exists a modest and, we hope, growing supply of private funds for research, and the decision as to how to use them can be made by people on the campus. You may not like all their decisions—but at least the people are right here where you can get at them.

But, for the bulk of university research these days, the decision is made in Washington. Now, I don't despise Washington as much as some people do. I worked there a year and a half and saw lots of smart and dedicated people working on just this problem. After all, Frank Press and Harold Brown are there *now*. But they are working under severe constraints. Some are imposed by Congress, some by the Budget Bureau, and some by the sheer impossibility of making valid judgments on the relative future scientific merits of the proposals that come in from various fields of science, from various scientific groups, in various parts of the country. (Don't forget that Congressmen are very zealous in insisting on a "broad geographic distribution" of research funds. They don't like to see all the money going to Harvard, MIT, and Caltech—as if it ever did!)

One of the serious restraints imposed by Congress was an amendment that removed the authority of the Department of Defense to support any basic research "not directly related to military applications." The fine research program of the Office of Naval Research was thus substantially dismantled, and no other agency was provided with the necessary funds to take over this research support. Though this amendment was later allowed to lapse, the damage was done, and even other agencies, such as NASA and the Atomic Energy Commission, decreased their support of basic research that was not clearly related to their primary missions.

Thus, only the National Science Foundation now has basic research as a primary mission, and in recent years even many of its budget increases have been provided specifically for applied rather than basic research.

And this leads to the second dilemma. How *should* the national research and development effort be divided between basic research, applied research, and engineering? There is no easy way to answer this question. One problem is that no one can define a sharp dividing line between these three areas of endeavor. They merge into each other and often overlap. Thus it is not easy for universities or their faculty members or their students to decide into which field they should direct their energies and talents.

It is tempting for research people to "put their effort where the money is." That is where the jobs will be, too.

But this may compound the problem. If more proposals for more money go to the government for popular programs, the government agencies will seek from Congress more money to meet this demand, and so the rich get richer. And yet, isn't it better for the scientific community to make these judgments, rather than a government bureaucrat?

Now we all know full well that there is a need for more research aimed at meeting urgent needs of our society. But we shall not succeed in this direction if we fail to produce the new fundamental knowledge on which future applications must depend. Nor will we succeed if we do not seek earnestly to make that knowledge applicable to human needs.

There is no consensus on this dilemma, even within the scientific community. It is a problem that you of the younger generation will face for years to come. I trust you will be thinking about it.

My next dilemma has to do with the public attitude toward science and technology. Since Congress supplies such a large proportion of the money for research, we must expect that public attitudes will have much to do with how Congress acts—how generous it will be, and what constraints it will impose. Is there any way of resolving the deep conflict between the way in which scientists seek the truth and the way in which legislators proceed? Scientists go to the laboratory; Congressmen go to a committee hearing. Is there any way that scientists and lawyers can learn to talk to each other intelligibly? If not, we are in deep trouble. This may be our toughest dilemma.

Clearly the general public must be educated to the point where the values, the limitations, and the promise of science and technology can be seen in proper perspective, properly related to social, political, and cultural problems, and then properly supported. In this task of public education we can all do our bit.

The public has, of course, heard of some of the spectacular successes of science—such as landing men on the moon and sending spacecraft to Mars, Venus, and Mercury—and soon to Jupiter and Saturn. Yet now, ironically, Congress threatens to cut off all future planetary missions. Instead of fully appreciating these achievements, the public asks why, if we can send men to the moon, can't we cure cancer, clean up our slums, stop pollution, and quickly find new sources of energy? The answer is that going to the moon and Mars was *easier*. The basic science and technology were well in hand when these missions were started. But for these other problems we need more scientific knowledge, or new technologies—or perhaps, more political know-how.

It was easier also when the government was itself the purchaser of these new technologies. But for new sources of energy, for example, the consumer must pay. And there is a limit to what he can afford, or thinks he can afford. If all the energy I use in my all-electric home were generated by currently available solar cells, I figure my power bill would be about $4,000 a *month*. I *know* I can't afford that!

Again, many people have turned against technology because it has without doubt introduced into the world many new hazards to life and health. But it has also greatly reduced even larger hazards of starvation, disease, and poverty. How safe do we insist on being? Is nuclear power a greater hazard than mining, transporting, and burning an equivalent amount of coal? Is saccharin a greater hazard to health than more sugar? Are certain insecticides a greater danger than hordes of insects that kill plants and trees or people? Is there a way of judging the balance between the hazards and benefits of a particular scientific or technological advance? Another dilemma!

But the greatest dilemma of all is what we, the people of the world, are going to do about the crisis that will be facing human beings in the next 25, 50, and 100 years. This crisis is related to a rising population and rising expectations coupled with limited natural resources and a limited supply of fertile land.

It relates to the rise of rapid communication between all people of the world, accompanied by a rising hostility between many. It relates to the decay of morality in the world's societies. As the world has solved many of its technical problems, it faces far more difficult problems in the social, economic, political, and ideological areas. The confidence that existed 50 years ago, or even 25, that peace and prosperity would some day come to all people, has given way to the fear that the age of affluence for people in other parts of the world may still be an impossible dream.

New advances in science and technology will surely alleviate some problems, such as those of energy, food production, use of natural resources, environmental degradation, and human health. But can we manage breakthroughs in the social, economic, moral, and political spheres so that new technologies can be effectively and humanely used? We don't know.

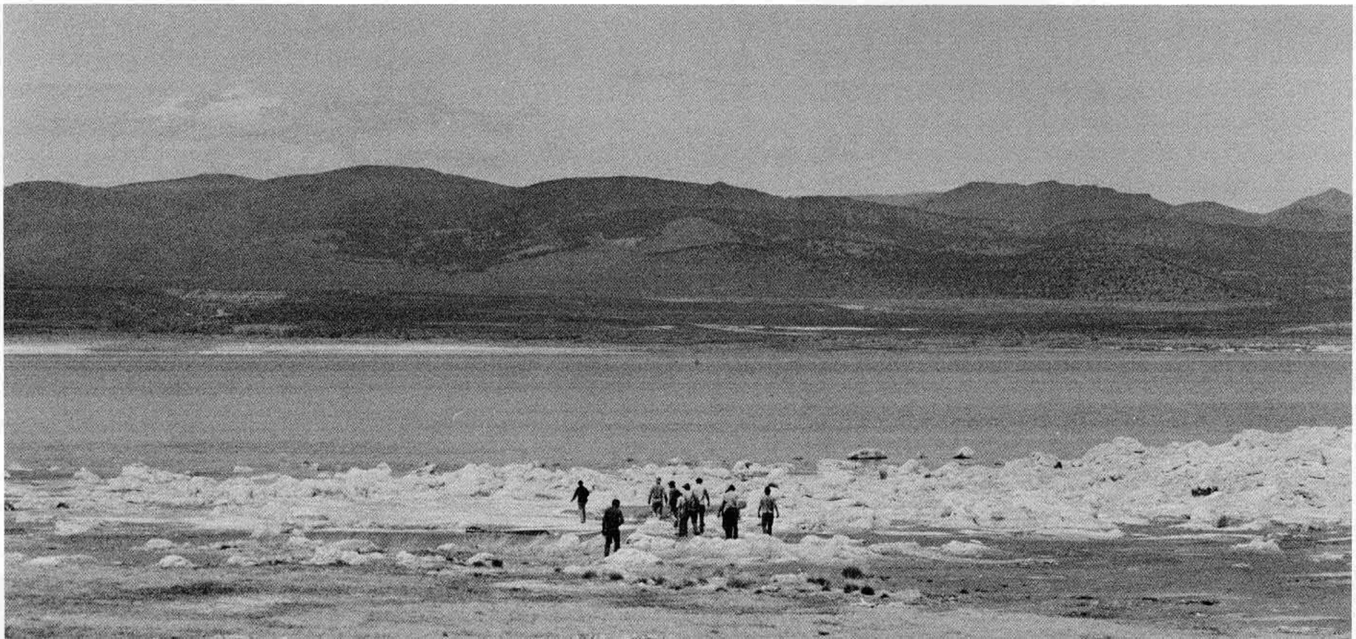
At least we are all more aware of these problems than we were a few years ago, and many people are now trying mightily to solve them, or at least to find ways around them. Most of you will live to see the outcome—and you will also have a chance to help make the outcome a more hopeful one.

Life would be uninteresting without problems to solve and challenges to face. My dear young people, *your* lives should be *very* interesting. My best wishes. □

Ge 136 — It's All Outdoors

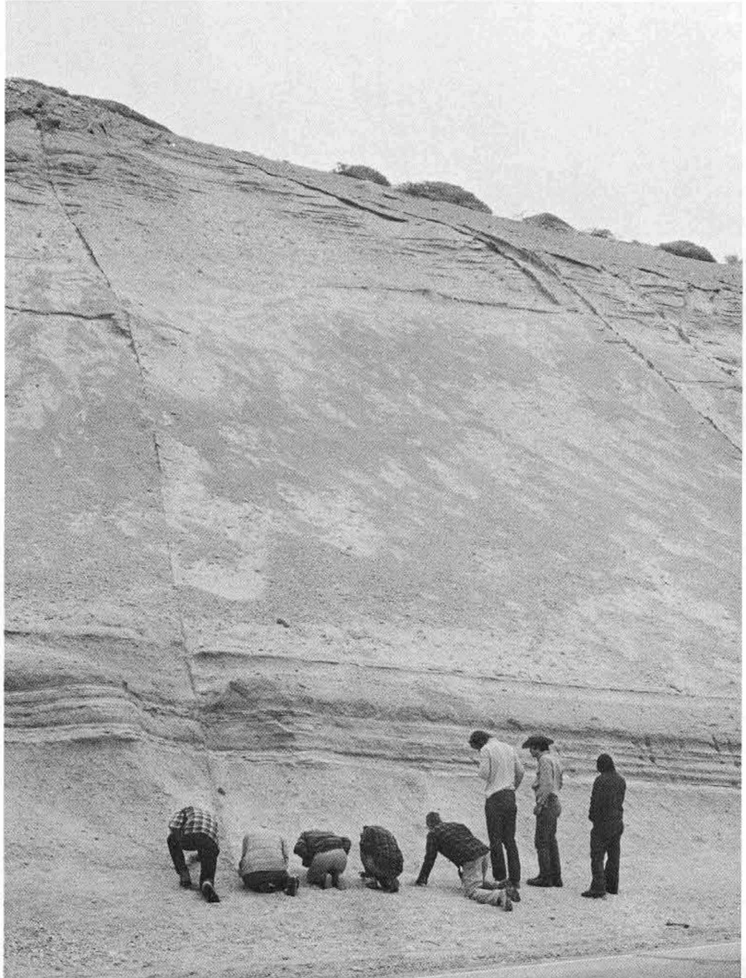
Craps? Marbles? Mumbledypeg? No, the Ge 136 class is looking over a map produced by Mike Malin (now a PhD), who was the expert on this area in the Mono Basin.

Students hike through the travertine deposits at Mono Lake, which has been shrinking spectacularly since the city of Los Angeles began taking three-quarters of its water. This saline lake, which has no outlet, used to be stabilized.



In the Convict Lake area, Peter German, '77, as resident expert, describes the glacial history of Convict Lake canyon and creek. The lake is behind him; Mt. Morrison (it co-starred with John Wayne in *True Grit*) is on his right.

At the top of Sherwin Grade, which rises northward from Bishop, students collect the glassy minerals that tell the age of Sherwin Till. This big pumice cut is riddled with clear crystals of potash felspar, which can be dated by potassium argon.



Caltech's geology division used to just train geologists. Now it turns out geochemists, geophysicists, and planetary scientists as well—and a lot of them have never had a course in classical geology. To rectify this situation, Robert P. Sharp, professor of geology, introduced an informal exercise that has now become a full-fledged and thriving course.

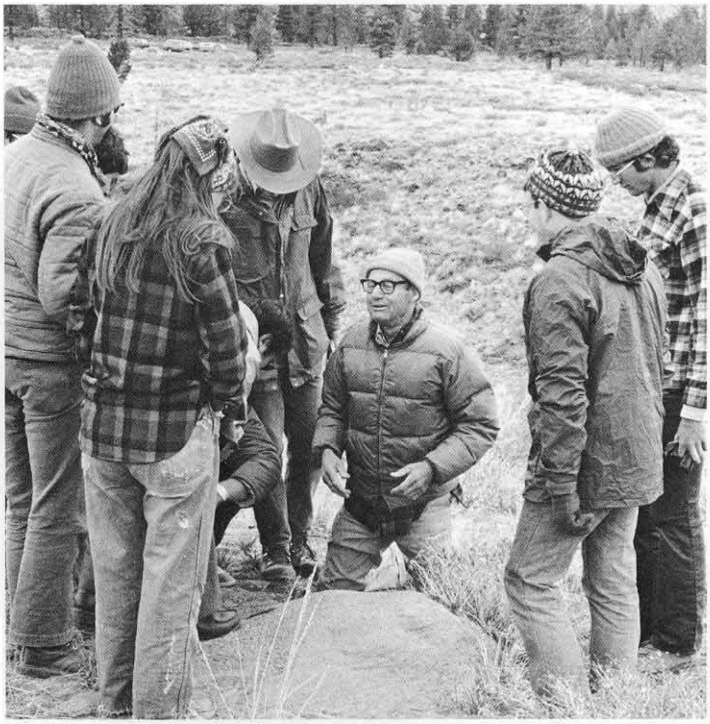
Ge 136 (Regional Field Geology of Southwestern United States) never meets in a classroom. Given once a year, usually in the spring term, it consists of at least nine days of weekend field trips to various areas of the Southwest. Since nine or ten different trips are offered in different combinations, some students take the course three times.

It's not all a lark either. Each student has to become an authority on part of the area the group will be exploring, or on special features to be found there, such as volcanics or earthquake faults.

On these pages, some highlights from this year's trip along the east face of the Sierra.

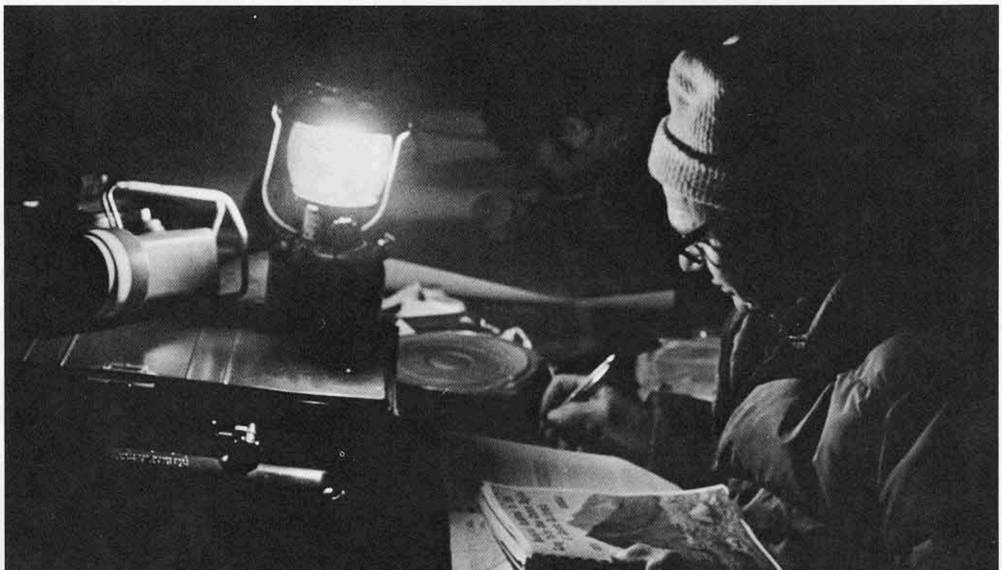
The view from the top of Sherwin Grade—site of one of the older glaciations of the Sierra Nevada, where Rock Creek Glacier spread out as a bulbous mass. The glacial deposits were later buried by a flow of hot fragmental pumice and hot volcanic rocks.

Sherwin Till, a glacial deposit, is 700,000 years old. Because of its age, normally hard granitic rocks in the till are disintegrated and crumble when they are touched.



Sharp worships a favorite rock on Casa Diablo Till, near Mammoth. There is one lava flow on top of this till, another below it. This boulder got baked in the till so that remnants of sands and gravel adhered to it.

Students only work from sunup to sundown, but the instructor has to keep plugging away into the night to stay ahead of them.



Computer Modeling: How Good Is It?

by DONELLA MEADOWS

COMPUTER MODELING IS A BABY THAT NEEDS TO DEVELOP AND TO BE GIVEN SOME TOLERANCE

As I go around telling people I'm in the business of making computer models, I seem to run into two and only two reactions. On the one hand I often get an expression of deep suspicion. I know that person is in the camp that thinks computer models are worthless. On the other hand I sometimes detect a note of awe and almost worship—probably best exemplified by a lady who called us up once at MIT and said she heard we had a world model and she'd like to ask it where she could find her dog. This group of people seems to have the idea that a computer model can deliver perfect information about anything for any time in the future.

I very much dislike both those attitudes. As a member of the field, and a fairly new and still skeptical member, I believe that computer modeling has too much potential to be dismissed or stopped at this point in its development. I also think, however, that computer models should be used tentatively and with a great deal of questioning, especially for the next few decades.

I'd like to summarize here my vision of the future of computer modeling as a tool for understanding how complex social systems behave. Let me start by defining a *model* as any set of assumptions or generalizations about a complex system. All of us carry models around in our heads, which Jay Forrester has called *mental models* (in "The Counterintuitive Behavior of Social Systems," *Technology Review,* January 1971). They are the sets of working assumptions and abstractions that we have drawn together from our experience in dealing with the world. Of course mental models must be great simplifications of the real world. You don't weigh down your mind with every detail of your firm or your town or your household.

All the decisions you make are dependent on mental models—upon simplifications of the world and not upon perfect, detailed knowledge of every aspect of every system that you deal with. The models behind your decisions and mine are incomplete and imperfect. They *must* be to be useful; if they were as complicated as the real world, they would be as hard to understand as the real world. The essence of any good model, mental or mathematical, is insightful simplification, the omission of trivia, and the inclusion of just what is important for solving the problem at hand.

There are many kinds of decisions that we as actors in social systems need to make about the future. Therefore we need many kinds of models. I'll just give you a few examples of the different kinds that are appropriate for the various decisions that may face us.

To start at the easiest and most successful end of the spectrum, as far as computer modeling is concerned, we have to make decisions that involve a very clear set of goals and a clear understanding about the system that has to be dealt with. In this case one can state the problem in terms of some sort of optimization. We want to do things so that profits are maximized, or costs are minimized, perhaps. For example, a city has a lot of streets, and mail has to be delivered on all of them, and there are certain pickup points and a certain number of mail trucks and drivers. What is the most efficient way to deploy the trucks, lay out the routes, and work out the timing so the mail is delivered in the cheapest way possible?

Such well-defined decisions about the detailed implementation of some predetermined strategy toward a clear goal are the places where computer models are most used, most successfully, at present.

Moving toward an area of greater disorder, there are problems where the goals are not quite so clear, where the policy instruments are perhaps identifiable, but the interrelationships among them are uncertain. In many public policy issues, broad social goals are brought into question, and it isn't at all clear whether anything should be optimized. In these problems we want to ask what general combinations of policies will allow us to move in the right direction. We want to help out a poor country. Should we work on family planning or health care or miracle grains or all three or none of them? Having made that decision, *then* we might use an optimization program to determine, for example, the least-cost way of distributing miracle grains.

One model that I feel is successful in this area of general policy formulation is a simulation model of heroin addiction in the New York City area. It was made for a neighborhood mental health clinic during the time when drug addiction was rising in that area. The goal was fairly clear—to stop or reduce the rate of heroin addiction and the street crime associated with it. And some policy instruments were visible. One could hire more police and assign them to arrest addicts and pushers. One could block somehow the inflow of drugs into the region. One could try to set up neighborhood treatment centers and half-way houses. One could establish methadone programs. And so on. There was a huge argument about which of these approaches would be more effective and what the long-term consequences of each would be.

A computer model was made that simulated or duplicated the essence of the heroin system—the flow of the drug and the market for it, the rate of addiction, the movement of addicts in and out of jails and treatment programs, and the effects of all these things on each other. (This has all been described in *The Persistent Poppy* by G. Levin, E. B. Roberts, and G. B. Hirsch, published by Ballinger, Cambridge, Mass., 1975.) As one example, the model came out with the conclusion that stopping the flow of drugs would result, at least in the short term, in an increase in street crime and an increase in addiction rate. As drugs became scarcer and scarcer in the city, the price would go up, and those who were addicted would have to commit more crime in order to buy the same amount of heroin. Furthermore, pushers, finding the supply drying up and the costs going up, would try to hook more people, because pushers are nearly all addicts trying to support their habit. The general conclusion of the model was that no single policy could be very effective alone. Another conclusion was that the two goals of reducing addiction and reducing crime are sometimes in conflict.

That's an example of this middle area—a fairly clear problem with fairly clear interconnections between all the aspects of the problem. A computer model in that particular case was very helpful because it allowed a rather argumentative interchange about alternate policies to be discussed systematically, and the consequences to be laid out explicitly and logically.

At the far end of the spectrum of decisions that have to be made about the future are decisions about systems that are not well defined, where there are many interconnected problems, and where we are hardly even sure where to begin in analyzing the causes of the problem. These are undoubtedly the problems of the future; the problems that encompass an entire complex social system that is not totally understood by anyone. Russell Ackoff in his 1974 book, *Redesigning the Future,* calls problems of this sort "messes." He talks about the urban crime/taxes/housing/employment mess. Or the development/aid/population mess. Or the environment/resources/pollution mess.

I'm going to concentrate on the use of computer models in this area of messes, primarily because these are the problems we understand least, the places where our simplified mental models are most likely to backfire. Another reason for concentrating on this area is that I think this is where the potential of computer modeling is greatest and where the performance of the field at the moment is worst.

I'd like to go through five advantages that I think computer models might have in analyzing the "mess" area of social problems, and indicate my assessment at the moment of how well current models do with regard to each of the five advantages. My statements will be based on my own generalizations, or mental model, derived from a rather intimate knowledge of five global models and nine national models. The national models are reviewed in my forthcoming book, *The Electron Oracle,* written in collaboration with J. M. Robinson.

The first advantage I cite when I try to convince somebody that computer models can be useful is that the process of taking one's assumptions and putting them in a form that can be understood by a computer requires a tremendous amount of rigor, precision, and consistency—much more than one is ever forced to have in one's mental models. The computer forces you to define every term you use and to make all the definitions mutually consistent. If you make a mistake, it gives you back a rude message telling you that you haven't done things correctly. You must be precise. You must look very hard at the data and at your assumptions.

There are many good examples of this, but I'll just cite one. A model has been made of the development of agriculture in Korea. The interdisciplinary team making the model started by looking at all the agricultural statistics of Korea, and they quickly found some puzzling figures. The reported agricultural yields were exactly the same as those in the five-year plan. With a little further checking, they found that the same Korean agency that made the five-year plans for agriculture also collected the data on the actual yields and production values. This had been going on for a decade, and no one had ever realized it until a computer team started to look at the numbers. Well, even before the model was made, that was a useful exercise. As a result, the whole data collection system in Korea was completely revised, and now the numbers, I am told, are considerably more reliable. That's a good example of the enforced rigor of modeling producing better understanding.

But most of the computer models I've looked at are rigorous about easy things, and unrigorous and inconsistent about the difficult things. As an example, one model requires you to predict outside the model, as an input to the model, what the population growth rate of a country will be, what its GNP (Gross National Product) growth rate will be, what the relationship between production and pollution, and production and resource-use will be. And once you have predicted all those things, it tells you such things as how many nine-year-olds there will be in the year 1985 or how much steel will be used.

This is rigorousness, precision, and consistency—that is, the demographic program that generates the number of nine-year-olds is correct—but in fact that model is little more than a glorified mental model. It takes your mental projections of a lot of important things, and then goes through some calculations and gives out some information that looks rigorous, but is probably not consistent, because your projections were probably not consistent. That's deceptive rigorousness, and it's very common.

A second potential advantage of computer models is that they are explicit. They are written down. They are criticizable. One can look at the assumptions and say, I agree with that, or I don't agree with that. That is impossible to do with mental models. If you've tried to pin a friend down on what he thinks about something and what are all the assumptions and experiences that lie underneath his opinion, you'll find that mental models are vague and moving targets. It's even pretty hard to figure out all the assumptions behind your own mental models.

Of the 14 models I have dealt with, I would say that four of them were really criticizable by me as a professional modeler. That is, I could see the equations, and I could understand all of them. These four models are excellent examples of the accessibility and explicitness of computer modeling.

Two of the others, I would guess, could not even be examined by their makers. That is, the programs that led to the published outputs were lost. They couldn't be repeated even by the people who made the models. These were exceedingly complex models. Nobody remembered quite what went into them.

The rest demonstrated intermediate levels of accessibility. The equations were generally around somewhere, though rarely published. Generally the modelers could at least trace what kinds of inputs produced what results. Very often they didn't really understand what was going on in the computer because the models were so complex, and the experiments done with them were so poorly documented.

There are many causes for this problem of impenetrability. One is the very size of many computer models; the modelers have forgotten that the purpose of modeling is to simplify rather than to duplicate every detail of the real world. Another problem is that modelers tend to be modelers and not writers, and therefore they sometimes have a hard time communicating with anything but a computer. I would say that your first basic right as an audience for a computer model is to have that model explained to you in a language you can understand. If the modeler can't do that, he doesn't understand it himself. In that case I think the model should be dismissed.

A third possible advantage is that a computer model can be much more complete and comprehensive than a mental model. There's an interesting psychological rule that says the human mind can handle about seven variables at one time, and after that the mind gets boggled. Well, computers can handle thousands or millions of numbers with no problem. There's no practical limit to the complexity of a computer model, and therefore it can contain and process more information than your head or mine. In fact, it could combine the information from both our heads and in that way come up with a more complete view of the world than either you or I have by ourselves.

Again, there are good examples in this area. Models have served as hubs for interdisciplinary research, where people from a lot of different fields have come together and used a model as a communication mechanism. For example, demographers and economists have been brought together for some modeling efforts, and public health experts and water resource engineers in others.

On the bad side, there are some large holes in the content of nearly all the computer models of social systems I have seen. Computer modelers tend to zero in on that part of a social system that is measured by statistical data bases. Where there are numbers, censuses, national economic accounts, and preferably where there are 25 years worth of consistent numbers, modelers pay attention. But glaringly absent from nearly every model I've looked at are goals and motivations and politics, cultural factors and norms, and the environment and natural resources. The data on these things are scattered, if they exist at all. Information is available, but it is not precise. Therefore it doesn't get included in computer models, although our mental models tell us it might be crucial to the system.

There's nothing to prevent a sociologist or psychologist or ecologist or poet from translating his impressions of how human society works into a computer equation, nothing except the pseudo-scientific prejudices of modelers. They seem to feel that information that comes only from a mental model can't be very good information. I disagree with that very strongly. Our mental models are full of accumulated wisdom about why people do what they do, what their goals are, how political systems work. That wisdom can be put into models, and a few brave modelers are trying to do it. Unfortunately, they tend to get laughed at by other modelers.

If it sounds contradictory for me to bewail large models on the one hand and yet complain that they are incomplete on the other, let me emphasize that there is a distinction between *complicated* models and *comprehensive* models. A comprehensive model need not be complicated (though it might be). I am saying that too many models are unnecessarily complicated and insufficiently comprehensive.

LET ME EMPHASIZE THAT THERE IS A DISTINCTION BETWEEN COMPLICATED MODELS AND COMPREHENSIVE MODELS

A fourth advantage—computer models can proceed with logical accuracy from a set of assumptions to the conclusions that follow from those assumptions. Drawing logical conclusions is something that mental models are very bad at. You have undoubtedly heard discussions in which people agree exactly on their assumptions about some system and then get into a big fight about what those assumptions mean. That is one place where computer models can help. The computer doesn't guarantee the assumptions are right, but at least, given those asumptions, the conclusions can be derived error free.

For those four models whose assumptions I was able to penetrate, I believe in each case the conclusions are also correct. For the others, I'm not sure, for two reasons. One is the simple possibility of errors of translation. One of these models has 80,000 numbers in it. There is almost a 100 percent certainty that *one* of those numbers was typed wrong. There is no way of finding it. That model costs about $2,000 each time it's run because it's so huge, and therefore it's not run very often—and testing by doing many runs under different conditions is the most common way of detecting typing mistakes. So when models get very big, I get suspicious about their logical infallibility.

Another kind of error arises in the interpretation process. Even if the computer has proceeded logically from assumptions to conclusions with no typos, conclusions don't come out of a computer in terms of simple wisdom, distilled and delivered to your doorstep. They come out in the form of sheets of paper, covered with numbers. The modeler must sit down with those numbers and form a conclusion from them. In other words, a mental model is required to interpret the results. It's not easy to derive wisdom from a stack of paper six inches thick covered with numbers, even if every one of those numbers is meaningful and correct.

I'll give you a glaringly bad example of an interpretation error. One model was designed to determine what resources might be used in the United States over the next 30 years, and whether there would be any shortages. The conclusion of the study was that there were no serious problems in sight. I took one look at the figures and noted that the model had happily allocated for United States consumption 150 percent of the world's known copper resources and 90 percent of the world's tungsten resources (nearly all of which are in China). The computer modeler had apparently not noticed that. I noticed it only because my bias was opposite from his; otherwise I wouldn't have seen it either. You can assume that every modeler, including me, unconsciously reads his own bias into the numbers on the paper.

That's a bad example. There are good examples. In several models the results surprised the modelers, and when they looked hard they decided the model was right and their mental model was wrong. One model of the Sahel region in Africa led to the conclusion that the Sahel would be much better off if all current foreign aid programs were stopped immediately. That was a conclusion that surprised the aid-donating agency very much, as well as the modeler. I believe that result, because I can reason through with the help of the model why it comes out that way. The modeler now believes it. I don't think the aid agency does yet. But it seems that the assumptions put into the model are roughly agreeable to everyone, and everybody had been coming to the wrong conclusion on the basis of those assumptions.

Fifth, a computer model can be tested and altered a lot easier than things can be tested in the real world. You can try wild ideas in a computer without breaking anything or upsetting people. You can try out wide ranges of numbers where you are uncertain, to see whether your uncertainty makes any difference. It's also cheaper and faster to run a computer model through the next 100 years of history than it is to try something in the real world and evaluate it 100 years later.

Modelers are in fact quite bad at testing models, probably because it's not in their best interests to do it. A model must be really well constructed to produce sensible results under a wide variety of assumptions. Most tests reveal inadequacies of the model, rather than knowledge about the system.

The best-tested model I know is my own world model, and that's only because all of my enemies tested it. I would recommend this procedure. They did things with it that it never would have occurred to me to do, and that was good—we all learned things about both the model and the real world in the process.

Another and more serious problem with testing is that the inherent logic of a number of modeling techniques really prevents policy testing. For example, the relationships in econometric models are derived carefully from historical relationships, data from a real system, operating in one particular way. One cannot use such a model to test the effect of changing any single relationship or any new policy, because the model does not contain any causal hypotheses connecting that change to all the other elements to which it's connected in the real system. That is, in the real system, changing one number *here* will cause hundreds of shifts in other numbers all over the place. But the model won't do that, it will just change the one number here and so it will give misleading results. Econometric models can only project the system continuing to operate in the way it has historically operated; they cannot properly represent a changed system without more data on the changed system. The only kinds of models that are appropriate to test policy changes are simulation models in which politics, goals, and motivations are explicitly represented, so that a new policy can give you back the changed behavior of people who are responding to that policy.

To summarize those five advantages of computer models in analyzing social "mess" areas: (1) Computer models *could* be more rigorous than mental models, but at present they are only rigorous about easy things, such as demographic changes. (2) Computer models *could* be explicit and criticizable, but their large size and the sloppy documentation habits of some modelers make many of them inaccessible. (3) Computer models *could* be more complete than mental models, but usually they are less complete, because they do not include psychological, political, or ecological factors. (4) Computer models *could* proceed without error from assumptions to conclusions, but only if they are small enough to be checked and if their output is interpreted correctly. (5) Computer models *could* be easily tested and altered, if they could be run cheaply, if modelers were trained in testing and motivated to test, and if the logical foundation of the models were causal, so that tests may have some meaning.

To realize the potential advantages of computer models, modelers have to be more responsible, more imaginative, and less pseudoscientific. Clients, meaning the people to whom the models are addressed—the policy-makers and decision-makers—need to be much more sophisticated about the appropriateness of a model, more persistent in finding out what's in it, and more critical all the way along the modeling process.

Believe it or not, after all that criticism, I think that computer modeling is a very promising new field. I wouldn't be in it if I didn't think that. But it is a field that is still in a primitive state of development. It has been catapulted into a position of too much power. Clients are entirely too eager to get information about the future; they need it badly in order to make important and urgent decisions. Modelers are too eager to supply such information, and they seldom provide sufficient warning about what they can really say on the basis of their models and with how much certainty. My fear is that the too-rapid development of the field is likely to lead to a backlash of disillusionment that may result in throwing the baby out with the bath water.

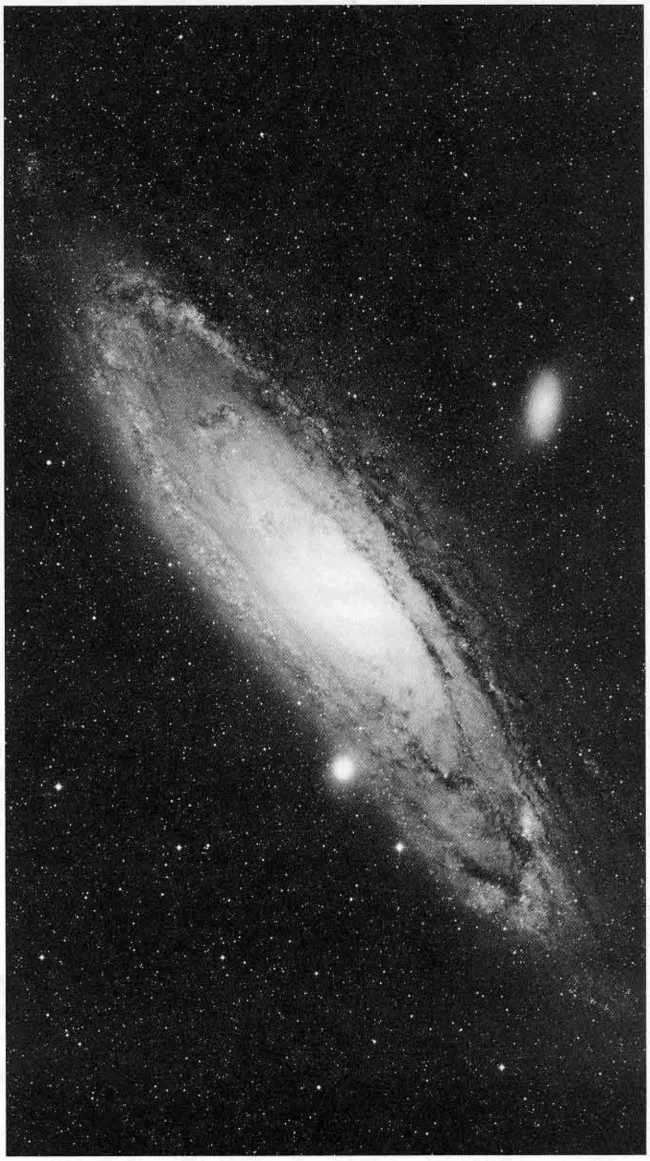
Computer modeling *is* a baby; it needs to develop and to be given some tolerance. It should be regarded as more of a basic research field than an applied one—except for those optimization models I talked about first. But with regard to social messes, the field is just beginning, and neither modelers nor clients should push it too far.

Let me end with a conditional prediction; a prediction of the "if . . . then" sort, which is the kind that mostly comes out of computer models. Over the next 80 years I believe that there could be improvements in our understanding and control of the complex interconnected messes that human society generates. Systems that are now out of control could become understood and regulated to increase human welfare. The tools for gaining this kind of understanding are systems analysis and computer modeling. Whether these tools are actually developed depends, unfortunately, on what I can only call human wisdom. Computer modeling has to be developed carefully and rationally and humbly, and for the benefit of all, rather than the benefit of the elite few who happen to seize the tool first. This could be said of any new technology—and computer modeling is a powerful, and therefore both promising and threatening, new technology. Whether human wisdom will be sufficient to develop that tool and to use it well, I will have to leave to the judgment of your mental models. □

Probing the Universe:

Big Bang, Black Holes, and Gravitational Waves

by KIP S. THORNE



The galaxy Andromeda, as seen through the 48-inch Schmidt telescope.

Astronomical research in recent decades has brought considerable understanding of the universe around us. We know, of course, that the universe was created in a "big-bang" explosion some 12 billion years ago. We know that the primordial gas, expanding outward from that explosion, condensed to form galaxies such as the great Andromeda galaxy shown here. We know that those galactic condensations occurred when the universe was roughly one billion years old, some 10 to 20 billion years ago.

We know that each such galaxy is made of some 100 billion stars, that each star has a finite lifetime, that stars are continually being born and continually dying. We know that stars are born in great clouds of dust and molecular gas. We know that when they die, they die in remarkable ways, producing, for example, white dwarfs—objects the size of the earth but with masses like that of the sun and densities of some tons per cubic inch.

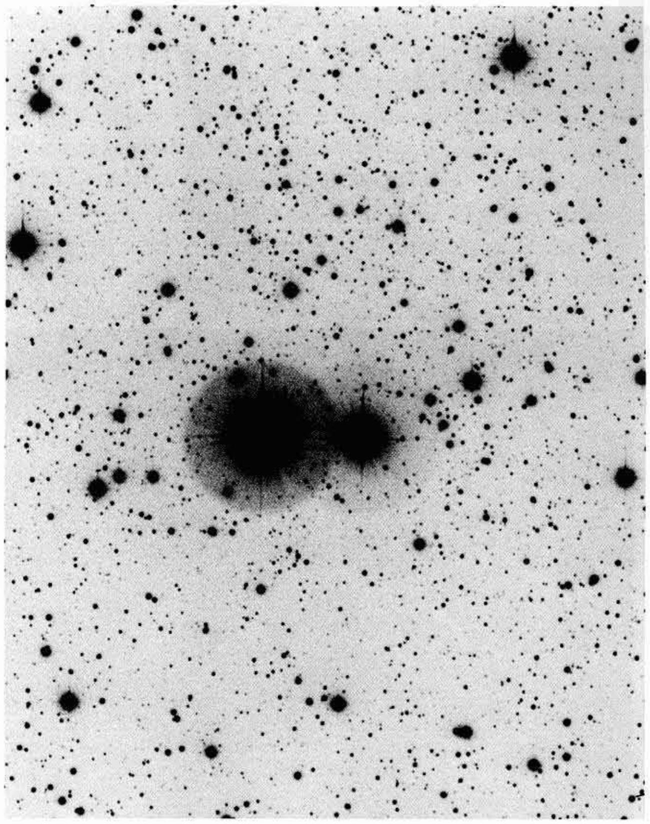
We know that other, more massive stars die to form neutron stars—objects only 20 kilometers across but weighing as much as the sun and having densities of a billion tons per cubic inch. We know that stars also die to form black holes—objects which are veritable edges of our universe in confined regions; objects down which things can fall, but out of which nothing can come; objects which seem more fantastic than anything conceived of by science fiction writers, but which Einstein's theory of relativity says must exist or Einstein is wrong.

In talking about the future of astronomy and things one hopes to discover, I could go from one topic to another, lighting your minds up with excitement for the many possibilities. But I have chosen instead to focus on just one small area—an area which to me is exciting, not least because, as a by-product of our inquisitive search, it promises to produce new technological innovations. This area is the challenge of understanding the deaths of stars, and particularly understanding black holes.

One can understand what a black hole is by imagining the fate of a very massive star—one perhaps 10 times as large as the sun—which has exhausted its nuclear fuel, and can no longer replenish the internal heat that supports it against the pull of its own gravity. Gravity then pulls the star inward upon itself into catastrophic collapse. Now, imagine a fleet of asbestos-covered rocket ships, all lined up on launching pads that float in the gaseous stellar surface. These rockets are to monitor the progress of the collapse by measuring the "escape velocity" from the star's surface—the velocity a rocket must achieve in its initial few moments of blasting, in order to successfully escape from the star's gravitational pull.

The first rocket, launched before the collapse begins, requires an escape velocity of, let us say, 100 kilometers per second. Later, as the star collapses, the gravitational pull at its surface becomes stronger because of Newton's inverse square law for gravity. Hence, a rocket launched when the star has collapsed to one quarter its original size requires an escape velocity of not 100, but 200 kilometers per second. And ultimately, when the star's circumference has shrunk to roughly 100 kilometers, the escape velocity grows larger than the speed of light. Now, we all know that nothing can travel faster than light—not light, not radio waves, not particles, not rocket ships, not anything. So the star at that point, with a circumference of 100 kilometers, cuts itself off from the rest of the universe and leaves behind something that can only be called a "black hole in space."

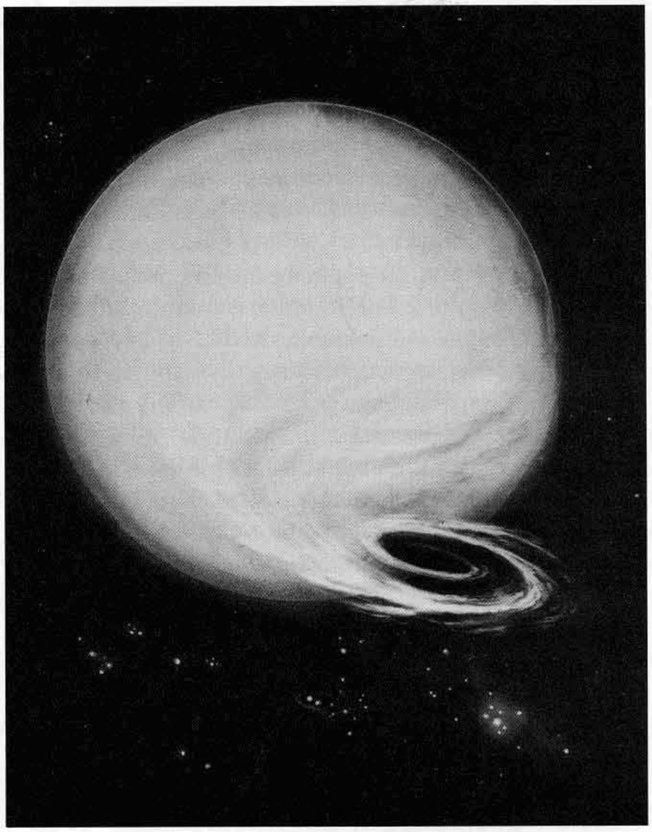
This kind of picture of a black hole—as simply a boundary with roughly a 100-kilometer circumference, out of which nothing can come—is only a shadow of what a black hole really is. When one tries to analyze black holes theoretically, using the mathematics of Einstein's General Theory of Relativity, one finds that a black hole in fact is gravity creating gravity. It is curvature of space creating further curvature of space. It is as though space itself were a gigantic rubber membrane, a membrane so massive in the neighborhood of the hole that it curves itself up so strongly as to prevent anything from ever getting out. The challenge for the astronomer in the near future is to study this extreme curvature of space observationally, to see if Einstein's predictions about it are correct.



Star field centered on the star HDE226868 (Cygnus X-1), photographed by Jerry Kristian with the 200-inch Hale telescope.

We are now at a point where astronomers are perhaps 80 percent sure that a black hole has been discovered. There are other good black hole candidates in the sky, but the very best one is an object shown above as photographed by Jerry Kristian of the Hale Observatories with the 200-inch telescope. The very brightest thing you see in the picture is a star whose number is HDE226868. The fact that it even has a number means that it's a very bright star indeed, so bright that if it were only 40 times more luminous, you would begin to be able to see it with your naked eye.

This star is an object from which we receive not only light but also X rays and radio waves. When its X rays were discovered, it was given the name Cygnus X-1. Thanks to the collective efforts of dozens of astronomers using X-ray telescopes on board satellites (primarily Ricardo Giacconi and his group at Harvard with the UHURU satellite), and thanks to radio and optical telescopes on the ground, one deduces that this



An artist's conception of Cygnus X-1.

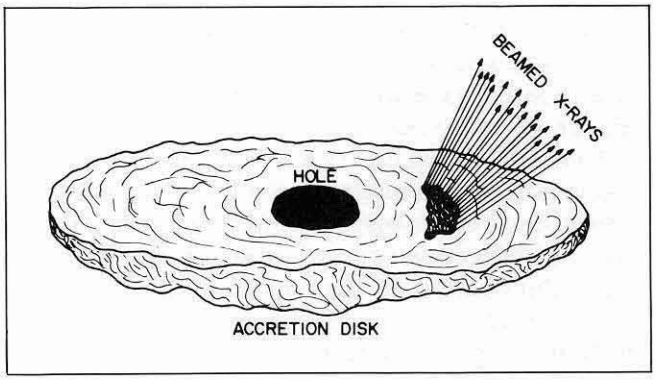
star probably has a black hole in orbit around it.

If we could get up closer (we are roughly 10 thousand light years away), we would probably see something like the artist's conception of this object reproduced above. The big thing in the middle of the picture is a very massive normal star; and down in the center of that swirl in the lower right-hand corner lives a black hole, tiny in size but with nearly as much gravity as the big star. The black hole is close enough to the star that its gravity pulls gas off the star. The gas is swirling down into the neighborhood of the black hole; and as it swirls, it heats up, becoming so hot before it falls into the hole that it emits X rays more intensely than any other kind of radiation—X rays which astronomers on earth can study and try to use to deduce the properties of the black hole.

The theory of this "accretion" of gas onto black holes has been developed by several groups in the Soviet Union, in England, and in the United States. Their theory suggests that centrifugal forces will throw the infalling gas into a thin disk, shown below, something like the rings of Saturn. The swirling gas in the disk may form hot spots, my Russian friend Rashid Sunyaev has suggested; and the X rays that come off of such hot spots would likely be beamed. As a hot spot orbits around and around the black hole, its X-ray beam should precess around and around in the sky above the hole. An earth-orbiting X-ray telescope should receive a burst of X rays each time the beam sweeps past it. If you could discover such bursts and time them in the manner that the radio astronomer times the bursts of radio waves from a pulsar, then you would be studying the orbital characteristics of things in motion around a black hole. Those orbital characteristics would tell you very directly the properties of the strongly curved space near the hole. When I say "near the hole," I should emphasize that roughly 50 percent of the X-ray energy comes from within less than 12 black-hole radii of the hole. So one has real hope of studying Einstein's conception of curved space near this strange object.

The bottleneck in searching for such bursts in Cygnus X-1 is their great rapidity, perhaps 200 per second, and the small X-ray flux seen at the earth—not much more than one photon per 1000 square centimeters per burst. Hints of such bursts show up in rocket-flight data taken by Dr. Richard Rothschild and his group at the Goddard Space Flight Center, but their telescope did not have enough collecting area to give definitive results, and other existing telescopes are even less useful.

Fortunately, the search for such bursts should be



A disk of gas accreting onto a black hole, and a hot spot on the hole that beams its X rays in a manner suggested by Rashid Sunyaev.

revolutionized this year by the HEAO-A X-ray satellite which was launched on August 12. On board the satellite are two banks of X-ray detectors constructed by Dr. Herbert Friedman and his colleagues at the Naval Research Laboratories. One bank of Friedman's detectors will total 2 thousand square centimeters; the other, 12 thousand; and they will have microsecond and millisecond time resolution. They should be able to really pin down the existence or nonexistence of the predicted bursts. And if the bursts are found, careful timing of them may give the definitive test of whether Cygnus X-1 is a black hole, and may help us to understand whether Einstein was right about the curvature of space around black holes. That's something we can expect and hope for over the next year or two or three.

Now let me turn attention to the more distant future —to the challenge of observing the birth of a black hole, of probing deep down inside a collapsing star and watching the curvature of space vibrate as the black hole is being formed. One can't hope to observe such things with light, X rays, or radio waves. There's too much obscuring matter in the surrounding stellar envelope. There are only two ways to look cleanly through the surrounding envelope. The best way, it seems at present, is to use gravitational waves rather than electromagnetic waves. The second possible way is to use neutrinos, which also escape relatively unimpeded from the interior of the star.

A gravitational wave is a ripple in the curvature of spacetime that is ejected from the black hole in its birth throes and then propagates toward the earth with the speed of light. Now the phrase "a ripple in the curvature of spacetime" sounds nice, but it doesn't really mean much to most people. What this ripple of curvature actually does is jiggle neighboring inertial reference frames relative to each other. And since matter initially at rest likes to remain at rest relative to its inertial frame, the wave also jiggles adjacent pieces of matter relative to each other. Just as the jiggling in an electromagnetic wave is transverse to the direction of propagation, so it is also in a gravitational wave. But whereas an electromagnetic wave jiggles only charged particles, a gravitational wave jiggles inertial frames—and thereby jiggles all forms of matter and energy.

Professor Joseph Weber of the University of Maryland has built a pioneering apparatus to search for gravitational waves from the birth throes of black holes and neutron stars. He took a one-ton aluminum bar and glued piezoelectric crystals around its middle. The bar at all times was ringing in its fundamental mode like a bell, due to its finite temperature; and as it rang, it squeezed the piezoelectric crystals in and out, producing electric voltages which when amplified told Weber the amplitude of vibration of his bar. If a strong gravitational wave, propagating roughly perpendicular to the bar, were to hit it, the wave would push the bar's ends first in and then out, driving a change in the bar's oscillation amplitude.

Now, gravitational waves have extremely small cross-sections to interact with matter. So whether the wave came up through the bottom of the earth or down from above made no difference. There was essentially no attenuation in the earth. Any wave coming in from any direction roughly perpendicular to the bar could drive its vibrations.

Professor Weber's piezoelectric crystals were able to measure end-to-end vibrations of the bar with amplitudes of the order of 10^{-14} centimeters. That's a tenth the diameter of the nucleus of an atom. You say how can one possibly ever measure things that are vibrating with a fraction of the diameter of the nucleus of an atom? The answer, of course, is that here one is not measuring a vibration of a single atom; rather, in the bar there are some 10^{29} atoms and 10^{29} atomic nuclei, and they're all doing this vibration at once. Of course, each one individually is doing a lot of other things. But the fact that they all do this particular vibration coherently, and that you have so many of them, enables you to talk meaningfully and with high precision about measuring the total bulk motion of the vibrating bar to a precision of a fraction of the diameter of the nucleus of an atom.

Now, Professor Weber thought at one time that he might be seeing gravitational-wave bursts arriving at the earth several times per day. But subsequent experiments have indicated that probably he was not. This is rather fortunate from the viewpoint of astrophysicists like me, because it was very difficult for us to dream up sources of gravitational waves so strong that they would produce a 10^{-14} centimeter vibration of Weber's bar.

The kinds of sources that we think one should search for are the birth throes of neutron stars and black holes—but not birth throes in our own galaxy, because they probably occur here only once every 30 years; rather birth throes out in more distant galaxies, say at a distance of 100 million light years, at which point you would have a dozen birth throes per year.

Such black-hole and neutron-star births should produce end-to-end vibrations in a Weber-type bar about three thousand times smaller than current detectors can measure. That's down in the neighborhood of 3×10^{-19} centimeters. So the challenge is to monitor the vibrations of that kind of a bar to a precision of 3×10^{-19} centimeters, an improvement of a factor of three thousand over current technology in amplitude, a factor of ten million in energy—and energy is really the more reasonable way to think about it. We need a factor of ten million improvement.

Well, this looks hopeless at first sight. But, thanks in large measure to Professor Vladimir Braginsky of Moscow University, we have before us a number of possibilities for making the required improvements.

The first of these possibilities, suggested by Braginsky six years ago, is to use instead of an amorphous metal bar as the detector, a monocrystal of sapphire or some other material. Professor Weber at Maryland and Professor David Douglass at the University of Rochester are now experimenting with sapphire and silicon crystals, and find them very promising. Weber's, Douglass's and Braginsky's present crystals weigh only one to five kilograms; but they hope ultimately to use crystals of 100 kilograms and perhaps more. Of course, such massive crystals are not found in nature; they are grown from the melt industrially. At present you can go out and buy a 10-kilogram crystal of sapphire, off the shelf, for a few thousand dollars.

The key point about such crystals is that, if you cool them to low temperatures, they have very high "Q's" compared to amorphous metal bars; and the more you cool them, the higher the Q goes. A very high Q means that, if you hit the bar, its ringing will die out only very slowly. And if the bar is sitting there ringing because of its finite temperature, its ringing will be so "pure" that its amplitude will change due to internal forces only very, very slowly.

This means that if a gravitational wave comes by and produces a quick change in the ringing amplitude, you can say that the change was almost certainly not produced by internal frictional or thermal forces, and this means, therefore, that such a change might well have been due to a gravitational wave.

The original Weber bars had Q's of about 100,000, which means they rang for 100,000 cycles before the bar changed its amplitude substantially. For comparison, one of Braginsky's sapphire crystals, cooled to about 4 degrees Kelvin temperature and not very well polished as yet, has achieved a Q of 10^{10}, which is a factor of 100,000 better than amorphous metal bars.

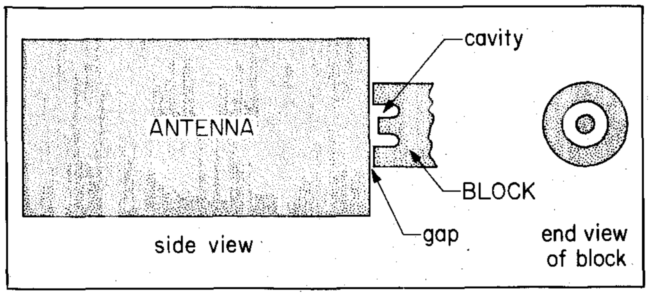
The cooling of gravitational-wave antennas is a second major innovation now under way. It is being pushed primarily by Professor William Fairbank's group at Stanford University and by Professor William Hamilton of Louisiana State University. They plan ultimately to cool massive bars down to millidegree temperatures. At such temperatures, and with improved polishing, sapphire or silicon crystals may well achieve Q's in the range of 10^{12} to 10^{14}. Such a crystal, if hit, would ring strongly for 3 to 300 years before it died out. And such a crystal, in thermal equilibrium, would have only a few thousand quanta of vibration in its fundamental mode—yes, we know that the vibrations of a big crystal must be quantized, just like the energy levels of an atom. And with such a crystal, at millidegree temperatures, you would have to wait roughly one minute for internal thermal forces to add or remove a single quantum! If you can monitor the number of quanta of vibration in the crystal with a time resolution of a minute; and if you see in that time a change by say 10 quanta, you can say that something very strange has happened, that something really hit the bar, and perhaps it was a gravity wave.

So that is the challenge: to take a 100-kilogram bar with a few thousand quanta of vibration in it, to regard it as a quantum-mechanical system—the most massive quantum-mechanical system that people have ever worked with—and to observe quantum changes in its vibrations. If you can do that, then you can study the births of black holes out to very great distances in the universe—out to such distances that you'll have many black-hole births per year. Conceivably, by the end of the century you might "see" all the way out to the edge of the universe. Of course, the question is how do you do it.

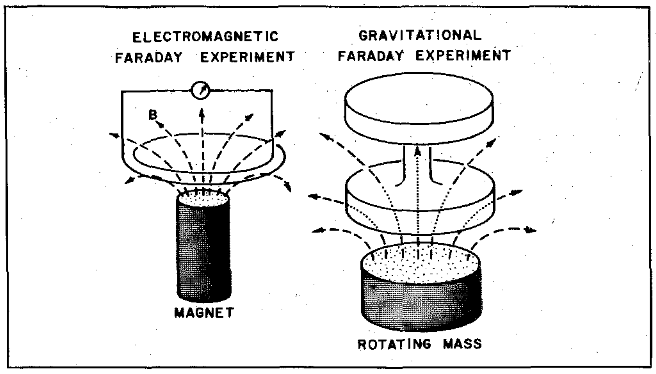
There is a very serious difficulty along the way which will take great effort to overcome; and that effort is likely to "drive" technology, producing important fallout elsewhere. To help in describing this difficulty one of the methods now being developed to measure the vibrations of a bar is shown below. This method was suggested by John Dick at Caltech six years ago, and is now being pursued by Braginsky in Moscow. You have an antenna or bar vibrating away with an amplitude of roughly 10^{-17} centimeters, and you want to measure changes in its amplitude of a few times 10^{-19} centimeters, corresponding to the creation or removal of a few quanta of vibration. At one face of the antenna you have a block of niobium, just barely not touching the bar.

The niobium block has a little reentrant cavity machined out of it, and it is properly polished and surface-treated to make it a good superconductor. The face of the bar is coated with niobium; and the bar, plus the block, forms an enclosed cavity which you drive into electromagnetic oscillation at microwave frequencies. The mechanically vibrating bar and the electromagnetically vibrating cavity form a coupled system. If you can measure the number of quanta of electromagnetic excitation in the cavity, then you can infer from it the number of quanta of mechanical oscillation of the bar. That's very helpful, because with modern technology it is much easier to measure electromagnetic oscillations than mechanical oscillations.

But now comes the difficulty—a difficulty first pointed out by Braginsky four years ago, but much clarified by Robin Giffard at Stanford last year. With any kind of sensor that one has nowadays for looking at microwave vibrations in a cavity, the best one can do is to measure the number, N, of quanta of excitation to a precision of the square root of N. If the cavity is excited with a million quanta, one can measure it to a precision of at best ±1000 quanta. That's not good enough. If that's the best you can do, then there is only very marginal hope of seeing gravity waves from black-hole births at a distance of 100 million light years, which is how far you have to look in order to see one black hole born say every month. You might be able to see that far, but you'll have to be very lucky. And there is no hope at all of seeing farther.



The use of a microwave cavity to measure the vibrations of a gravitational-wave antenna.



Left: Faraday's 1831 electromagnetic induction experiment. Right: a possible gravitational induction experiment by which one might hope to detect magnetic-type gravitational fields.

Braginsky has given the name "quantum-non-demolition sensor" to any device that can measure the number N of quanta in an oscillator more accurately than $\sqrt{N}$. This is because the key to the failure of all standard sensors is that they disturb the oscillator—that is, they demolish the quantum state in which it resides—in the process of making their measurements. The problem, then, is to devise a quantum-non-demolition sensor. And if one can do so, and build it, one may be able to use it as a foundation for innovations elsewhere in technology. For example, such a non-demolition sensor might become the key element in a new generation of amplifiers, with far lower noise temperatures than the best amplifiers that exist today.

Quantum-non-demolition sensors can surely exist in principle. Theorists have no trouble inventing idealized ones. But to invent one that really works in practice is something else. Recently Braginsky in Moscow and Bill Unruh at the University of British Columbia have invented promising devices; but it will take a long time—perhaps five years—to construct working models. Meanwhile, there is a search for better, simpler designs.

Even without quantum-non-demolition sensors, one can hope to do some wonderful gravitation experiments with high-Q sapphire or silicon crystals. The figure above shows an example that Braginsky in Moscow and Carlton Caves and I at Caltech have been thinking about together.

continued on page 30

The Rolling Stones

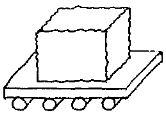
by JOHN D. BUSH, '55

I once had to move a 16-ton block of granite by tumbling it end over end, and it occurred to me that, if I tied segment-shaped pieces of wood on four of the block's faces, it would roll like a drum. Since the idea seemed simple enough, I wondered if someone else might not have thought of it first. I looked up a few books on the pyramids, and sure enough, the Egyptians had the perfect device for the job—something known as a "cradle."

According to S. Clarke and R. Englebach in *Ancient Egyptian Masonry*, numerous models of cradles have been found. But, as far as I can determine, no one in recent times has suggested using four of them for moving stones with a parbuckle. A parbuckle is a sling for rolling cylindrical objects up or down an inclined plane. It consists of a rope looped over a post or the like, with its two ends passing around the object being moved.

I tried making a model cylinder using a 20-pound granite cobblestone. The stone is not a perfect rectangle so my cylinder is more oval than round. Nevertheless, it has so little friction that it will roll by itself down a 1°-slope. That corresponds to a coefficient of friction of less than 2 percent.

There are three reasons why it is vastly easier to hoist a drum by parbuckling than by hauling it on a sledge with rollers. First, rolling friction is inversely proportional to the diameter of the roller. As far as we know, the Egyptians' rollers were small—about three inches in diameter according to Clarke and Englebach. But a pyramid-block cylinder would be about 16 times that diameter. Therefore, the rolling friction would be cut to one-sixteenth.



Second, the friction of a sledge is two times worse because it has double friction: The sledge rolls on the rollers at the same time as the rollers roll on the ground.

Finally, with sledges, the haulers must not only raise the load but also their own body weights as they march ahead of the vehicle. This could easily cut their usable output in half. With parbuckles, however, the men can haul on the level as the stone rolls up the ramp. If we multiply these three factors together, we find that the parbuckle-cradle hoist could be as much as 64 times more efficient than a sledge.

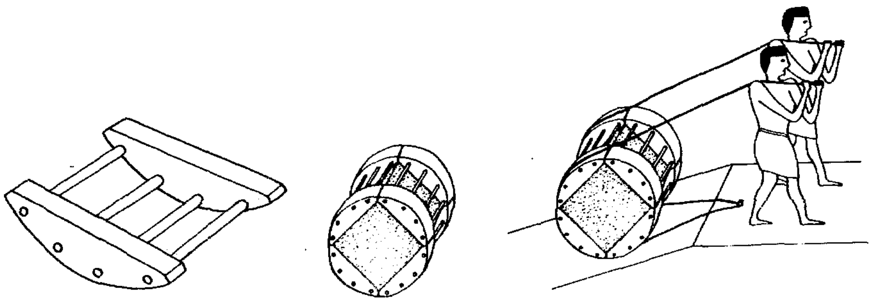
We can also calculate the *theoretical* minimum number of haulers needed to build the Great Pyramid. A man's output for an eight-hour day is about one-tenth horsepower, or 55 foot-pounds per second. If he puts in a six-day week, the man's output totals 0.5 billion foot-pounds per year.

Next, multiply the weight of each course of stones by its height above the base, add up these products, and you get a grand total of 1.8 trillion foot-pounds. That's the total potential energy, due to gravity, of all the blocks in the pyramid. Dividing 1.8 trillion by 0.5 billion, you find you need 3600 man-years of hauling.

Historians are generally agreed that the Great Pyramid was built in about 20 years. Therefore, it seems the Pharaoh would need 3600 ÷ 20, or 180 haulers. But that figure would apply only if the men worked continuously at 100 percent efficiency. The mechanical efficiency of the parbuckle-cradle hoist could easily be 50 percent; and if the men spent half their time walking back for the next block, the overall efficiency would be 25 percent. Therefore, the Pharaoh needed four times as many haulers as the theoretical minimum, or 720.

The conventional method of building a pyramid with sledges would require hordes of slaves—perhaps as many as 100,000. There's considerable doubt whether that much manpower was available in ancient Egypt. But a fraction of 100,000 could have done the job with parbuckles and cradles.

Surprisingly, the archeological evidence supporting the sledge theory is meager, and even proponents of the theory like Clarke and Englebach have acknowledged that the evidence is comparatively slight. Only a few sledges have been found, and they were all much too big to haul pyramid blocks. It is generally agreed that the Egyptians were efficient organizers of manpower, and since they built up columns of drums of stone, they must have discovered how easily drums rolled. Therefore, using cradles to roll blocks would have been a simple, logical extension of rolling drums. We may never know for sure just what they used, but I'd bet on the "rolling stones." □



dles around a block . . . make a cylinder . . . which can be parbuckled.

Bucharest '77 — Richter 7.2

by FRANK LAMSON-SCRIBNER, '46

The night is beautiful. The moon through thin cloud cover is soft. Friday, the streets are full, the people festive, as I walk back to the hotel.

In my room I slip into my pajamas and settle down to read my new John Jakes paperback, *The Furies*. Much better if I finish it so I can leave it with some friend. English books are valued and in short supply in Romania. Occasionally my mind drifts; just a few days and I will be on my way home—to finish fixing up the boat, to repair the ice-ravaged pier, and then to enjoy spring sailing. I am not sure of the time—about 9:30 p.m.

Suddenly, a thundering roar. Perhaps a jet breaking the sound barrier. My mind is not prepared to think of earthquakes. If it were California or Tokyo, yes—but not Bucharest. Now the room rocks and sways violently, but mostly bounces up and down as if I were sitting on a rapid pile driver. Oh, God, is the building collapsing floor by floor? The thought of 19 floors above me is chilling. I stumble to the window—panic, a throng of screaming pedestrians, a deafening roar as a building across the street collapses in a cloud of dust, all illuminated by arcing from the wires of overhead trolleys.

Get out—get out. Pants over pajamas, shoes without socks—I can't find anything. I suddenly realize the lights are out as the floor still heaves and sways almost like a boat in choppy water. Grabbing my car coat, I stumble into the hall, making my way to the emergency exit. There I find perhaps six people, one leading with a lighter that he works like a strobe. Behind me a man with a candle catches up. I pass him to the front of our little parade down a circular staircase covered with plaster debris.

Down, down, we go. Where to get out, nobody knows—the doors all seem to be "one way." Finally, in the second or third subbasement we find an incoherent Romanian young lady, and I coax her to lead us back up. It seems to take forever. Finally, we are in the restaurant, the lobby—hurrah—we can really get out before there is another tremor.

Confusion builds, people mill about. I yell, "Get out into the open square." Even a few of the hotel people take my advice.

The first casualty I see is a lady hotel employee, lying on a sofa. After conscripting three or four others, we carry her out, sofa and all. But first we have to find the key to the large (not revolving) door. What thoughts, or lack of thoughts, people have. Why hadn't it been unlocked before? Safely away from the hotel, we put the lady down. Later, doctors determine she probably has two broken legs and a broken back.

Cars are racing along the boulevards, each without doubt trying to get home. I meet a young German businessman who has also been helping. We decide to take a walk. I surely have no intention of going back to the hotel very soon.

We walk in the middle of the street, if possible, so as not to be hit by falling debris. I finally remember to tie my shoes. Several buildings close by have completely collapsed. Tragedy and hysterics abound. A young woman, with one or two others around her, lies huddled. We try to help. Is she injured? Physically, no, but across the narrow street is the building where her father, mother, and baby were. There is no hope.

As we walk, I am seized by a young Romanian girl—21, we find out later—in a fluffy fur jacket and roundy hat. Beautiful—a veritable image of Lara in *Dr. Zhivago*. "Where is the Intercontinental Hotel? My parents are there." We take her there as she sobs. Our communication is in an Esperanto of Romanian, French, German, and English, one sentence in one, the next phrase in another. Misunderstanding her story, we think her parents work in the hotel, but it turns out they were just to have dinner there. The doorman will not let her in—only hotel guests are allowed. She dissolves in tears.

Though my German friend and I are almost comrades in arms by now, we introduce ourselves. "I'm Earnest," he says. "I'm Frank." The young girl breaks out laughing, only then realizing that all of us are strangers. "I'm Cory, really Cornelia," she says. We are now fast friends. Earnest and I convince Cory that she must return home, that her father is probably there and worried about her. No taxis, so we walk. Along the way we pass the Academy of Economic Science, an old building whose dome is crowned with a bronze casting, on the top of which is a sphere—too high to tell if it's the world or what. Local legend has it that if any female student who is still a virgin walks through the Academy's door, the "ball" will fall. I break into laughter—the ball remains. If an earthquake can't unseat the ball, what power is virginity?

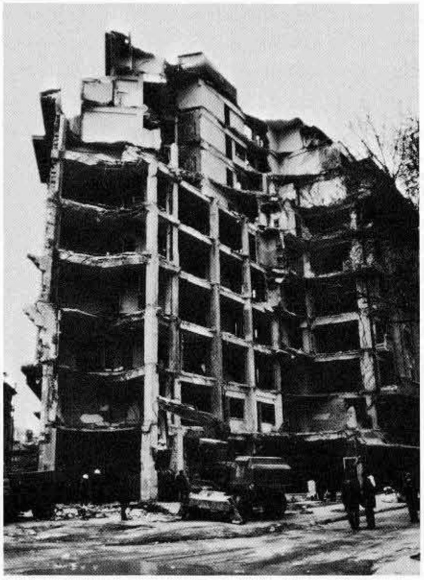
Somehow, Cory attaches herself to me—a father substitute, no doubt. We walk hand in hand, with her occasionally taking Earnest's arm, the three miles to her house. She says her father is a Romanian diplomat. She has lived in Tokyo, works at some unknown place in the daytime, and takes University courses at night. If I were 20 years younger and she weren't so worried about her family, I definitely would suggest that Earnest get lost.

We reach her house, but her father is not there. Exacting a promise that she will stay there, Earnest and I hitch-hike back to the center, but several blocks from our hotel, debris prevents our getting through.

Without electricity, we depend on the moon and the marvelous accommodation of the human eye. It is now perhaps three hours since the quake. Volunteers and a few police direct traffic around those streets that are impassable with the rubble of demolished buildings, some of which have lost their fronts so all the rooms are exposed as in a doll house. Others are severely damaged and are being evacuated. At last the word has been posted to get out of the buildings in case of another quake. Families huddle in blankets, sitting on park benches in the cold.

The "refugees" now start appearing among their belongings on the sidewalks and in the streets. A little lady, shriveled and old, perhaps 4′10″ and 80-90 pounds, sits on her pile—a carpet, folded up, for a chair, piles of clothes, and a radio. I almost cry with the thought of her starting over at what must be the age of 80 or more. But at the next pile I almost split with laughter. Against a TV is a beautifully framed picture, 2′ by 3′, obviously a point of pride to its owner. In all truth, peering at me, illuminated by the moon, is that serene enigma of all time—*Mona Lisa.*

On the outside, most of the older, heavier buildings, such as the University, appear untouched, but all evening I have worried about the multitude of newly built apartments on the outskirts of the city; I find out the next day that none of these collapsed, luckily. In Romania there has been a housing shortage since the War. Families live in one-bedroom apartments; parents in the bedroom and children in the living room, or vice versa. When the young get married, the vast majority merely move in with one set of parents or the other. Considering this, the population per apartment building is high, and the thought of the number of casualties in the collapsed ones turns one's stomach.


A building in Bucharest, its front sliced away, still has perhaps a six-foot width of rooms and hallways.

Finally, I am exhausted. I start back to the hotel. On the way, I marvel that at least downtown I have seen no fires. How very lucky, considering the gas lines in the streets and that most people cook with gas.*

One thought that has concerned me the whole time is that my family may be worried about me. I am all right, but, considering time differences, by the 7 or 11 o'clock news, Caltech seismologists will tell the world there has been a major earthquake in Romania. So, I make my first call on the American Embassy, though I have been in Romania eight times totaling almost seven months. Communications have been stopped for the night, but I leave my name and U.S. address. The Embassy is full of its staff (and families). I surmise some have lost their homes and are trying to fly out quickly since an airline man is checking flights in the airline guide. A Pan Am pilot stands quietly in a corner, saying that he will fly in the morning if the runway allows it.

I return to the hotel, and curl up in a corner of the lobby, hoping to sleep, but where I can run outside if there is another quake. The lobby is full of ghost-like blanket-covered figures with similar thoughts. But it is noisy and cold. The wind has picked up and blows through broken windows. Someone's radio is turned loud with the "news," but I understand only a little Romanian, and there isn't much news anyway—just a proclamation, point 1, point 2, and so on, to conserve electricity, don't drink water, and the like.

With a short prayer, and a fatalistic approach, I decide I would rather be dead in bed than die of fatigue and exposure. I return to my room by candlelight and drop on the bed, exhausted, but still tense and nervous at every sound or vibration.

Saturday's dawn comes. After fitful sleep, I decide to get up. No water or electricity. I get dressed and go to the Government office that is my contact, arriving just as my good friend, a high official, does. Thankfully, he, his family, and his house escaped serious harm. He has no news either.

Back to the hotel. Electrical power is back on; brown water runs from the tap. Rather than rest, I watch the first

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*The lack of fires, which I attributed to luck, was not luck at all. I was told that both electricity and gas had been turned off before the tremor stopped—certainly the electricity had. I marvel at the discipline. Though many store windows are gone and goods are there for the picking, not once have I seen any looting.

organized efforts on the collapsed building across the street. By 9 a.m., less than 12 hours since the quake, a number of dump trucks have appeared (and hamper traffic). They start to work, with little system, and progress is slow.

It is easy but saddening to tell when those who perished are found in the debris. First, someone will see the body, and halt the earthmoving equipment. Then a small group, four to six, sometimes soldiers and sometimes workers, climb up the pile of debris. With care, bricks, concrete, or whatever is picked away. Usually there are sheets or blankets among the debris, and the body is wrapped and carried down the "mountain." The ambulance is loaded, pulls away quietly, without flashing light or siren. There are many trips.

Man's obsession for possessions is also evident. A building, its front half sliced away, still has left its rear walls, the hallway of the apartments, and perhaps a six-foot width of rooms, the whole face exposed. Several occupants return, creeping through the back halls to collect light furniture, clothes, pictures, TV's, and stereos, despite the danger. In the normal houses, which are extensively damaged but without the crushing weight of tons of concrete, groups of volunteers help owners move out everything from the smallest object to wardrobes six men must carry.

I walk the streets. The day is pretty—blue sky with cottonball clouds—but a strong wind chills. Lunchtime. (Where was breakfast?) I go to the University Club and find it scarcely operating. I manage to get the last piece of meat and a bit of last night's bread. No beer, so I order a kilo (about 1½ bottles) of wine. Perhaps the afternoon/evening will be more pleasant through a haze. One of my closest friends, a professor, and his daughter come in. They are too late for lunch, but we all have large jars of yogurt (18 oz.), and he helps me a bit with the wine. And now I get the first news. The earthquake did not just hit Bucharest, though it is the most heavily damaged. The area badly hurt was vast, ranging southward from the Carpathian Mountains.

Saturday morning there are lines to get bread and food. Radio announcements are explicit in what arrangements are being made. By afternoon in areas I walk in, the food situation appears close to normal. The fresh vegetable and fruit market operates as well as before, with good supplies of produce. The flower market next to it is the scene of tragedy as floral arrangements for funerals are piled into cars.

Saturday evening, the cleanup work now proceeds in earnest. Systems have been worked out. Lights are rigged to allow work through the night.*

Sunday morning. I am rather dirty, having not yet ventured a cold, brown shower. Don't know if I would be ahead or behind if I did. My hair, thick with blown dust, looks like the "before" of a shampoo commercial. My legs are so stiff from walking that I have adopted the gait of an 85-year-old man. As I start to write this, I am thankful to be able to do so. All the walls of my room are cracked from one end to the other. The most severely cracked is next to my bed. Only the vinyl wall covering holds it together. A 15-20 pound piece of plaster has hit the bed next to where I was at the time of the quake. Whether this happened while I was still in bed, I'll never know. With just a little difference, I might have had a whole wall as a bedmate.

After writing this, I venture down for a walk, stumbling on stiffened legs. The lobby is now almost more confused than during the quake. There has been an invasion—the press has come, NBC, CBS, TV equipment, Italian paparazzi with cameras. Though many of those in the hotel when the quake occurred raced away Saturday morning, I think we have filled again—a few people from damaged hotels, diplomats, "western" families who are homeless, and now this horde of newsgatherers. I ask some of them for news, but the only thing that seems to be reliable is that the quake measured 7.2. My ignorance is shared by all.

Looking outside—what a change. Yesterday, Saturday, the open areas were filled with Romanians watching the clearance operation; today the area is clear. Remarkable, since it's Sunday, the day most families walk. I go outside and find out why. Soldiers, reserves, and other organization young men are prohibiting sidewalk superintending. "Circulante. Circulante." Keep moving. Keep moving. I think the whole of Bucharest is being jostled along at 5 km/hr. Much of the center of the city has been barricaded, and most cars have been prohibited. Work now proceeds without danger to onlookers or interference to salvage equipment.

Between barricades and pedestrian lines, going outside the hotel is virtually impossible. I am a prisoner. And the prison isn't in the best condition. Without gas to cook with, the menu is shorter than a pizza parlor that only serves pizza. But, so long as the beer holds out, I guess we are OK.

Sunday afternoon, still hemmed in, with fear and trepidation I decide to finish *The Furies,* hoping that this does not loose another tremor. I debate taking the book outside, but there is no place to sit. With courage, in my room, I open it and start to read. No tremors. I finish the book and put it aside. But I can't put aside thoughts of Bucharest '77, Richter 7.2. I'll carry those forever. ☐

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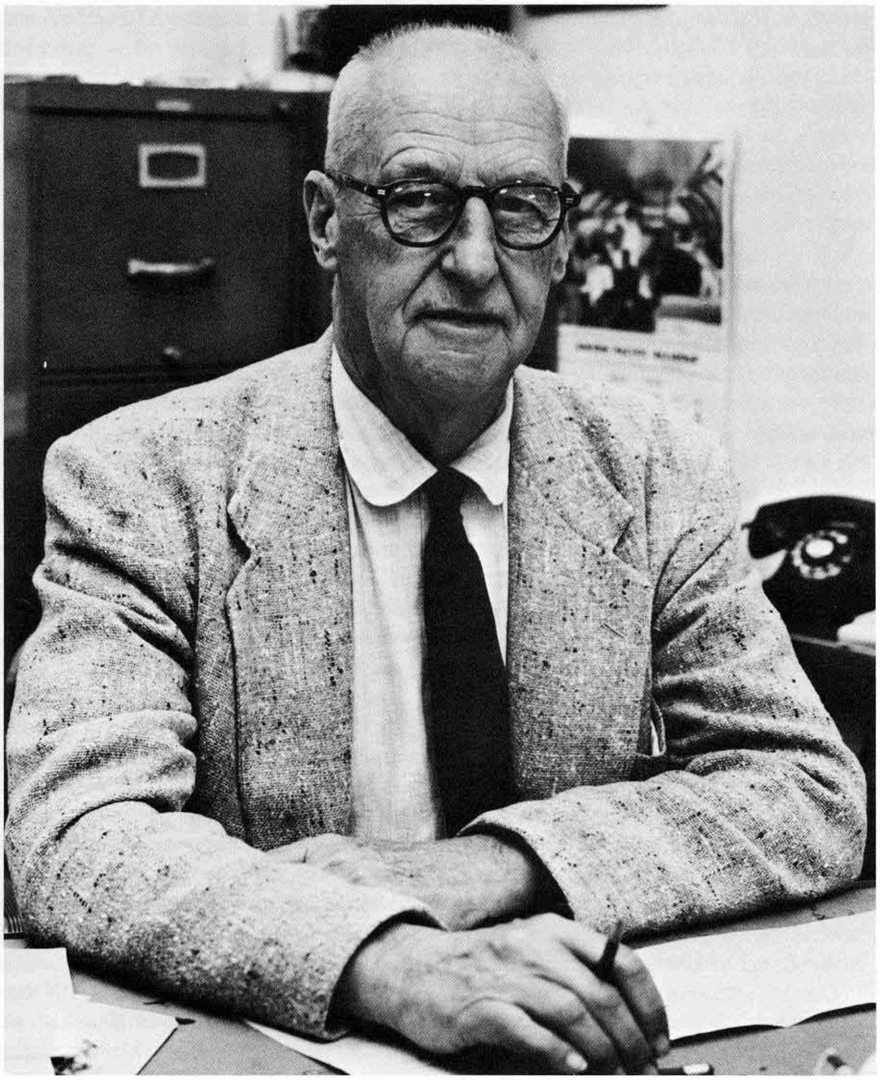
*The mobilization of effort in the 12-24 hours following the quake was fantastic. In my opinion, this was done much faster than it would have been done in most cities of 2 million people in the United States or most other countries.



In Memoriam

William W. Michael

1885-1977

A Tribute by Paul C. Jennings

William W. Michael was born on July 13, 1885, in Palatine Bridge, New York. He received the BS in civil engineering from Tufts College in 1909 and was then employed for nine years by the J. G. White Engineering Corporation, where he worked on numerous hydroelectric construction projects. Joining the civil engineering staff of the California Institute of Technology in 1918, when it was still Throop Polytechnic Institute, he served on the faculty for 38 years and retired as professor emeritus in 1956. He died on July 2, 1977, just 11 days short of his 92nd birthday.

Professor Michael was a life member of the American Society of Civil Engineers, and a member of the American Road Builders Association, Sigma Xi, and Theta Delta Chi. He was also a Rotarian and a Mason, and he served as president of the board of trustees of the Throop Memorial Church for several years.

At Caltech he taught many of the undergraduate courses in civil engineering, teaching several thousand students during his career. He is remembered as a patient and gifted teacher with a fine sense of humor, and a student adviser par excellence. His particular specialty was precise surveying, and his expertise in this area led to frequent requests to serve as consultant on engineering projects. He was consultant to the Lands Division of the Department of Justice in the handling of cases involving the relocation of highways that were closed to accommodate federal facilities. He was associated with many of the mapping projects and surveys made in the early development of southern California, including the Palos Verdes area, and he made the topographic surveys of Palomar Mountain required for the construction of the 200-inch telescope at Palomar Observatory. He also served as consultant to the Los Angeles County Flood Control District.

To many members of the Caltech community Bill Michael was best known for his skill as a fisherman. At the age of 10 years he was coached by a favorite uncle in fishing in the Catskills, and this led to a lifelong study of fishing from the scientific point of view. He wrote an authoritative and popular book, *Dry-Fly Trout Fishing,* published by McGraw-Hill Book Company in 1956. He also wrote articles on fishing for such magazines as *Outdoor Life, Colliers,* and *Hunting and Fishing.* He was recognized as one of the top trout fly-fishermen in America.

His favorite waters were in Idaho, where he went fishing every summer or fall until he was well into his 80's. He had a fund of fishing stories, many from the days before southern California was so populated: of driving to Hot Creek near Mammoth when the road from Mojave was unpaved; of steelhead trout running in the San Gabriel River; of catching brown trout in Bouquet Canyon; and of many successful trips for brown and rainbow trout to the San Gabriel River before the great flood of 1938.

His greatest fishing tragedy was the failure to land the largest trout he ever hooked. He describes the incident in his book. The brown trout, a monster nearly a yard long, was lost because of a bungling attempt at netting the fish by an inexperienced fishing companion. It is characteristic of Michael that he did not identify the angler who failed him at such a crucial time. In later years he would identify the stream capable of growing such a giant fish, but the unfortunate net handler was never named. At the age of 90, Michael's advice to his young friends was, "Get in all the fishing you can, while you can."

Professor Michael is survived by a son, William D. Michael, who is professor of psychology at the University of Southern California.

Bill Michael led a full and active life, pursuing, with excellence, both his vocation and his avocation. He will be missed and remembered by those who had the privilege of knowing him.

Paul Jennings is professor of applied mechanics and civil engineering and executive officer for both of these options at Caltech. He is also, like Michael, a devotee of fly fishing.

Don M. Yost

1893 - 1977

A Tribute by Terry Cole

With the passing of Don Yost, professor emeritus of inorganic chemistry, on March 27, the Institute lost one of the few remaining links with its beginnings. Don served Caltech, chemistry, his country, and the cause of scholarship for over 50 years. He is survived by his widow, Marguerite; children, Max Caley Yost and Helen Marguerite Yost; and two foster children, William Neal Yost and Bettie Yost Long. As Don's last graduate student, I am honored to commemorate his career.

It has always seemed to me that Don's pioneer youth had a profound influence on his character and unique approach to science. He was born in the village of Tedrow in northwestern Ohio. By 1899 economic conditions forced his father to give up farming there and move, first to the lumbering camps of northern Wisconsin, and finally, in 1902, to a ranch in the Boise Basin of southwestern Idaho.

Don's often-interrupted education continued at a frontier school near the ranch. Its enrollment consisted of about ten children and a half dozen wintering cowboys. He once remarked that the lessons were far from memorable, but the exhibitions of fancy horsemanship by the cowboys at noon recess were always exciting. During high school Don acquired his enduring fascination with mathematics and languages so familiar to later generations of his students. Although no science courses were offered in those days, he taught himself enough electrical theory to build a crystal radio set using galena crystals he found in the surrounding mountains.

In the summer of 1914, his acceptance in hand and the $10 out-of-state tuition paid, Don arrived, via rail and steamship, in San Francisco to begin his college education at UC Berkeley. His freshman year was decisive; by the summer recess he had found his calling through the inspired teaching of his chemistry professor, Joel Hildebrand, and a young lab instructor, Richard Tolman.

During his second year Don met, and in the following year married, Susan Marguerite Sims, later affectionately known to his students as Mamacita. A month after their marriage the United States entered World War I, and Don enlisted in the Navy, where he served for three years. He graduated from Berkeley in 1923.

At the urging of Professor Walter Bonner of the University of Utah, where Don spent his first year as a graduate student, he applied for graduate work with Arthur A. Noyes at the fledgling Institute. His career at Caltech was brilliant and wide ranging. Upon receiving his PhD (magna cum laude) in 1926, he was appointed instructor in inorganic chemistry and began the application of the most modern physicochemical techniques to the elucidation of the chemistry of the rarer elements. A Rockefeller Fellowship in 1928 took him to study X rays with Manne Siegbahn at Uppsala and the newly discovered Raman effect with Peter Pringsheim, at the University of Berlin. Upon his return he began pioneering applications of Raman spectroscopy to the determination of molecular structure and the thermodynamic properties of inorganic halides. His work on the volatile fluorides brought him international recognition.

In collaboration with Louis Ridenour and Edwin McMillan he helped to found the chemistry of artificially radioactive elements. During the 1930s Don published over 50 papers contributing to chemical kinetics, gas equilibria, the chemical effects of X rays, electrochemistry, the chemistry of the platinum metals, low-temperature thermodynamics, and rare-earth chemistry. His achievements during this time are the more outstanding when viewed in historical context. In those years there were no high-technology instrument manufacturers; any apparatus more complex than a galvanometer or simple glassware had to be built or improvised as the research went along.

Soon after the formation of the National Defense Research Committee, Don was sought out to direct war research. He was appointed Section Chairman under the OSRD, directing research teams at Caltech, Northwestern, and Los Alamos. His achievements in this capacity were to bring him the Presidential Certificate of Merit.

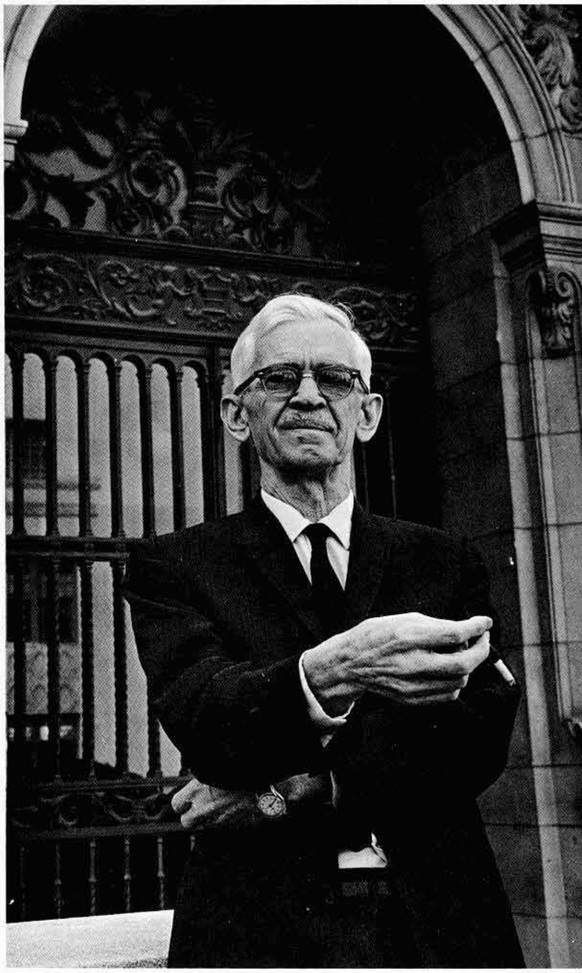
Toward the close of the war he was struck by a series of serious illnesses that robbed him of much of the physical vigor remembered by his early collaborators. Despite these handicaps he continued active participation in research and as he used to say, "the care and feeding of scientists of imagination." His two books, *Systematic Inorganic Chemistry* and *The Rare Earth Elements and Their Compounds,* were written during this period. Don recognized that the great strides in microwaves and radio techniques made during the war could have a profound impact on physical chemistry, and in the succeeding decade he led a small band of us to saddle up and explore the virgin territory of radio and microwave spectroscopy.

Even after his active participation in research declined in later years, his interest in scholarly matters continued and was expressed through correspondence with colleagues, students, and members of the Iron Nail Club—La Sociedad des Clavos Hierros Cuadrillados, an intellectual and philosophical corresponding society founded by Don (Cisco) and Pancho P. Gomez of Idaho City, Idaho, dedicated to the free though intermittent discourse on politics, art, science, and humor; listing (by noms de plume only) many of the great and near great of American science and free enterprise. He also wrote on mathematics, the historical aspects of science, and—most memorably—book reviews.

Don's book reviews, published in the *Journal of the American Chemical Society* and *Nuclear Science and Engineering,* have become minor classics of their genre, filled with his perception, erudition, and wit. As a brief exemplar of his style, he began the review of the volume, *Applications of Nuclear Physics*: "There was a time when those of us born west of Dodge City pictured England as a pleasant, provincial island where the men raced around the countryside in Rolls Royces chasing small foxes, where the women rode through the streets on horseback protesting oppressive taxes, and where millions of innocent children were brought up on Latin, *Alice in Wonderland,* W. Shakespeare, and on the exploits of the privateer Sir Francis Drake. But this picture is, in part, now notably different, the change really having been initiated by a transplanted New Zealander (Rutherford) and a visiting Dane (Bohr)."

Characteristically independent, he was always a staunch defender of individual independence against the strictures of official policy. His normally gentle wit became a rapier when deflating administrative pomposity or bureaucratic presumption. One of his former students has called him "the foremost anti-stuffed-shirt in American science."

Don prized and encouraged originality and independence in his students. He expected them to take the initiative in getting the work done; yet when genuine problems arose, he was always generous with his time in discussion and in sharing his vast scientific experience. Caltech can be a rather intimidating place for a new graduate student realizing how many scientific giants inhabit this small campus and how much he has to learn. Don's courtesy, informality, unfailing good humor, and grace in instruction toward this former student are memories I shall always treasure.

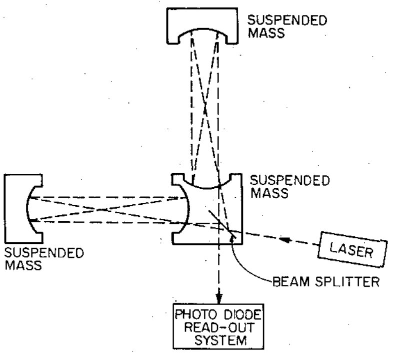


Terry Cole, PhD '58, who is a senior staff scientist at the Ford Motor Company's Research Staff, spent most of the last academic year back at Caltech —this time as a Sherman Fairchild Distinguished Scholar.

As background for this experiment, I must tell you that, according to Einstein, gravity must have associated with itself "magnetic-type" forces as well as "electric-type" forces. All past gravity experiments have measured only electric-type forces—forces that are independent of the velocity of one's apparatus. A challenge for the near future is to detect magnetic-type gravity, gravitational forces that depend linearly on the velocity of one's apparatus. In a sense, gravitation research today is where electromagnetic research was at the beginning of the 19th century.

One way to make gravity catch up with electromagnetism would be to perform a gravitational analog of Michael Faraday's famous induction experiment. In his 1831 experiment Faraday moved a magnet up and down near a coil of wire. As the magnetic flux linking the coil changed, it induced an electromotive force (EMF) around the wire, causing electrical current to flow and to be measured by the device at the top of the picture.

Over on the right-hand side of the picture we see the gravitational analog of Faraday's experiment. It is an experiment that might be done five years or so from now. The rapidly rotating mass produces a magnetic-type gravitational field—something nobody has ever seen before, but something that might be useful for technology in the distant future. Above the rotating mass is a sapphire crystal that has been machined into a dumbbell shape so that its period of torsional vibration is one one-hundredth of a second. The rotating mass is moved up and down one hundred times per second; and as it moves, there is an oscillation of the flux of its magnetic-type gravitational field which threads the lower half of the dumbbell. The changing flux induces a gravitational "EMF" in the crystal. In other words, it induces an oscillating, circular gravitational force in the bottom part of the crystal; and that force, oscillating away for about one week, drives an amplitude change of perhaps 10^{-18} centimeters in the crystal's torsional oscillations.



A multipass Michelson interferometer for use in detecting gravitational waves.

One can hope to measure such a change with standard sensors. A quantum-non-demolition device is unnecessary if the experiment is carefully designed. And having made such a measurement, one could not only say unequivocally that magnetic-type gravity exists; one could also determine whether Einstein's general relativity correctly predicts the amount of magnetic-type gravity produced by the rotating mass.

Let me return now to gravitational radiation, and describe for you two other detection techniques that are currently under development. These make use of the fact that the larger the detector is, the larger will be the signal produced by a gravitational wave, and the less need there will be for a quantum-non-demolition sensor.

One technique, being developed by Ray Weiss at MIT, Ronald Drever in Glasgow, and H. Billing in Munich, makes use of a "multipass Michelson interferometer." Four mirrors are suspended by pendula below an overhead support, to form two arms of an interferometer, as shown in the picture above. (In practice one would probably use eight mirrors and four arms.) The laser beam is split in two; and the two beams are bounced back and forth between the mirrors of the two arms. After roughly 1000 bounces—with each bounce making a distinct and separate spot on a mirror—the beams are recombined and examined for interference.

The swinging frequencies of the pendula are far below the frequencies of the searched-for gravitational waves; so waves hitting the device drive the mirrors back and forth as though they were "free" masses. Moreover, because gravitational waves have spin 2 (according to Einstein), they will drive the mirrors of one arm toward each other while driving the mirrors of the other arm apart. The resulting oscillations of the arm lengths will produce oscillations of the interference pattern of the combined beams.

This device has the advantage that, because of the 1000 bounces of the laser beam, its effective length is 1000 times the length of the arms. Nevertheless, to detect waves from stellar collapses 100 million light years away, one will need arm lengths of several kilometers or more; and one will need enormous isolation from seismic vibrations. It is not at all clear whether such size and isolation can be achieved on earth. One might have to deploy the device in space. More modest prototype devices with arm lengths of several meters are now under construction and should operate successfully on earth with sensitivities better than current Weber-type bars.

I turn now to gravitational-wave detectors and gravitational-wave sources with sizes far larger than the ones described above. Our photograph of Andromeda on page 17 illustrates the fact that most galaxies of stars are very quiescent systems, beautifully calm and quiet. However, occasionally one finds a galaxy such as M82 (right) in which gigantic explosions are occurring in the nucleus. It seems likely that those explosions are either generated by huge black holes, or produce huge black holes as by-products. By "huge" I mean black holes weighing a million to a billion times the mass of the sun.

The challenge for astronomers is to measure the gravitational waves produced by the birth of such a gigantic black hole as that. A way in which to do this—a method that is under active investigation by Hugo Wahlquist, Frank Estabrook, and others at JPL—is by means of spacecraft tracking. One sends out highly monochromatic radio waves from the Goldstone tracking antenna; one receives them at a spacecraft in deep interplanetary space; the spacecraft retransmits them back to earth; and the tracking antenna receives them and measures their net Doppler shift, their change in frequency. From that Doppler shift one infers the velocity of the spacecraft relative to the earth.

Now, when a burst of gravity waves passes through the solar system, it induces very tiny motions of the spacecraft and the earth relative to each other. If you can measure those motions, using the Doppler tracking data from the earth-spacecraft link, then you can learn from them the details of the gravitational wave, and try to infer information about the birth of a huge black hole in the nucleus of a very, very distant galaxy.

What does this require? It requires making measurements of the velocity of the spacecraft to a precision of something like one part in 10^{16} of the velocity of light, which means you need clocks on the earth that are stable to about one part in 10^{16} over times of the order of minutes to hours. And in fact, those kinds of clocks are on the way. We are accustomed to thinking of atomic clocks, particularly the hydrogen maser, as being the best clocks around. But the record for the best clock is not held by the hydrogen maser any longer; it's held by a "classical" clock—a "super-conducting cavity stabilized oscillator," which is nothing but the same kind of little microwave cavity I was talking about before, in connection with detection of the gravitational waves from the death of a normal star. The ticking mechanism of such a clock is the microwave oscillations in its cavity.

The galaxy M82, photographed by Alan Sandage with the 200-inch telescope.

Professor John Turneaure at Stanford has built such a clock and has achieved a stability of 6 parts in 10^{16}, which is four times better than the best hydrogen maser. And Turneaure expects soon to achieve one part in 10^{16} or better, which is what is required for our spacecraft tracking project. With further improvements on the way in Moscow and elsewhere, we can hope for one part in 10^{17} in five years or so. And if other parts of the Doppler tracking system can be cleaned up, which is one thing JPL is currently looking at, then we can have real hope in 10 to 15 years of seeing gravity waves from the births of gigantic holes out at the very edge of the universe, waves emitted back when galaxies were very young and their explosions with black-hole births were perhaps most frequent.

Not all of the future efforts and progress in cosmology and relativistic astrophysics will come from experimental work. Theoreticians can hope to make some contributions too. Let me fire up your imagination with the following example:

When we talk about the birth of the universe in a big-bang explosion some 12 billion years ago, the normal man in the street always wants to know what caused the explosion; where did it come from. And up until now we physicists have had to say, not only do we fail to know the answer; we don't even know how to ask the question in such a form that there is any hope of learning the answer. However, within the last three years hints about how to ask the question have come from the work of Dr. Stephen Hawking in Britain, and also of Leonard Parker and James Hartle in the United States and of Yakov Borisovich Zel'dovich in the Soviet Union. Thanks to them and others, we can begin to hope to understand the birth of the universe.

It appears that very strong gravitational fields—such as those that occur in the centers of black holes but not typically at their surfaces, and such as those that had to occur in the initial state of the universe when the big-bang explosion began—are able to create matter. In fact they have to create matter. In their presence the "vacuum" is unstable against production of matter. And one is in the position now of beginning to do calculations to see just what kinds of matter and how much had to be produced by the initial intense gravitational fields at the birth of the universe. Moreover, there are glimmers of hope that from such calculations we may learn the precise form in which the matter had to come out, that we may learn why there are more baryons than antibaryons in the universe, and why the ration of photons to baryons in the universe is 10^9.

My colleagues label me an optimist when I speak, as I have here, of future goals and trends in relativistic astrophysics. However, to me the achievements of the past two decades—the creation and development of X-ray astronomy and long baseline radio interferometry, the construction of unmanned observatories in space, the discoveries of quasars, pulsars, neutron stars, cosmic microwave radiation from the big bang, and perhaps black holes—these achievements justify high goals for the future, strong optimism, and intense work. ☐

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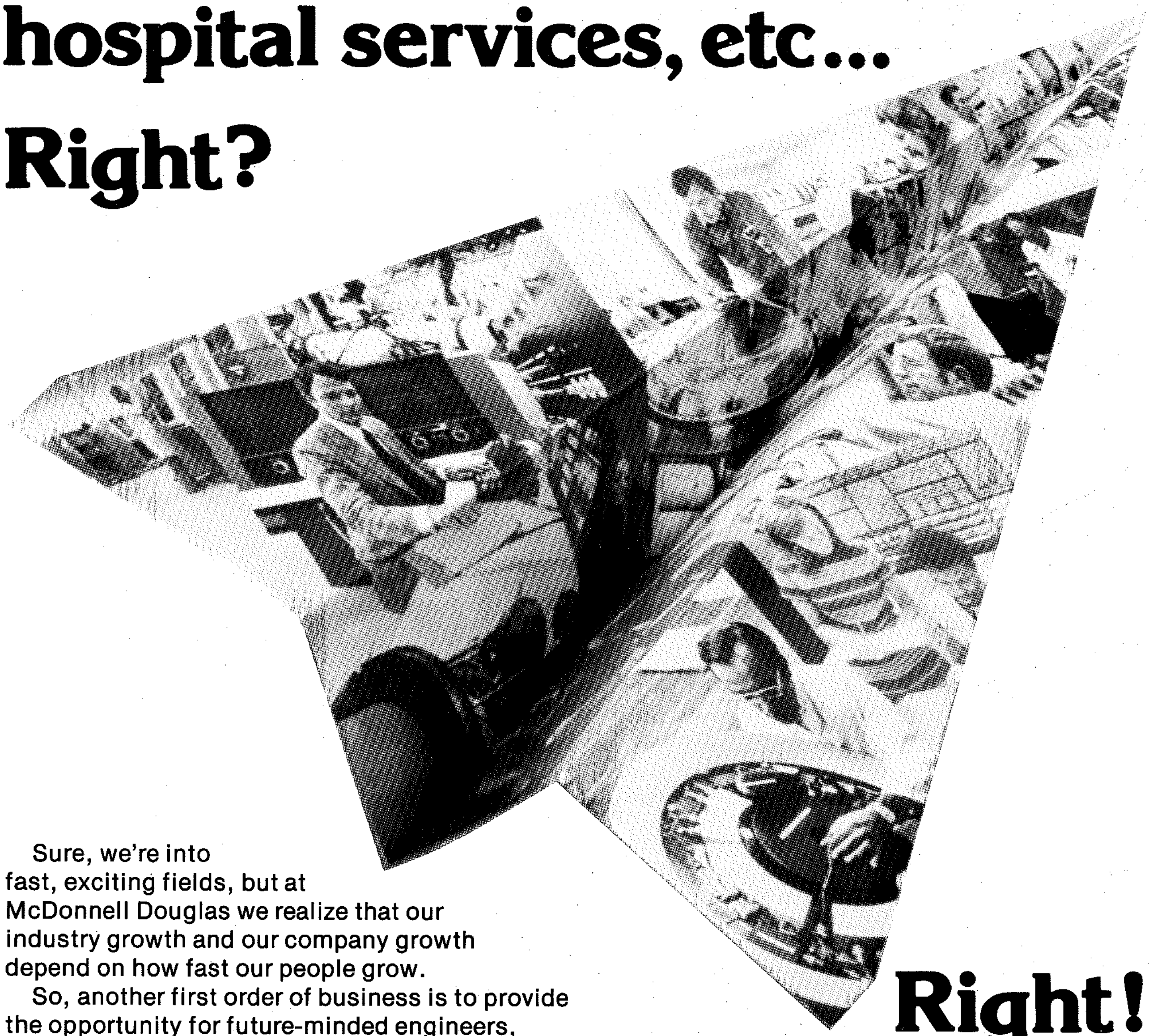

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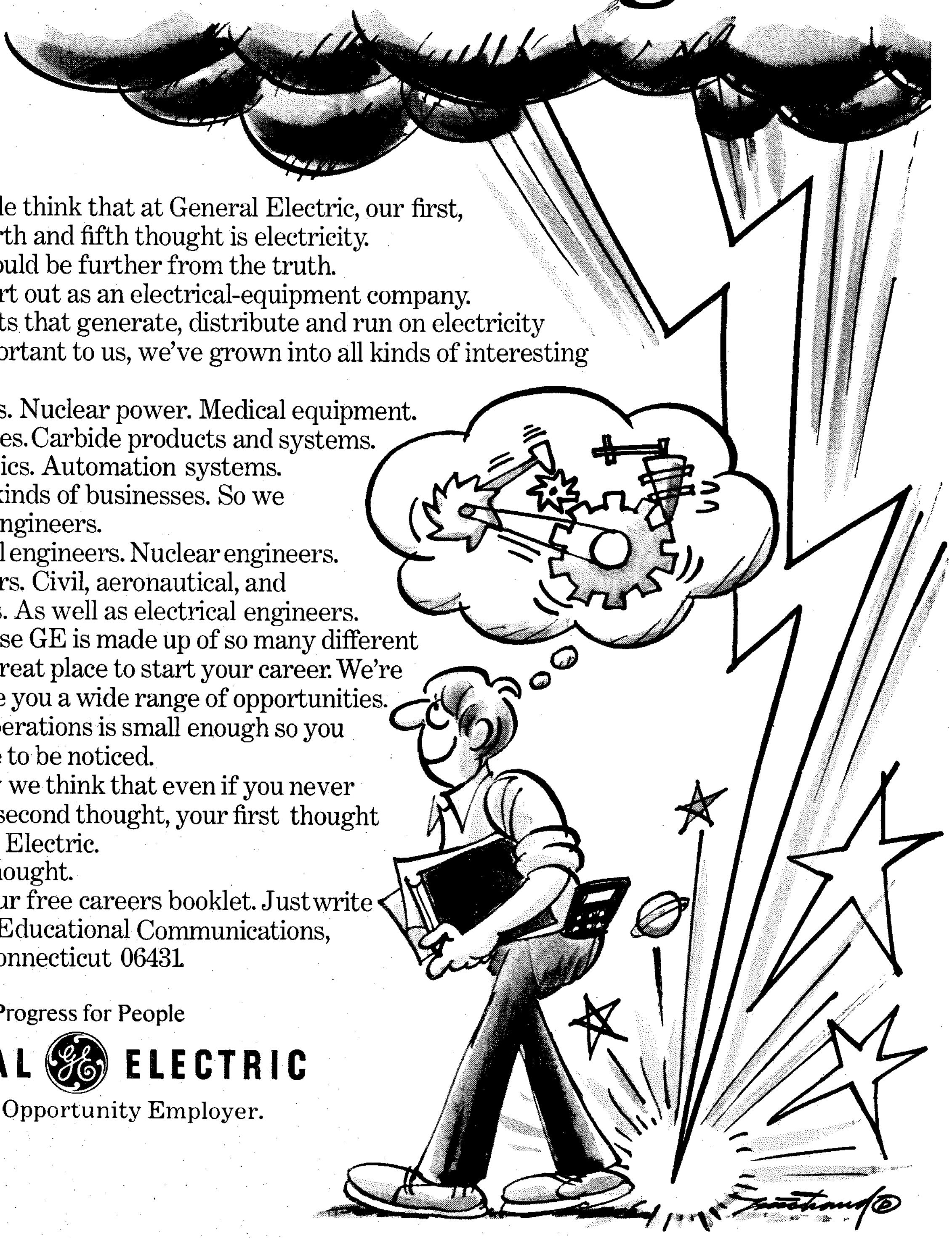

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