Engineering & Science

California Institute of Technology | January-February 1978

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In This Issue

Night Flight

On the cover—one of the world's most unusual birds sits for his portrait. The oilbird has a highly developed visual system even though it is reared in total darkness. This anomaly prompted John Pettigrew, associate professor of biology, to spend several weeks in Colombia, South America, studying the bird at close hand. What he found proved so provocative that he extended his studies to another oddity in the avian class—a night-flying seagull of the Galapagos Islands.

Pettigrew's research has been largely on the role of early vision in the development of the brain. He is an MD from Australia by way of a three-year postdoctoral stint at UC Berkeley where he shared in the discovery of a special class of "binocular" nerve cells in the cat. More recently, he has worked with ethologist Mark Konishi, Caltech professor of biology, on the visual cortex of birds.

No one is more delighted than Pettigrew with the opportunities for travel offered by these new subjects. He closed a recent Watson Lecture about them with a half-promise that his next trip would be to investigate a nocturnal parrot in New Zealand. "Vision and Birds of the Night" on page 8 is adapted from that talk.

John Pettigrew and guides in Colombia.

Nuclear Energy

Alvin M. Weinberg is currently director of the Institute for Energy Analysis at Oak Ridge Associated Universities in Tennessee, a position he took in 1965 after 20 years as director of the Oak Ridge National Laboratory. Weinberg actually began his career as a mathematical biophysicist at the University of Chicago, but World War II cut that effort short, and in 1942 he began work in nuclear energy. He's been at it ever since.

In 1960 Weinberg received both the Atoms for Peace Award and the U.S. Atomic Energy Commission's E. O. Lawrence Memorial Award for his contributions to the theory and development of fission reactors. More recently he received the New York Academy of Sciences Award and the first Heinrich Hertz Prize of the University of Karlsruhe.

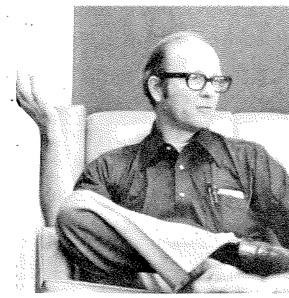
Weinberg has written extensively on some of the difficult problems of public policy posed by the growth of modern science, and in 1967 he published *Reflections on Big Science,* a book that discusses the new kind of large-scale scientific enterprise, of which Oak Ridge National Laboratory is an example.

"Outline for an Acceptable Nuclear Future" on page 4 is adapted from a talk Weinberg gave at an energy policy seminar held on campus last May.

Fair and Colder?

Nobody can do anything about the weather—much less the climate—but Stephen Schneider is one man who is trying to understand what's happening to the world because of climatic factors. He is currently deputy head of the Climate Project at the National Center for Atmospheric Research in Boulder, Colorado, and is widely called upon as an expert on topics such as water supply and climate, climatic effects of nuclear and alternative energy systems, climate dynamics, and climatic variations.

Schneider is editor of *Climatic Change,* a journal devoted to the description, causes, and implications of climatic change, and he is the author of the recently published book *The Genesis Strategy: Climate and Global Survival.* With all those—and more—credentials, he was a natural to speak at The Next Eighty Years conference held at Caltech last spring. "The Possibility and Consequences of Climatic Change" on page 15 is adapted from that talk.



Local Boy

John Andelin was right at home talking to members of the Caltech community last October as a guest of the Caltech Y. He's talked to them before, particularly to students. In the first place, he was a student here himself (BS '55, PhD '67). During his graduate school days he was also an RA for a couple of years for the off-campus student group, Throop Club. He was then the Ricketts House RA for four years. He was so good at it that in 1962-63 when Robert Huttenback, at that time Master of Student Houses, took a year's leave of absence, Andelin took over as Acting Master. After that, he says, he gave up the RA business to get down to the business of finishing up his graduate work.

Doctorate in hand, in 1967 he went to work for his thesis adviser, James Mercereau, doing low-temperature physics at Ford Scientific Laboratory in Newport Beach. In 1969 he changed jobs and focus by going to Harvard to work in solar physics.

Andelin started a stint in government service at Oak Ridge National Laboratory in the spring of 1971, leaving there in the fall to go to work with Mike McCormack, Democratic Congressman from the state of Washington. He has been in Washington, D.C., ever since, and he talked about his experiences there at Caltech last October. "My Life As A Hired Gun" on page 21 is an informal account of a Techer's life in politics.

Letters

Cover Story

In its last issue E&S *ran a cover picture and a story about Robert Sharp, professor of geology, and his annual course in classical field geology—Ge 136. We got some fan mail on the story, but what was even nicer, so did Sharp. Here are excerpts from a couple of his letters.*

San Francisco

Dear Bob,

I suppose congratulations on the Penrose Medal*—a well-deserved award—are in order. Yet I enjoyed more the spread in *Engineering and Science* re Ge 136.

I recently had a note from my businessman brother-in-law saying he'd read in some journal a forecast of "professions in the 1980s." It listed geology after medicine (and before computer analysis!). I've been saying for a long time that geology *is* that important. If the science does reach that position, it will be because of your type of teaching—to which black-box stuff and research is both peripheral and indebted.

Congratulations,

BILL
(William H. Freeman, President, Freeman, Cooper & Company, publishers)

**The Penrose Medal is the Geological Society of America's highest honor. Sharp recently received it in recognition of his contributions to geology through both his research and his leadership.*

Coronado

Dear Bob,

Vicariously I shared the excitement and hard work of your Ge 136 in the *E&S* that arrived this morning.

Although I don't think you resemble him in many respects, the picture of you worshiping your favorite rock on Casa Diablo Till looks remarkably like Nelson Rockefeller (above).

Since retiring in '73, except for my continued consulting, I've become more active in alumni activities at Caltech. It's good to see how well so many of our former colleagues have done, especially in achieving happiness.

After all, that's what it's all about, isn't it? You certainly are near the top in this department.

It was sad to hear that football is being discontinued, though I'm not surprised. I'm sure you share my conviction that we were indeed lucky to have been at Caltech when it was possible to combine sports under Stanton* and scholastic effort in reasonable proportions.

Keep up the good work. Hope to see you for more than a handshake one of these days.

CLARK
(Clark Goodman, '32, professor of physics, emeritus University of Houston)

**William L. "Fox" Stanton was Caltech's physical director from 1921 until the early 1940s.*

For the Record

Villanova, Pa.

Editor:

Let me say, first, that in my opinion you and your staff are doing a magnificent job in putting out *Engineering & Science*. One only wishes that funds were available for more frequent issues. The latest one came in today's mail and, as usual, I read it from cover to cover with great interest, even though Kip Thorne had me hanging on the ropes a bit.

Now for the nit-picking. In the fine tribute to Bill Michael, whom I remember well, the statement is made that he joined the staff of the California Institute of Technology in 1918, when it was still Throop Polytechnic Institute.

In 1918, and for several years previously, CIT was TCT, Throop College of Technology. I cannot tell you the date when TPI became TCT, but it is my impression that its name was changed at or before the time when Throop Hall was built on the present campus site, and that was 1912 or earlier, if I am not mistaken. My father, W. Howard Clapp, became a member of the faculty about 1914, and it was definitely Throop College of Technology at that time.

The big T on the mountain was constructed by removing brush soon after 1914, and I suspect that many present-day students are not aware that it originally stood for Throop, not for Tech.

GEORGE W. CLAPP, '26

You're right; we're wrong. Here—for your records, ours, and anyone else who is interested—are the facts: Throop University was founded in 1891. In the spring of 1893 it was renamed Throop Polytechnic Institute. In 1913 that name was changed to Throop College of Technology. Finally, in 1920, TCT became the California Institute of Technology. And Throop Hall was dedicated in 1910. We have now posted all this information on our bulletin board. We hope it will be an effective reminder.

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Outline for An Acceptable Nuclear Future

by ALVIN M. WEINBERG

Nuclear energy is in trouble. Despite the reassurances offered by its proponents, a substantial, and possibly growing fraction of the public is uneasy about the course we are following. To be sure, the nuclear moratorium bills in the United States have been defeated at the polls by a vote of 2 to 1. Nuclear proponents point to the two-thirds who favor nuclear development and consider this a mandate to go ahead. But I think it is fair to say that a primary energy system that is feared or rejected by 33 percent of the public is not going to survive.

Three possibilities emerge. First, that nuclear energy will gradually disappear and, except for the bomb, the world will revert to the situation that existed before December 2, 1942, when the first man-made chain reaction was established. One must remember that fission itself is rather a fluke. The conditions for establishing a self-sustaining chain reaction might not have been met had the number of neutrons per fission been less than 1, or if man had evolved after all of the ^{235}U had disappeared. There was nothing pre-ordained about fission energy or the discovery of fission in 1938. Had this discovery been delayed by 50 years, we would somehow be doing without it.

The second possibility is that our fears about fission energy may simply subside. As man acquires more experience with reactors, and more particularly, as the public acquires familiarity with radiation, it is quite possible that the opposition to fission will wane. The analogy with the introduction of electricity is close. There are still some elderly people around who were once uncomfortable about electricity; yet these fears have largely disappeared as electricity has become part of everyday living.

An essential element is missing in this analogy: the bomb. Even though we resolve our concerns about personal safety, or even genetic hazard, the bomb and proliferation make nuclear energy special. It is on this account that I believe nuclear energy will never be accepted to the unquestioning degree that electricity has been accepted.

The third possibility is that we can devise fixes—technological and institutional—that will make nuclear energy acceptable. Can we draft a peace treaty between those who oppose nuclear energy and those who support nuclear energy? Can we outline an acceptable nuclear future?

Acceptability and need are conjugates. What is acceptable, and how much risk we are willing to take, depends on how badly we need, or think we need, nuclear energy. This perception of need depends both upon our projections of energy demand and upon the energy situation at a given time. To be sure, the rate at which nuclear energy was introduced in the United States was determined pretty much by competitive market forces rather than by perception of energy demand. (And it is notable that in the Soviet Union nuclear power has been introduced at less than one-fifth the rate it has been introduced here.) But once nuclear power has been introduced, once a $75 billion industry is in being, the *need* for continuing the enterprise is overwhelming. It would be disastrous, at least in the short run, simply to shut down the nuclear enterprise, what with oil embargoes and other energy shortages.

What is at issue, then, is not so much what is to be done with the nuclear system already in place; it is what is to be done in the future. If this future is perceived to be a high-energy future, then the need for nuclear energy and the acceptable risk are correspondingly higher; if it is a low-energy future, the need, and therefore the acceptable risk, is lower.

SEMANTIC CONFUSIONS AND CONSENSUAL CLIMATES

Let us first dispose of a semantic confusion—the two meanings of the word "acceptable." As the nuclear enterprise now stands, it is acceptable neither to those who oppose nuclear energy nor to those who favor it. It is unacceptable to the former because they consider it somehow too dangerous. It is unacceptable to the nuclear industry because they are frustrated by administrative delay and uncertainty, by serious cost overruns, and by continual bickering.

But most of the frustration of the proponents simply reflects the basic unacceptability of nuclear energy to the opponents; many of these frustrations would disappear if nuclear energy were acceptable to the opponents. The regulatory process is lengthy and court-ridden not because the regulators are poorly organized or doing a bad job, but rather because the underlying technology that is being regulated has not received an adequate consensus. In the absence of such consensus, the regulatory process has become subverted; it becomes an instrument for bringing to focus the profound differences in perception between pros and antis, and in the process it frustrates nuclear energy. Indeed, the process becomes a background for what has been described as a religious war. Thus, although some of the fixes that I propose will be aimed at getting nuclear energy off dead center, I consider these secondary. Unless we can arrive at a system that commands a consensus, any fixes that satisfy only the pros will be submerged in the overall opposition.

It can be argued that we set an impossible task. How do we know when an adequate consensus has been achieved? Who, after all, speaks on whose behalf? Which antis are to be placated, which pros are to negotiate? In a democratic representative society, are we not constrained to use the duly constituted instruments of authority—our elected representatives, our regulatory bodies, our judicial system? I doubt that anyone has a clear conception as to how to fully legitimize dissent when it is deep-rooted and widespread. Nevertheless, our system has been resilient enough to establish what I might call "consensual climates" even on issues that at one time were bitterly divisive. Civil rights was deeply divisive, yet it was finally largely resolved. We have achieved a consensual climate with respect to this issue. It is this consensual climate with respect to nuclear energy that we seek to establish without giving up nuclear energy.

CRITERIA TO BE MET

What really bothers opponents of nuclear energy? The opposition is concerned with issues at three different levels. First is growth. Those who are opposed to growth as a matter of principle are opposed to nuclear energy since nuclear energy, insofar as it is unlimited, gives the technological base for unlimited growth. Related to this concern is centralization and bureaucratization; growth can be managed only by centralization, and centralization is bad. Since nuclear energy is the epitome of centralized technology, it evokes fears among those who long for a decentralized and, one hopes, a more resilient society.

The second concern has to do with proliferation; indeed, this at the moment seems to be the main objection to nuclear energy. There are many who insist that no nuclear energy system can be devised to be proliferation-proof, and that this alone warrants a rejection of nuclear energy. Proliferation is of course not indissolubly connected with nuclear power. The best one can hope for is a way of delaying, not stopping, proliferation.

Finally, there are the concerns over the intrinsic safety of nuclear energy—waste disposal, reactor accidents, routine emissions, possible accidents during transport, toxicity of plutonium, vulnerability of the

A Nuclear Glossary

^{233}U	— A fissile isotope of uranium produced from ^{232}Th
^{235}U	— A naturally occurring fissile isotope of uranium
^{238}U	— The abundant naturally occurring isotope of uranium
^{232}Th	— Naturally occurring thorium
^{239}Pu	— Artificially produced plutonium; this isotope can be used as a nuclear fuel just as can ^{233}U and ^{235}U.
^{240}Pu	— An artificially produced isotope of plutonium
Breeder	— A reactor that produces more nuclear fuel than it burns
Burner	— A reactor that burns more nuclear fuel than it produces
EBR-II	— A 60-megawatt experimental breeder reactor
MW(e)	— The unit of electrical power of a nuclear reactor as opposed to thermal power
GWe	— A billion watts of electric power
Quad	— 1 quadrillion British thermal units

nuclear system to sabotage and diversion. Most of my proposals will be aimed at remedying the present system's deficiencies in this general area.

The first concern, growth, we cannot remove by devising an acceptable nuclear future. Indeed, if one is convinced that growth is intrinsically bad, then one has relatively little incentive to devise such a future, since any nuclear future makes growth, or at least a shift to electricity, more feasible. If this belief comes to dominate, and we adopt an extremely low-energy, non-electric style of living, then it is doubtful that nuclear energy can survive in any case. But the remaining two concerns—proliferation and safety in the broad sense—I believe can be ameliorated without rejecting nuclear energy.

The nuclear energy system comprises mining, enrichment, fuel fabrication, reactor construction and operation, reprocessing, and waste disposal. It is complex and intricate. The larger the system, the greater are the chances for system breakdown, since there are more points that are vulnerable. All of the concerns increase as the amount of nuclear energy increases. If the nuclear enterprise were small, and served merely as a short-term transition to other, more benign forms of energy, the concerns would be small and limited. The issues become stark and urgent only when the nuclear system becomes very large, and is regarded as the energy mode that will continue far in the future.

Thus if we are to design an acceptable nuclear system, we must first agree on criteria for acceptability, not merely when the system is small but when the system is large, and the full systems problems emerge. In theory we must decide, for example, what calculated reactor accident probabilities, or how much flow of plutonium, or how big an inventory of wastes in a fully deployed system are acceptable. We cannot, of course, state precisely the acceptable thresholds for these probabilities. We can, however, estimate these probabilities and their implications in a fully deployed system, and then see what can be done to reduce these probabilities.

THE TWO PHASES OF NUCLEAR ENERGY

Nuclear energy will develop in two phases, Phase I and Phase II. Phase I is based on reactors that burn ^{235}U, Phase II on breeders that essentially burn ^{238}U or ^{232}Th. We cannot say with assurance when Phase II will displace Phase I, since we do not know how much uranium we have. This is the strategic dilemma that has always plagued development of nuclear energy. Some of the early workers, notably Walter Zinn and Eugene Wigner, hoped to avoid this dilemma by skipping Phase I altogether. It was on this account that most of the original civilian reactor development at both Argonne National Laboratory and Oak Ridge National Laboratory was centered on the breeder. Others, notably Bennett Lewis of Canada, disagreed: Nuclear energy based on ^{235}U, possibly enhanced with the introduction of advanced converters, was a sufficient goal. "Breeders are not necessary," thundered Bennett Lewis in the 1950s; and even Eugene Wigner, who considered the enterprise valid only if the breeder were developed, conceded that the breeder might develop out of the technology of burners, rather than developing entirely de nouveau.

Phase I is self-limiting. We can estimate its magnitude if we can estimate the reserve of uranium cheap enough to be used in a burner. We may take for this the official figure of 3.5 million tons in the United States, though we recognize that the acceptable cost, both economic and environmental, of uranium usable in a burner may increase if we are badly strapped for a non-fossil energy source. The uranium required to inventory and fuel a 1000-megawatt Light Water Reactor (LWR) for 30 years without recycle is about 6,000 tons, with full recycle about 4,000 tons. Thus our assumed uranium reserve will support between 600 and 900 large LWRs for their lifetime. The Institute for Energy Analysis's most recent "low" estimate of nuclear electricity in the year 2000 corresponds to about 300 LWRs, growing to, say, 400 LWRs by 2010. Phase I then might run its course, say, 50 years later—by 2060.

Phase II, based on breeders, could last immeasurably longer, since the reserve of low-grade ^{238}U or ^{232}Th usable in the breeder is so great. Let us consider, somewhat arbitrarily, an ultimate Phase II comprising 1000 large breeders each operating at about 2000 MW(e). This system corresponds to 120 quads (q) being produced by nuclear energy. An alternative ultimate system might be half as large—500 breeders corresponding to 60 q. Let us now estimate the risks implied in systems of this magnitude.

We do not have figures for probability of meltdown for Liquid Metal Fast Breeder Reactors (LMFBRs)—assuming these are what we deploy—comparable to the estimates for LWRs. The latter probability as estimated in the well-known Rasmussen study is .00005 per reactor per year, of which only one-fourth grossly breach aboveground containment. If the LMFBR accident probabilities are the same as those estimated for LWRs, then the expected accident rate for the large system is .05 per year; for the small system, .025 per year—i.e., we can expect one accident every 20 or 40 years, depending on the size of the system.

Before asking whether this is good enough, we must recognize that by the time the ultimate United States system has been reached, the rest of the world will have also deployed many breeders. Scenarios have been developed at the Institute for Energy Analysis and the International Institute for Applied Systems Analysis that contemplate 10,000 large breeders. If the Rasmussen probabilities given above are taken seriously, then one estimates an accident, on the average, every two years.

This calculation illustrates the dilemma. From the point of view of an operator or utility operating a single reactor, 1 chance in 20,000 per year is acceptable. On the other hand, from the point of view of the system as a whole, it would seem the probabilities (1 every 2 years) are too high. Just as all DC-10s are grounded if too many DC-10s anywhere in the world misbehave, so one would imagine that breeders simply would not survive if, on the average, one of them melted every two years—and this is independent of whether the meltdowns occurred in the U.S. or elsewhere.

The same problems of system vulnerability, as opposed to individual vulnerability, apply to the other, less quantifiable risks. The U.S. system would contain some 2,500 to 5,000 tons of plutonium (Pu); the world system possibly 10 times as much. Whatever the risk of diversion or of contamination when the world inventory is, say 100 tons of Pu, these risks are certainly much larger when the inventory is 250 to 500 times larger. Or consider the matter of retired reactors. In a world of 10,000 reactors, some 300 would be retired every year, 300 new ones completed to replace them. Is this really credible?

When one looks at the matter from this point of view, one has almost reduced the full-scale deployment, worldwide, of breeders to an absurdity. Yet we cannot say any of this with certainty. Surely the gradual evolution of the technology will reduce the a priori risk probabilities. The legislated meltdown probability for LMFBRs of .000001 per reactor year, if achieved, would relieve much of our concern about large accidents. Some 40 years ago a distinguished Swedish aeronautical engineer estimated that if as many airlines flew as fly today, we could expect a crash every two days!

The system will inevitably be self-limiting: It will expand only to a size with which the society is comfortable, and this size will depend primarily upon the state of the technology. But no matter the state of the technology, we shall have to exercise non-technical ingenuity to reduce the risks, even though they cannot be quantified. In particular, what we seek are mechanisms that will:

- Minimize the likelihood of physical disaster
- Minimize the consequences of disaster
- Ensure institutional responsibility for as long as the nuclear system requires care

I would suggest the following measures will be required if Phase II—which may be very large and last for a very long time—is to be acceptable.

Physical isolation. It seems evident that only a relatively small fraction of our planet ought ever to be in contact with high-level radioactivity—and the smaller the better. This leads to the idea of committed sites surrounded by sparsely populated areas—in short, to a strongly collocated system. One thousand reactors might be accommodated in 5,000 square miles within the United States—say, 100 sites each containing 10 reactors and supporting chemical facilities occupying 40 square miles apiece, and 1,000 square miles for waste disposal. Some of these sites would be in the oceans; many of them would represent expansions of current nuclear plant sites.

The appropriate degree of collocation is negotiable. If breeders and their chemical plants are collocated (a return to the original concept of the breeder as a closely coupled reactor and a chemical plant), one minimizes transport of plutonium; on the other hand, the optimal size of the chemical plant may not match the output of the cluster of reactors, and in any case, as Phase II gets under way, transport of fuel between the reactors and off-site chemical plants is inevitable.

A firm national commitment to the principle that the nuclear enterprise is to be confined to as few sites as possible seems to me the very minimum. A more far-reaching policy—that all breeders and their supporting facilities shall be collocated—seems to be indicated, though this stronger policy is more open to argument.

continued on page 26

Vision and Birds of the Night

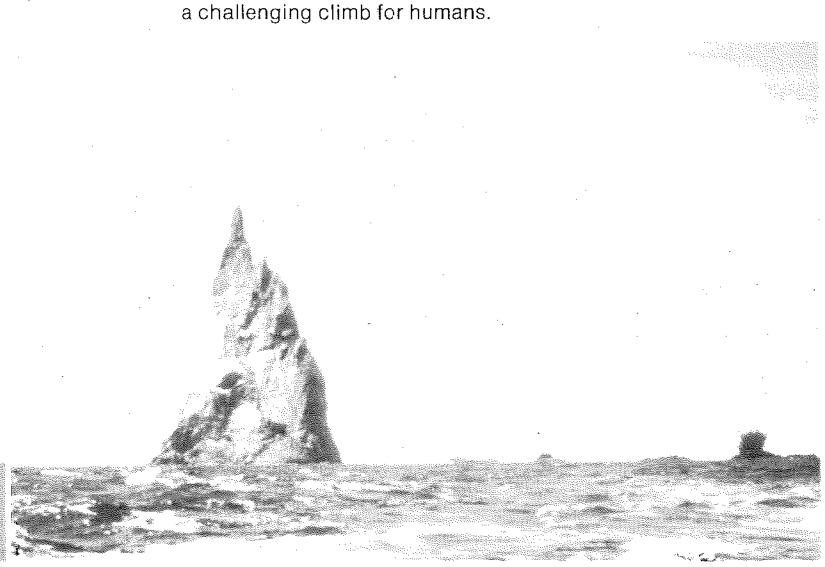
by JOHN D. PETTIGREW, MD

My interest in birds dates back to my medical school days, when I was a member of the first party to climb Ball's Pyramid, a 2,000-foot spire of rock that sticks straight up out of the ocean in the South Pacific not far from the Australian coast. There had been numerous previous attempts to climb this rock, but most of them failed because of various problems like seasickness and huge waves that made it almost impossible to land on it. In 1965 our party was able to land by swimming in through the big swells.

There were tremendous problems getting around to the climbing site even after we got ashore. And the standard of rock climbing was also fairly high—most particularly at a point near the top called the Cheval Ridge, which is so narrow that the safest way to climb it is to sit astride it, like a horse. In fact, it's possible to spit into the ocean from either side of this ridge.

Apart from all the difficulty and adventure, one of the attractions of the place for me was that it was a gigantic rookery. There are thousands of birds here.

Ball's Pyramid is a 2,000-foot-tall spire of rock rising out of the South Pacific near Australia—a home for thousands of birds but a challenging climb for humans.



At that time I didn't have any really good reason to justify studying them scientifically. Last year, though, a rationale was provided for fitting this adventure into my science—a discovery I made with Mark Konishi, professor of biology at Caltech and a neuroethologist with a lifetime's experience studying birds.

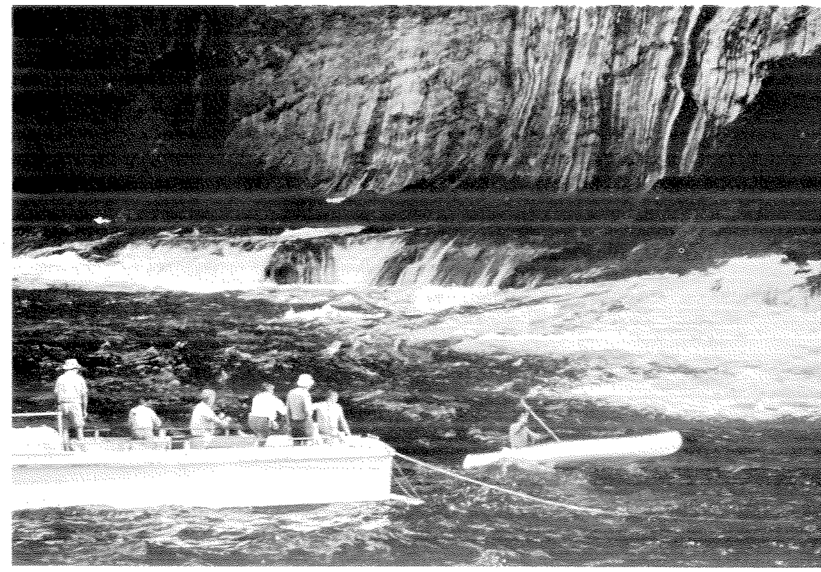
We were working on the brain of the owl, and there's a part of it called the *Wulst,* which under a microscope looks totally different from the human visual cortex. I'll remind you that owls evolved independently from us about 200 million years ago, so they're a separate evolutionary line. When we studied this part of the brain, we discovered to our surprise that it functioned in the same way as the visual cortex of monkeys, and presumably the visual cortex of man. We also discovered that, just as in kittens and baby monkeys, and presumably baby humans, this part of the brain is really sensitive to what the baby owl sees or does not see in his early development.

NATURE AND NURTURE IN THE OILBIRD: A NATURAL DARK-REARING EXPERIMENT

This fact led us to look more widely at the bird kingdom, and we were intrigued by reports of a strange bird from South America, called the oilbird, which has many puzzling features. First, it spends its first 100 days of development in total darkness. We found this puzzling because of our studies showing how important visual experience is in early development. The oilbird, *Steatornis caripensis,* also had a fantastically well-developed visual system, and yet it was reported to be able to fly about in caves in total darkness.

It's called the oilbird because when it was first described by Alexander von Humboldt back in 1799, the practice then was for Indians to pull the baby birds

A Caltech biologist speculates that there is a relation between the kind of eyes found in a couple of exotic birds and the fact that they live nocturnal lives



The rocky shore of Ball's Pyramid is no more hospitable close up than from a distance, but on his second trip in 1969 Pettigrew and his friends managed to get a canoe through the heavy swell.

out of their nests and roosting places high in the caves and boil them down for oil. The babies actually weigh more than the adults because they're fed with palm nuts, which are very oily. In South America the natives call it *guácharo,* which refers to the raucous cry of the bird. It's about the size of a crow, and it uses echolocation like a bat. It emits a sharp clicking cry and listens to the echo being returned from obstacles in its path. Unlike the bat's cry, this one is audible to the human ear.

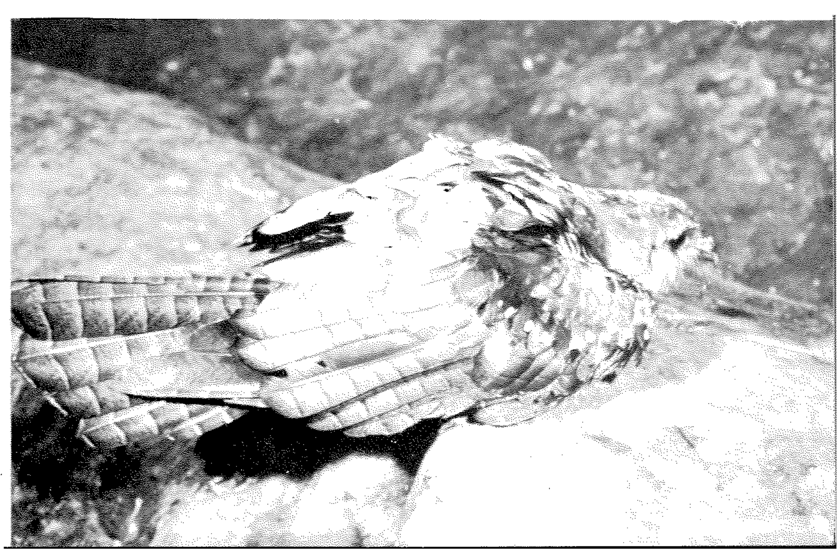
There were so many puzzles about this bird that we decided the only way to solve them was to go down there ourselves and sort them out. And for three weeks at Christmas of 1976 we went there, funded by the National Geographic Society. Oilbirds are found all the way from Trinidad to Venezuela, in northern Colombia, perhaps in Panama, Ecuador, and northern Peru. We studied the bird in the Tolima Province in Colombia, about 200 kilometers from Bogota.

My first introduction to the village where we hired some guides to take us to the cave was to arrive there with two flat tires—the car being jacked up beside the road about six miles back. At the local service station, Miguel Soler was very rapidly able to fix the tires with his vulcanizer kit, and he then joined our party. He's a very strong man who knows a lot about the cave. Our other guides included Marcos Meneces, a farmer with a wonderful knowledge of the local wildlife. In the dead of night he can tell you what just about every sound is. You'll hear a weird gurgle, and he'll say *chorola,* which is the local word for a tinamou. You'll hear another call, and he'll say *buhio,* which is the local name for a nightjar, and so on. He also has a fantastic knowledge of all the plants and all the animals in the area. In addition, we were helped a great deal in getting up to the cave by *Mediamundo* (which means "half a world"), a one-eyed horse. In return for the help he gave us in getting up the hill, we had to help him a little on the switchbacks. He could make the left-hand turns very well because his left eye could see, but on the right-hand turns you had to tug a little on the reins so he wouldn't go galloping off into the jungle.

Mark and I were very appreciative of *Mediamundo's* help because both of us suffered from a nasty lung complaint. We inhaled some insects in the cave, and both of us came down with acute breathlessness and chest pain. So we had a little trouble getting up the hill.

One member of our party, Nobua Suga, an expert on echolocation from Washington University in St. Louis, got very excited as we approached the cave. He had spent his life studying echolocation in bats, whose echolocating cries are largely inaudible to the human ear. It was therefore quite a thrill for him to experience directly the readily audible clicks of the oilbird. To get access to the cave and actually watch the

Oilbirds are beautifully marked brown and gray birds about the size of a crow. Their feet are so weak that they always "perch" on their breasts as this one is doing.



Looking fierce as a predator, the oilbird actually lives solely on fruit. The cat-like whiskers probably help guide the parent birds in feeding their young in the cave's darkness.

birds we had to swim down into total darkness—and you wouldn't want to swim too far because the stream ends in a big waterfall. The cave is full of birds, all circling around, and it was one of the most exciting experiences of my life to hear these birds effortlessly hovering overhead and clicking away.

But let me tell you a little about how these birds live. What do they feed on? The answer to that was fairly easy to determine because the seeds of the fruit they eat are littered underneath the nests in the cave, and many of these seeds germinate in total darkness. In 1799 when von Humboldt visited the cave in Venezuela, he was unable to persuade the natives to venture very far into the cave. They were frightened by the sounds of the oilbirds and the darkness, and by the ghostly white plants stretching up. For us, the plants offered a very convenient way of identifying just what the oilbirds eat, because I was able to talk the less superstitious natives of 1976 into collecting samples of the seeds. I took them outside, where we washed them in the creek, and then I could identify what the seeds were and match them with seeds from many of the local plants. It turned out that out of the 1,300-odd seeds I counted, 1,257 were of the variety that the locals call *chonta*—a type of palm nut. They are distinctly unpalatable, and I don't know how the oilbirds stand them. Some of the fruit they eat is more palatable. For instance, a *mamonsillo,* a delicious fruit that is sold by the local children on the streets, is also eaten by the oilbirds, and we fed this fruit to the birds we kept for a few days.

Inside the total darkness of the cave the oilbird has a rather slow, hovering, vertical flight pattern, and each bird constantly emits clicking noises to help orient itself.

What about the chicks? I went down to Colombia to find out something about baby chicks, and unfortunately, because of bureaucratic problems and delays, I wasn't able to make as many studies as I would have liked. They live in nests made of mashed-up palm-nut debris, which is added to every year until some of the nests get four to five feet tall.

These baby birds spend their first 100 days in the nest, which is a very long period of development for any bird. How do they manage without getting any visual experience? I don't have all the answers, but I have a few suggestions, one of which comes from studying the pattern of flight of these birds.

TWO MODES OF FLIGHT

When they're in the cave in total darkness, the oilbirds constantly emit clicks and listen for echoes so they can judge the distance and location of obstacles. In this situation they have a vertical, hovering flight. This mode of flight is rather slow, but even so it's impossible to catch them in a net—presumably because they feel the net with their wings and also with their whiskers. (They have very large whiskers, bigger than a cat's.) They are flying so slowly and they are so maneuverable that it's possible for them to feel the net and retreat.

In contrast, if there is the least bit of light, they stop clicking immediately, and they fly in a regular bird fashion, with the body horizontal. At the mouth of the cave, when there's a little bit of starlight, the clicking stops and the bird flies in a fast, soaring fashion, very adroitly dodging foliage and trees that were extremely difficult for me to see in the dim light.

After sitting at the mouth of the cave for some time, I noticed that some birds weren't as good as others at navigating in the dark. Their clicks varied much more in frequency, and occasionally I could hear them brushing against the walls of the cave. The other thing I noticed was that they came down to the entrance in very large groups. Often 10 to 20 birds would circle around in and out of the cave, and sometimes one of them would make a sortie out, but then the whole group would go back into the depths of the cave.

I speculate, although I don't have much evidence to support it, that one of the reasons they have such a long period of development is that these birds do a lot of learning. They may be acquiring this very difficult skill of building up a model of the world purely on this information from the echoes they get coming back. The large groups of birds that fly out to the mouth of the cave may be social groups where the young are being tutored in managing this very difficult task.

The second question is how they manage to see in such dim light. I was puzzled by the fact that a bird that can navigate in total darkness using echolocation should have such a good visual system—which it does. The eye is large; the aperture is very large compared to that of other birds. It's comparable to an owl's, so the oilbird's visual system is very well adapted to low levels of illumination, and this probably explains why it doesn't use the clicking means of navigation once it has a little light.

Another feature we found is that the ear recovers very quickly from a loud sound. In less than a millisecond and a half, the ear returns to its former sensitivity. This is an amazingly short recovery cycle, and presumably it would enable the bird to hear the faint echo come back very soon after it emitted a loud cry.

After the expedition was over, I was wandering around the Museo de Oro in Bogota where I saw some beautiful gold effigies of birds dating from the days of the conquistadores and before. Because these effigies have hooked beaks, weak and ineffectual feet, large wings, and long tail feathers, they are obviously likenesses of oilbirds. The figurines have spiral ear plugs just like those worn as a sign of nobility or deity among the ancient Indians. In fact, because the Inca nobility wore such large spiral ear plugs, the Spanish nicknamed them *orejones* (the Spanish word for the outer ear) to refer to their elongated ear lobes. There is also a legend about the origin of the Incas that involves a bird and these strange ear plugs.

In the light of that I was intrigued to find some artifacts in which the Indian artist chose to emphasize that feature of the oilbird that accounts for its astonishing ability to navigate in total darkness—the ears. I wonder whether our discovery that this bird has very special auditory abilities is not new at all. Perhaps some very astute Indian naturalist thousands of years ago, by observation, came to the conclusion that this bird has very sensitive hearing and perhaps even came to the conclusion that it was using echolocation to navigate in the dark. If that's so, it might explain some of the legends that are based on birds, and this custom of emphasizing the ear lobes.

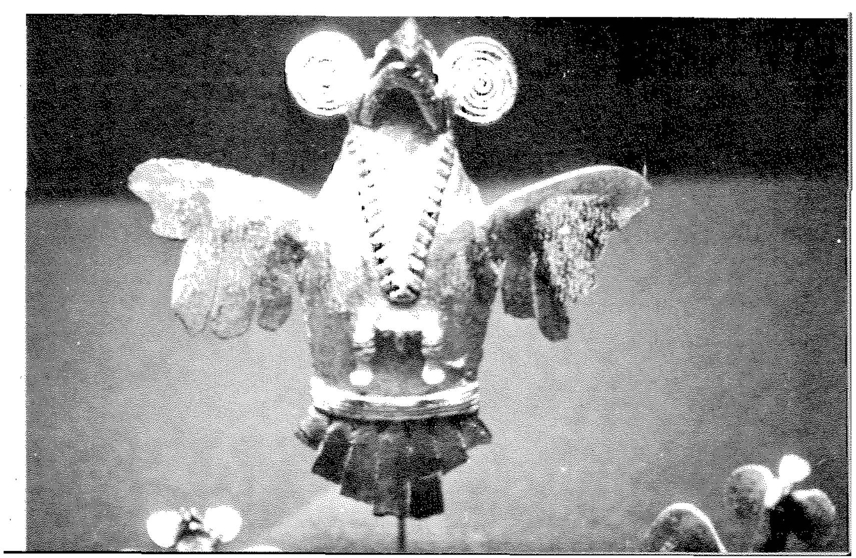
At the entrance to the cave where the oilbirds live, one specimen, stimulated by light, begins to fly horizontally—and silently—in the fashion of most other birds.

WHY HAVE FRONTALLY PLACED EYES?

There's a final puzzle about the oilbird: It has very frontally directed eyes. Most of us associate frontal vision—both eyes looking straight ahead—with an animal that hunts for its living, and this bird lives solely on fruit. It's rather hard to look at it and imagine that such a fierce character is a vegetarian, but what I've seen of it, plus some observations I've made on hawks and falcons, has led me to puzzle about this frontal-eye syndrome. The fact is that this bird has frontal vision but is not a predator, and many predators have very good binocular vision—that is, they can see very well in front of them—but they don't have their eyes pointed straight ahead. Swallows are also like this.

So let me speculate for a minute about this puzzling arangement. My theory is that the frontal arrangement of the eyes is related to the fact that birds with such an arrangement evolved to live in the nocturnal niche. It's related to the fact that the bird is adapted for night-

A golden effigy of a bird from the Museo de Oro in Bogota is almost certainly an image of an oilbird, complete with Incan spiral ear plugs indicating its auditory abilities.

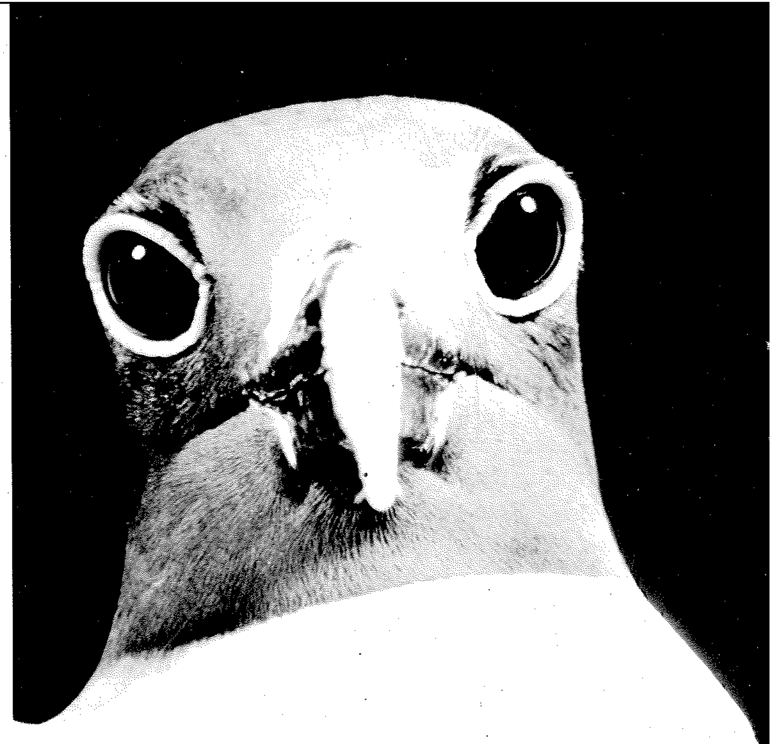


Vision and Birds of the Night

time viewing. It has a region of its retina called the fovea, and that is the part of the eye directed toward objects of interest. Now the swallow, falcons, kingfishers, and hummingbirds, for example, besides having a fovea that is directed straight ahead also have a fovea that points 45 degrees off to the side. In the same eye such birds have two fovea—one looking ahead for binocular vision and one looking off to the side for peripheral vision.

If it is possible to have simultaneous frontal and peripheral vision, why is it that the owls (not to mention ourselves) have both eyes rotated forward so that both point in the same direction and there's no peripheral vision? The answer, I think, is related to the fact that in contrast to all the birds who have simultaneously good binocular vision and peripheral vision and all of whom are diurnal (that is, they are active when there's plenty of light), the owl has a visual system that is adapted for working in very low light levels. The major adaptation is the enormous aperture of the optical system—the size of the opening in comparison to the focal length.

Though these swallow-tailed seagulls have taken flight over the sea, they are too unafraid of humans for it to be likely that they were startled by the approach of Pettigrew's field assstant, Dorothy Butler.



A close-up portrait of *Larus furcatus*, the nocturnal swallow-tailed seagull of the Galapagos Islands, shows its wide-aperture, frontally directed eyes—unlike those of most gulls.

Photographic buffs are familiar with the term "F number." Well, the owl has an F number of about one—a very fast lens. In fact, it's only recently that photographic manufacturers have been able to make a lens this fast. In contrast, the eye of the swallow has a much smaller aperture—a ratio of around two to three. One of the peculiarities of those large-aperture systems is that there are many aberrations. It's very difficult to build one of these systems that also has a wide angle because by the nature of the wide-aperture system, if rays enter away from the optical axis they tend to suffer a lot of aberrations. So if you have a large-aperture eye, adapted for working in low-level light, you may be forced to point that eye in the same direction from which you're getting the visual information. It may not be possible to look simultaneously in two directions with high accuracy. My prediction is that the frontalization of the eye that we saw in the oilbird and that is shared by the owl (and by us) is related to adaptation for the nocturnal niche.

A NATURAL EXPERIMENT

This is a nice theory, but how does one go about testing it? For what is known as a natural experiment, we should look for two closely related birds, one of which is nocturnal and the other a daytime bird. I talked to some ornithologists who told me that down in the Galapagos Islands there is a nocturnal seagull. Most seagulls, of course, are diurnal, and they have laterally placed eyes for peripheral vision. Without having seen this seagull in the Galapagos, I predicted that it would have more frontally placed eyes. Checking my prediction gave me an excuse to go off to the Galapagos Islands last summer. This nocturnal, swallow-tail gull is one of the very interesting adaptations that Darwin missed.

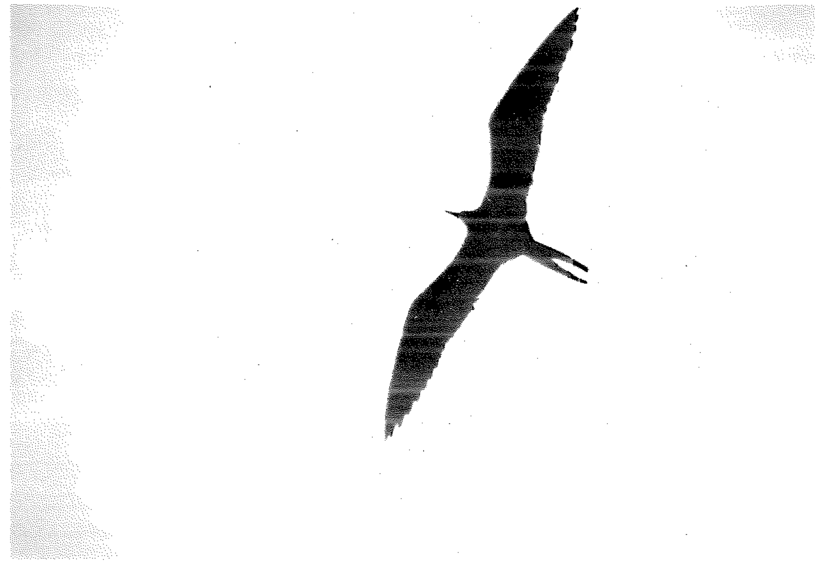
The Galapagos are on the equator about 600 miles from the coast of Ecuador. A plane goes there a few times a week from Guayaquil, and since there are about 60 islands in the archipelago, you have to choose which one you want to go to. I chose Genovesa, also called Tower, which is not a very good name because it's a very flat island. It's so flat that the fishermen who take you there refuse to start if there's any chance of arriving after dark. They're afraid they'll miss the thing and go heading off to Panama.

The Galapagos Islands are extremely inhospitable. Tower is rocky, harsh, and hot. I could only work in the early morning and the late evening. There are lots of fresh, sharp lava beds and lots of cactus. Despite its inhospitable nature, Tower is a paradise of wild life, and all the birds are very tame. It's literally possible to catch them by hand. To take photographs of their eyes, I just walked up to them, talking sweetly, and caught them. So even a rank amateur like myself can get quite passable pictures of these birds.

On Tower the bird I wanted to study is present in a colony of thousands. It's called *Larus furcatus,* which refers to its forked tail—a nocturnal, swallow-tailed gull. During the daytime these birds are in evidence all around the island. They hang around their nest sites, or congregate around the headland. They are rather beautiful birds—a little larger than the usual seagulls one sees around the California coast. When the nest site is approached, they give a characteristic alarm call and fly in a circle and come back and land on the nest. The reason they stay home all day is that they're protecting their nests from the real villain—the frigate bird.

The frigate bird has a wingspan of about 7 feet and weighs about a pound. It has the lightest wing loading of any bird; it's a master flier—and it's also a villain. I couldn't help getting a little paranoid about this bird because I arrived on the island a little bit seasick and a little sunstroked, and these birds, with their vicious hooked bills, filled the sky. They were soaring around all day, and their huge black shadows swept across the island, giving me visions of those nasty flying beasts in Tolkien's *Lord of the Rings*—the Nazgûls.

This sounds like a delusional vision, but it was confirmed when one of these birds, as I was watching it, grabbed hold of the little pink foot of a booby who was



A master flier, the frigate bird is also a master predator, who steals both food and baby birds from the gulls and boobys that nest in the Galapagos.

returning after a couple of days of hard work getting fish for its young. As the booby was flying in, this frigate bird gave a few tweaks on the booby's foot. It does this because it wants to hear what kind of a noise the booby makes. If the booby makes a loud noise of protest, it means it has an empty crop. If the noise of protest is rather more muffled, that's a sign that the crop is full, and then the frigate birds get to work on the fellow. Eventually, if he does have a full crop, he regurgitates it and crash-lands in the water. Then the frigate birds, completely effortlessly, drop down and before the food hits the water, they snap it up with their long bills.

I've painted an overly harsh picture of these frigate birds. It's true that they do go catch flying fish for themselves, and they're remarkable both for their visual ability and their aerodynamic ability. They have a visual system similar to that of the swallow, with the laterally placed diurnal eye. Certainly, when the sun goes down, this bird clears from the sky immediately and goes to roost. The reason I overly stressed the villainous piracy of the frigate birds is that it is the probable explanation for the evolutionary pressure that has driven the swallow-tailed gull to become a noc-

The nocturnal seagull is one of the inhabitants of the Galapagos Islands that Darwin missed. These two specimens keep a wary watch for the rapacious frigate bird.

turnal forager. It stays home all day to protect the nest, and on many occasions we watched frigate birds try to take the chick from a nesting gull. Then at night, when the frigate birds have gone to bed, the gulls go off and forage.

More questions arise as to how they can manage to forage at night. They do have a very large eye; in fact, it is comparable to that of the oilbird, which is comparable to the eye of a barn owl. Like an owl they have an eye with an F number of about one. It's much faster than the eye of other seagulls. Still, I was puzzled about how they could hunt on a very dark night. I was puzzled because the other birds I've mentioned—for example, the owl—get a lot of help from other cues. Owls can hear their prey rustling, and they have acute auditory systems that enable them to catch prey even if there is no light at all. The oilbird, similarly, has a very good nose and is able to track down foods partly by olfaction.

I wondered about what help this bird might get, and I think the answer was provided by the fact that the Galapagos, like many tropical waters, have a large amount of bioluminescence. If you go swimming at night, and you move your hands under the water, you can create beautiful patterns because the marine organisms luminesce as they are disturbed. And if one examines the contents of the crops of these birds when they come back from a hunting expedition, mixed in with the squid and fish they've taken are many of the luminescent organisms. So it's possible that they get a lot of help in tracking their prey at night by the track that is left by fish streaking through the water and disturbing the phosphorescent organisms.

But how about my prediction? I went all the way to the Galapagos to test this prediction I had made that if a bird is nocturnally adapted, it should have more frontally directed eyes than a diurnal close relative. I took pictures of the inside of the eye. I made some measurements. But I think all of these are just incidental confirmation of the fact that this seagull's visual system is rather owl-like. It has frontally directed eyes, in comparison with other seagulls. So it was very gratifying to me to enjoy this adventurous expedition and at the same time to come up with a finding that verified my prediction. There is a link between frontalization of the eyes and the nocturnal niche.

That's one natural experiment where my theory was vindicated, but where will I go to next? Can I find any more? It turns out there is a very nice natural experiment one could do in New Zealand. There's a nocturnal parrot that's called the kakapo, or owl parrot. You can guess from the name that this parrot has very frontally directed eyes in comparison with other parrots. In fact, one of the first people to study this bird asserted that it was indeed an owl rather than a parrot with very frontal eyes. Unfortunately, it is an endangered species; not many have been seen in this century. But I have corresponded with a few people in New Zealand who have seen them, and it appears that the prediction also holds that the kakapo has much more frontally directed eyes than his close diurnal relatives.

This observation that there's a link between the nocturnal niche and frontalization of the eyes has some other implications in the way the brain is organized, because the visual pathways of a frontally eyed beast are different from those of a laterally eyed beast. It's for this reason that the statements I've been making have a little relevance to understanding our own visual system. We might ask the same question. Is it possible that there's any relationship between the human or primate condition—having both eyes frontal and looking in the same direction—and the nocturnal niche?

I think the answer is possibly yes, and the reason for this is (and most paleontologists would agree now) that the first primate—the first member of the line that gave rise to man—was a nocturnal mammal. The fossils of the first primate look very similar to the skull of a beast called the tarsier, a tropical animal found in Southeast Asia. It has a rather owl-like face—very frontally directed eyes—and there is much agreement that the primate line began with a beast like this. It is extremely nocturnal; it doesn't come out in the daytime at all. It's a predator that catches its prey at night.

I guess the point I'm making is that in getting some insight into the frontal-eye syndrome and its relationship to nighttime vision, I may help solve some of the puzzles about our own condition.

In conclusion, then, I hope I've given you some feeling for the way in which the zoological ferment of the tropics has influenced me in my thinking about vision and further influenced me to put this thinking into a broader evolutionary context. My proposal that the frontal position of the eyes could be related to the nocturnal adaptation has many ramifications and, of course, many prospects for exciting trips. It's also possible that it may lead to some elucidation of some of the puzzles about our own visual system. □

The Possibility and Consequences of Climatic Change

by STEPHEN SCHNEIDER

How does climate relate to the world predicament? I think the most obvious way is through its effect on the food supply.

In the very short time scale, climatic fluctuations take food production along with them, and globally that means that food production varies up to 5 percent per year. In 1972, after an almost unprecedented decade in which world food production was increasing faster than population (3 percent as against 2 percent), production dropped by about 1 percent over the previous year. Since we needed a 2 percent increase to keep even with population, and 1 percent to stay up with the added affluence (which primarily meant feeding grain to animals for the New Rich), this was really a loss of almost 4 percent.

That loss created shock waves in the form of price fluctuations, and people started talking again about famine. In the winter of 1972 India declared itself self-sufficient in grain, based on a very short number of years of experience with the Green Revolution, during which time there were also very good monsoons. But a monsoon failure in the following summer and another in 1974 made us aware that food production can still fluctuate globally on the order of 5 percent.

How do we deal with that? I think the way is through reserves. That doesn't just mean stockpiling enough grain in the world; it also implies a *distribution system.* The situation is analogous to being out in a beautiful but snake-infested region, where you know there are dangerous cobras, and so you bring your anti-venom serum along with you. But the day you go out on a hike and happen to get bitten is the day you have left your kit back in the tent. By the time you get back there for an injection, you're dead.

So the important point with reserves is not just to make sure that there are enough stockpiles in the world, but that they are where we need them at the time. This isn't so much a technical problem as a political one, of working out who has control of the local stocks and how they distribute them.

In my view, the middle-term time scale involves the rates of development, particularly the rates of energy development. If the decrease in birth rate that follows increasing quality of life is related to per capita energy consumption, we have to say that the rate of energy development may be an important component in bringing about a stable transition to a stable global population. If that's the case, can we bring the resources to bear in time to prevent those terrible catastrophes that many people see, such as times of famine?

The problem is that, in the process of bringing those resources to bear, one may get catastrophic side effects—an environmental one, perhaps. And therefore we have to ask: What are the climatic consequences of rapid technological development that involves pollutants? Climate change is only one of a variety of environmental possibilities from such developments, of course. For example, there can be local climatic effects from deforestation, or desertification could result from overgrazing marginal lands, particularly when there is extensive well-digging. There are also global issues such as potential climatic modification from carbon dioxide and other pollutants that are frequently by-products of energy development.

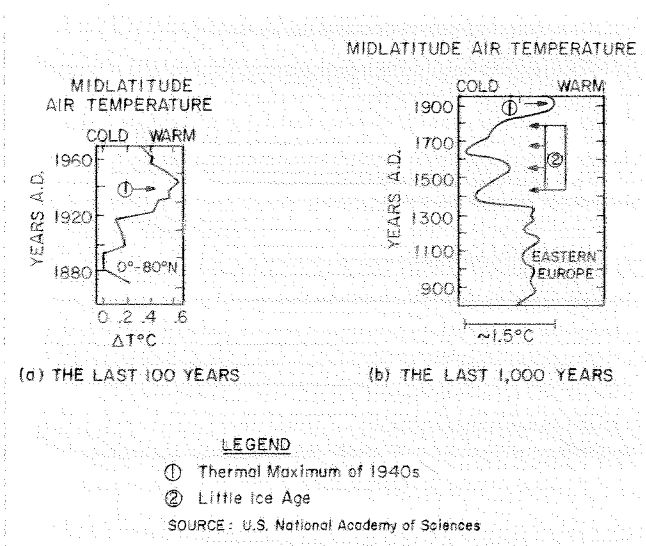
In the long run we want to have a sustainable steady-state population, with most people having a decent standard of living. But, since quality of life may be proportional to per capita consumption of something like energy, then the total population has to be small enough so that we can have a per capita standard that doesn't damage our environmental systems too badly. However, we have to start working toward that goal *before* we have too many people to have environmentally safe applications of high per capita technology. That time, of course, is now. And carbon dioxide pollution from fossil fuel energy is a prime example.

RECENT CLIMATIC TRENDS

The chart on the next page shows air temperature in the northern hemisphere, based upon the existing network of thermometers. In the short term (left) the temperature has risen by about ½ degree Celsius since the 1880s, and from the middle 1940s to the middle 1960s it dropped about ¼ degree. What's wrong with this picture is that there should be large error bars on it, because there are still vast regions of oceans not covered by thermometers. But the main point is that the range of variation is only on the order of ½ degree, and that's probably been significant enough to cause important local changes.

Taking a longer perspective (right), a record from eastern Europe over a 1000-year period shows a cold event that was called the "little ice age," when maybe it was only about 1½ degrees colder. In fact, I'm sure that no ice age has been much more than 5 degrees colder than today, on a global average. Long-term changes on the order of more than a few tenths of a degree in global temperature really start to become large. Locally, even over the long term, or globally, from one year to the next, the changes can be much larger, but globally and in the long run, ½ degree is a big change.

The little ice age is historically chronicled, and there is a quote I enjoy, from the French climate historian Leroy Ladurie, discussing what happened in France in the middle 1700s in a number of climate-induced local famines: "The price of wheat in Liège in 1740 went up . . . to astronomical heights. . . . The poor people of the town menaced the canons and others



Air temperature in the northern hemisphere—in the short term (left) and the long (right).

who had well-rounded bellies, threatening to ring their carillons to a tune they would not find at all to their taste. Prince George Louis, the governor, told the more prosperous citizens to 'fire into the middle of them. That's the only way to disperse this riff-raff who want nothing but bread and loot'."

One can find dozens of such stories. They make two points: (1) that climate-related famine situations have not only geophysical or health overtones, but also political ones; and (2) that a factor (which is not clear from these records) now becoming identifiable is the role of technology in changing our vulnerability to climatic fluctuations.

VULNERABILITY TO CLIMATIC CHANGE

In the past, Europe had more of a peasant village culture, and local villages depended to a large extent on local produce. The village, though basically self-sufficient, was probably living fairly close to the vest, with narrow margins of supply and demand. If there was a local problem with the harvests, it would generally lead to local pressures on nutrition, and perhaps to starvation.

After 1740 we don't hear of very many instances in Europe of local climate-induced famines. Possibly the climate got better, but I think the overwhelming factor was improvements in the technologies of storage and transport of food. The fact is that one can mitigate local fluctuations by buying food from a neighbor. It may cost you your hard-earned money, but you can get it. Then if you save up over a period of time, you can trade around.

I think that, through technologies, we've finally minimized our vulnerability to small local fluctuations. But I also think we're not invulnerable to climatic fluctuations; we've just changed the character of our vulnerability. In the past we had high-frequency, low-amplitude vulnerability—very frequent small failures in local regions of near self-sufficiency. Now we have what I would characterize as low-frequency, high-amplitude vulnerability. This is because there are two, three, or at most four major world granaries, producing much of the food for people other than those who grow it. We now face the situation where simultaneous shortages in, say, the United States, India, and the Soviet Union would mean numbers on the order of 40 million starving per year. I think you could increase that by a factor of five under the worst climate scenario I could conceive of, with tens of millions of people being threatened by the elimination of the surpluses in North America, or shortfalls in the USSR, or India.

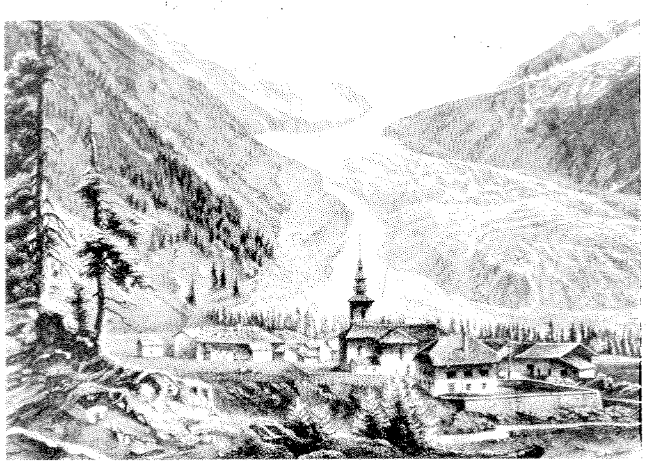
Fortunately we can put a buffer in the system, if we put aside adequate reserves. I've just written a book called *The Genesis Strategy,* pointing out that it isn't any new-fangled thinking to suggest that we have to have large margins of food safety as a buffer against fluctuations in the geophysical environment. In the book of Genesis, Joseph warns the Pharaoh of the seven fat years to be followed by seven lean years and suggests the storage of grain in the good years against the inevitability of the bad ones. Unfortunately, our Pharaoh during most of the last several years was Earl Butz, and it was very difficult to convince him of that wisdom, so that we had wildly fluctuating food prices and famines from 1972 through 1975. The Genesis strategy these days certainly is more than just saving food, and it's more than a food distribution system. It involves safety margins in a whole variety of technological and management systems that contribute to our basic survival commodities.

One more point I want to make about the ½-degree temperature change is shown in the pictures below of a glacier in the French Alps near the town of Argentière. One is a photograph taken in 1966, the other an etching made of the same scene 100 years earlier, when the hemisphere temperature was perhaps only ½ degree or so colder. A hundred years ago the glacier was right down to the plain of the town; in fact, that's probably why the town was put there. This area of the French Alps is a "marginal" environment, and I think that's the main point of almost all climatic changes. It's at the limit, where a slight change in temperature or precipitation can cause a large change of something else—in this case a glacier.

One other thing about this town—I can never resist noticing how unchanged it is, 100 years later, and I wonder how these people managed to stave off modernity. Don't you think it would look better with one of the old houses replaced by a Safeway or K-mart?

The real point here is that when we talk about fluctuations in climate we are not talking about the end of the world. I don't see that climate changes could bring an evolutionary end to the human race. I don't think they could even threaten more than 10 percent of the world's population in a direct sense; and that would be without reserves under the worst kind of fluctuations I can conceive. Does that make climatic change a crisis? Well, if you take a completely evolutionary perspective, I don't see it as a crisis at all.

I'm sure if we went back into human history, we would find many examples of famines or pestilence causing fluctuations in global population far greater than the 5 percent or so I see as possible from climatic fluctuations now. But with the world's current population, if only 1 percent were threatened (and I think that's quite realistic, even this year, if we had a major failure of the Indian monsoon), that would be 1 percent of 4 billion people or 40 million people. And 5 percent of course is 200 million. This is the level at which I see direct threats to people. We have to also ask the question whether a threat to "only" a few percent of humans by climate-induced famine can occur without leading to some other kind of desperate response that



A glacier in the French Alps near the town of Argentière—as shown in an etching made in 1866 (left) and in a photograph taken 100 years later. In 1866 the hemisphere temperature was perhaps only ½ degree or so colder.

leads to more general conflagration. Do we want, in fact, to risk that experiment (leaving aside the question of the moral compunction to prevent starvation)?

With that kind of perspective in mind we can see that climate change is certainly not going to eliminate the human race. It is not the small changes everywhere that concern me, but rather the large changes that could occur in marginal areas. Places that have just the right growing season may lose it, or those with just the minimum amount of water for agriculture might lose it. I don't see a 1-degree temperature change, for example, as being significant for most places in the world. What the 1-degree change represents would probably be a shift in the established position of major atmospheric circulation systems, so those people living at the margins of such circulation systems (like the southern end of the mid-latitude storm belt or the northern end of the Indian monsoon belt) could find that they've had a drastic change. Those people constitute the threatened 5 percent, and they could experience those radical changes. The rest would hardly notice anything —except by social, economic, or political connections to those directly affected. There would be compensation in other parts of the world also. The immediate problem is that farmers plant their crops to pre-existing expectations, and it may take them 25 years to catch up with a new climate.

Some people are talking now about warming and cooling trends. I don't know if the earth has been warming or cooling for the last seven years or so, because all we have are quick partial indications. We have millions of bits of temperature and other kinds of meteorological information, but they remain either unanalyzed or sitting on computer tapes, and there are only a handful of people around the world who analyze these things. So, ironically, we know the least about the ten years we've just lived through.

THE CASE OF THE COLORADO RIVER

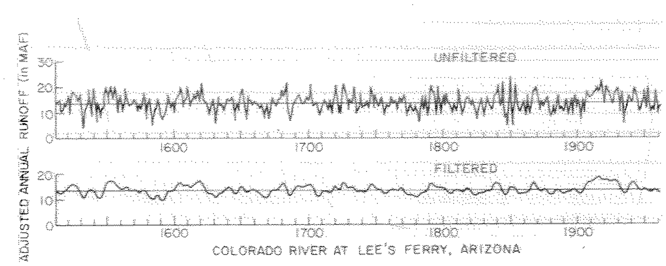
The chart at the right shows the adjusted annual runoff in MAF (millions of acre feet) for the Colorado River at Lee's Ferry in northern Arizona. The MAF measurement is a rough indication of what the Colorado River runoff is from the upper basin states—Colorado, Utah, and Wyoming.

When the Colorado River compact was drawn up in the middle 1920s, it was agreed that the upper basin states had to release half of the river's water to the lower basin states—primarily California, Arizona, and New Mexico, and to some extent Mexico. And the amount of water apportioned was based on people's concepts of "normal" flow. The runoff figures on the chart go back to the year 1500. Obviously no one was back there taking measurements, to our knowledge, so the early years are a reconstruction—by tree rings in this case, which measure climate and give a rough indication of whether it was a wet or dry year. We calibrate the long-term part by the recent record, where we know the stream flow.

Our reservoir capacity may not be capable of dealing with some of the periods, such as 1900 to 1930, which is about 25 percent above normal. Ironically, the Colorado River compact was drawn up at the end of this high-flow period, and, as a consequence, it was decided that the upper basin states should give 7½ million acre feet of water to the lower, annually. This amount was selected because 7½ million was assumed to be about half of the "normal," which recent experience (at that time) indicated was 15 million acre feet, as opposed to what we know now to be more realistically "normal"—13 million acre feet.

I said before a Senate subcommittee in March 1977 that we might want to renegotiate the Colorado River compact. (As a physical scientist I have to be careful what I say, because this is a value issue over which I have no "expert" credentials.) That's something I'm sure Californians don't want to hear. But the fact is that the Colorado had a long stretch of abnormally high runoff in the first quarter of the century, and the wrong numbers were apportioned. Nobody was evil back then; they just didn't have the right kind of records, and they apportioned a fixed *amount* of water, not a fixed *percentage,* as the upper basin states' obligation.

In the context of the next 80 years, the point is that whenever we depend upon a fluctuating physical



Adjusted annual runoff in millions of acre feet (MAF) for the Colorado River at Lee's Ferry in northern Arizona. The MAF measurement is a rough indication of what the runoff is from the upper basin states—Colorado, Utah, and Wyoming.

system, such as climate, we'd better leave large margins of safety, because the real catastrophes that are coming up in the Colorado River Basin stem from the fact that we have come to depend on every last drop of the 15 million acre feet we don't necessarily have. If we leave in large reserve capacities for extra flexibilities and we do our reservoir design for low flow, then someone will accuse us in the "good years" of wasting resources and slowing down development. Of course, the consequence of using the reserve is that we'll proceed with developments and then get the kind of shortages that lead to collapse.

Suppose, then, we are entering a 20-year period of 20-percent-below-normal rainfall, or even a 15- or 10-year period? If we let go of the remaining reservoir water and send it downstream to quell political pressure, that will be politically palatable this year, but during one of these long droughty periods we could have a very severe problem. If we don't let the water go and it turns out that next year is a good one, then we will be accused of all kinds of waste of resources, and we would have to pay for it politically at the polls. The point is that we do not know from theory whether to expect a protracted drought period or even if the long-term climate could be changing altogether, thus changing the mean runoff. Therefore, the best prudence in my opinion is to maintain, to the extent that we can, a reserve capacity based on the known frequencies and amplitudes of fluctuation. That generally means hedging, and hedging means insurance, and insurance means premiums, and premiums cost money. I think that has to be recognized as the price for hedging.

There is one more important consequence for the next 80 years, and that is that there are several ways to "cure" this water problem, and I think it applies to food also. One way is to build dams or diversion projects to increase reservoir capacity.

I'm very pleased to see that President Carter raised the red flag of question on some of those projects, and he did it in spite of some people screaming, "How could he have done it *now* when we need the water even more?" Well, many of them aren't going to bring water in tomorrow; we're talking about projects whose effectiveness is probably more than a decade away. Furthermore, there is more than one method to balance water supply and demand, and one can also talk about demand conservation as well as supply augmentation. We can reexamine where we waste water and ask whether we might be better off by curbing wasteful demand rather than building in more supply. We must recognize that building supply has some risks involving plain cost, risks that are environmental, and, in the case of dams, risk of safety.

A dam may well be the least safe of any energy alternative, unless we don't let people live in the flood plains. That's another issue of development, and the consequence for the next 20 years of this sort of issue is: If we are going to ultimately decide that we still need to augment the supply in order to provide a Genesis strategy—a reserve capacity—then we have to make sure that the price we're paying for the additional supply does exactly that.

This may mean that we will have to have some sort of statutory requirement that says a project built ostensibly for reserve capacity must be kept for reserve capacity. This involves things such as national land-use planning. There may even be constitutional questions in that kind of development-related growth situation.

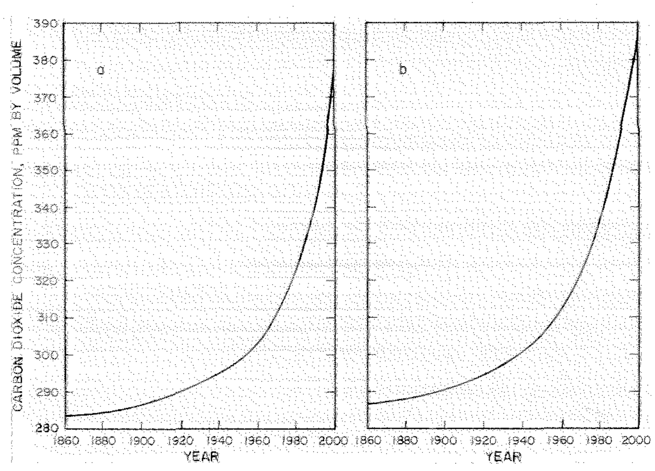
Moreover, there's an implication for energy in these reservoirs as such. That is, if we are going to turn to oil shales and coal in the West, which call for a stable supply of water, then we must ask: Where is it going to come from? After all, we are even now having trouble providing enough water for present users. The water issue has to be looked at very carefully, not only in the western United States, but in many other places in the world.

THE CO_2 PROBLEM

A word more about the carbon dioxide problem. We know that CO_2 is emitted from the burning of fossil fuels, and we have pretty good records of how much that is. We know that carbon dioxide interferes with the transfer of terrestrial (or infrared) radiation much more actively than it interferes with solar energy. That's the "greenhouse effect." In other words, if we increase the carbon dioxide content of the atmosphere, we're still letting much of the sunlight in, but we're trapping much of the outgoing infrared heat. The analogy to the glass in a greenhouse is inexact, but we believe that CO_2 increases in the atmosphere will lead to warming. How much? First, we have to ask how much CO_2 we'll have.

Based on 20 years of measurements we know that the carbon dioxide has been increasing roughly so that half the amount of CO_2 released to the air from burning fossil fuel can be accounted for as an increase in CO_2 concentration in the atmosphere, and the other half must, thus, be going somewhere else. The major "somewheres" that people have postulated are either into the oceans or the biosphere, and this has led to a grand debate. A number of biologists, most notably George Woodwell from Woods Hole, have argued that the biosphere can't be expanding; if anything, it's shrinking. He argues that the deforestation rates in the world, particularly in tropical forests, are perhaps on the order of 1 percent per year. If that's true, since the tropical forests have about the same magnitude of CO_2 tied up in the wood as there is in the atmosphere, it means that the destruction of 1 percent of the forests should be putting just about as much CO_2 into the atmosphere each year as industrial processes. And if that's the case, we don't know where it's going because it should have been building up at a faster rate in the atmosphere, since the ocean chemists can't imagine that more than about half of the *fossil fuel* CO_2 emissions could be taken up by the oceans.

So there are still large uncertainties in these projections. However, it is interesting to look at Lester Machta's 1971 projection for CO_2 increase (left, below), which shows about a 15 percent increase, to 375 parts per million, by the year 2000 over the roughly 320 parts per million observation of CO_2 in 1970. This has been criticized as being based on little data, and thus it could be wrong. We obviously can't base a strategy of industrial development on this kind of sloppy model, it has been argued. Yet, when more data were added (right, below), his projection



Projections of atmospheric carbon dioxide concentration from fossil fuels. The 1971 estimate on the left shows 375 parts per million (ppm) by the year 2000, while the updated 1974 model on the right predicts a CO_2 concentration of almost 390 ppm—proving that not all "doomsday" predictions are overestimates.

increased from 375 to nearly 390 parts per million! Uncertainty doesn't mean one has overestimated a possible effect.

How much stock are we going to put in such a projection? Are we going to redirect our energy policy to phase out fossil fuels? Are we going to change the derivatives of industrial growth based on such a model? This is where value judgments come in, because if we turn these models over to a panel of climatologists for a decision, they'll probably say, "Oh God! All these uncertainties! We don't want society to make a mistake because of us." But whether the mistake is paying too much or too little attention to projections with large uncertainties has to be based on your own value judgments about how you view the quality of life.

I could see those judgments being very different in a country with an inelastic income from one with an elastic income. We in the United States might consider our present energy use sufficient so as not to take the chance of dealing with the CO_2 scenario coming from models like Machta's. On the other hand, a poor nation like India might consider that since energy is a major source of scarce food, then it's worth it to them to risk the CO_2 problem. I think the troublesome issue is that it may well take the physical scientists as long to provide some reasonable certainty to the political system over these climatic problems as it would take the real world to "perform the experiment" of telling us whether our present theories provide projections that are too high or low.

I'm fully convinced that we face this dilemma with CO_2, because the CO_2 effect on climate should jump up out of the climatic noise level in the next decade or two, according to our climate models, and time will tell us whether the models are right if it happens. Will that event move the grain belt three or four hundred miles north, possibly drying out parts of the high plains or the California mountains? I don't know—but it is quite possible. We think a warming might improve the monsoons if it happened, and a grain belt displaced to the north would open agriculture at the northern end. But will there be fluctuations along the way?

These are the kinds of questions we have to address, recognizing that the people who are threatened are those at the margins of the circulation systems, and those at the margins of the nutritional requirements—for whom any further stress is fatal. I think that really is the outline of the climate message. □

My Life As A Hired Gun

A Techer's Life in Politics

by JOHN ANDELIN, BS '55, PhD '67

There was a time when I was a scientist, and I knew my field better than most other people. That is no longer true. In moving from a narrow technical discipline to a broad-based political one, I've been forced to become what I would call a "generalist" if I were flattering myself, or a "superficialist" if I were trying to denigrate myself. The time demands put upon congressional staff are so great that we are constantly wishing we had time to know something better.

Let me try to tell you about Congress. I had the chance several months ago to do that for some foreign newsmen, so I decided to start with the basics. I got a blackboard, drew a box on it, and put the word "Congress" inside. I put two boxes down below, and I wrote "House" in one and "Senate" in the other. Then I realized I was wrong. The House and Senate aren't parts of a unified body known as "Congress." They *are* the Congress.

The two houses have to agree exactly on a piece of legislation or it's a standoff—and a standoff means that nothing happens. Making a positive decision by the whole Congress is frequently impossible. As a body, it does not lead: It follows, critiques, and restructures. Standoffs are the rule more often than not.

A key to understanding the Congress is the realization that it is the only body in our government that explicitly recognizes and encourages politics and special vested interests. In Congress, you are automatically a Democrat or a Republican, and you belong to this or that or some other grouping politically, and you're proud of it. You represent the district that is dominated by particular companies, or the wilderness, or logging camps, or some such combination; you will defend those parochial interests because you have been elected to defend them.

The details of the two houses are different, but I'm most familiar with the House, so I'll try to explain that. In the House there are several hundred small, independent businesses, each headed by a Congressman. Congressmen, just like shoe salesmen and many franchises, have exclusive territorial rights to the district assigned to each of them. That district may go to some other Congressman in the future if he doesn't do well enough now, but nobody can speak for him as long as he is that district's elected representative.

Congressmen are very sensitive about crossing state or district lines. They represent their own constituents. One of the major functions they and their staffs perform is that of answering phones and being a pen pal. They are also ombudsmen when someone at home has a problem with an agency or a local government entity. Congressmen must either write back sympathetically to their troubled constituents, or better yet, must take some action to alleviate their problems. If, for example, a business is trying to get a grant from some department and you as a Congressman or his staff think the business is being mistreated—not on the substance of its proposal, which you don't usually judge, but on the mechanism by which the proposal is being evaluated—then without any question you interfere. You say, "Damn it all, set up an appointment with this guy. He wants to see so and so, and you're giving him the run-around. Let's get this going."

Above and beyond serving and responding to their constituents, Senators and Representatives must vote on national issues. To the extent that they have national interests or aspirations, they will serve a broader community than their own district. (A Senator always worries about his state; a Senator who wants to be President worries about the whole nation—and acts accordingly.)

Another form of small business in the Congress emerges in the commitees. Obviously, with more than 500 Congressmen and Senators, there's no way to deal with a complex issue and let everyone have his say, so the Congress is broken up in little pieces called committees, many of which are, by themselves, too large. Each member serves on one or more committees, many of which schedule concurrent meetings. There's still no way to run the system with efficiency and complete responsibility.

Constituents seem to expect Congressmen to be in their offices at all times, day or night; and they also expect them to be on the floor of the House or Senate at all times, whether anything interesting is going on or not (even if it's 10 or 15 minutes just for roll call); and they are also expected to be in all committee meetings. Members actually go off to private meetings together to do much of the substantive work, but they try to go to the full committee meetings when something important is going to happen, or when they know enough about an issue to make a significant contribution.

Every Congressman worries about all issues—veterans, old people, environment, defense, and so on—but committees deal with narrow issues only. To some extent, because the committee chairmen are so strong, the committee members are heavily influenced by the interests of the chairman. Usually there are enough pressures on these senior members so that, on the average, they do not abuse this power.

In the long run, chairmen come and go, one state gets power and then another does. Somehow, the genuine perceived interests of the nation seem to get through the system eventually. In the short run, however, aberrations occur.

The results of the action on any given bill, and possibly even the action of a single Congress on a given issue, can be heavily distorted by someone who is in a position of power, or someone who is very good at the game. The game is to know the rules of procedure on the floor and in committee. You can almost never force anything through, but if you're very good at those very complicated rules, you can almost always stop something from happening—in the short run. This works for everything from the Presidential vetoes down to, in some sense, staff vetoes.

Staff, that's me. I'm too busy to do everything, so I have to do things in some order. Whose order? Mine—until somebody complains. At that stage, I'll pull something out from the bottom of the pile and work on it, but meantime I've been a bottleneck.

I'm working in the Congress partly because I want to understand it and partly because of specific measures I'd like to see enacted—or not enacted. Just like me, anybody in the system can sometimes be a bottleneck. Part of my role is the removal or illumination of other bottlenecks. If somebody's sitting on a bill, it's usually obvious. Sometimes they only need to be reminded of that to get it moving; sometimes that's not enough. If it's an important bill, there's usually some vested interest group to push it. If I think it's responsible to do so, I find that vested interest group and encourage them to help push the bill through the system.

Neither the system nor the actors are perfect. One problem is a serious mismatch between the duration of the problems to be solved and the time allowed for their solution. Most major societal problems are measured in decades if not generations, and the span of interest in solving them by members of our government is limited—two years for the Congressmen, four for the President, six for a Senator (unless he's running for the Presidency), and one for the Office of Management and Budget. Tell a Congressman about a problem that really isn't going to be solved in less than a couple of decades of concerted and, possibly, painful effort, and he'll have to say, "That's too bad, but won't the first steps have a negative effect on my next election?" Those members who don't consider that won't be around two years later to work on the next step of this 20-year effort.

Members of Congress are somewhat like hockey goalies in being remembered for their mistakes. Somewhere else in the books you may notice they've done a lot of good things, but constituents seem particularly sensitive to mistakes. Their dominant attitude is, "What have you done for me lately?"

The congressional system is very well designed to make decisions based on questions and problems for which there is no rational decision, and possibly not even any responsible one. People say, "The environment is dirty. Let's clean it up." So here comes a bill to clean up the environment. It does something like banning strip mining. Is that bill good or bad? There's no way in the world that we can measure the specific effect of that bill—even if everything else would stand still for the next five years. No one can guess what other changes will take place that will affect the situation one way or another. Data are not only inadequate, they really aren't very meaningful in many cases. But the bill is there because somebody wants to stop or start something. Unfortunately, the members can't walk up with their electronic voting cards and find the "I don't know yet" slot. They do the best they can, and put it in the "Yes" or "No" slot. It's done.

We go through this legislative ritual, and there's a final vote—and the losers don't go out trashing in the streets. There are no riots, no bombing. They say, "Well, the process was fair. *My* taxes go up, but what the hell, we'll get *them* next time." And so we routinely get decisions on tough issues, and the losers, literally, gamely walk away from it. Industries have been put out of business by federal legislation, but they don't destroy society because of it, because the process is the fairest we've been able to come up with. That's the Congress—decision-making in the face of terrible uncertainty, done by a bunch of individual businessmen and committees with parochial interests.

I fit into this process in a funny way. I now work for one of those committees, but I used to work for one of the members, Mike McCormack, Democrat from Washington State. I met him largely by accident, but it turned out that he was, at that time, the only scientifically trained Congressman and therefore we had something of a common language. We could evaluate each other fairly quickly. It didn't taken long to work out mutual trust. I learned what he did and didn't do, he learned about me and accepted me, and we worked and argued together quite effectively. About a year ago I switched from the Congressman's staff to work for the Committee on Science and Technology as a science consultant and subcommittee staff director.

Working for Mike McCormack, I worried about everything in general plus what he worried about specifically because of his expertise and interest in energy policy. So I spent five years dealing with energy policy from a member's office, where I also had to be aware of what his concerns would be should something impinge on the state of Washington.

Working for a committee is very different. Instead of worrying about everything, I just worry about what falls under the jurisdiction of the Committee on Science and Technology—which includes NASA, NSF, and used to include ERDA. Now the committee worries about the Department of Energy—partly. The distinction is that ERDA was just concerned with R&D; the Department of Energy deals with R&D and regulatory and financial aspects of energy. Therefore, its charter is larger than the jurisdiction of our committee. This means that responsibility is shared with other committees. Where that line is drawn eventually will depend on how aggressive the committee chairmen are and how the Department of Energy is constructed in detail.

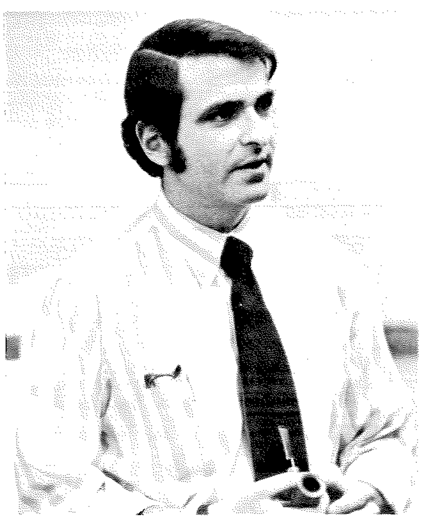
By and large, I guess I do two different things. First, I'm an information broker. It's pathetic how much time I spend on the phone—getting information in and turning around and getting it out. I do my best to act with integrity and to keep the members informed of whatever they're concerned about so that somehow programs move forward. It's an awkward business because we are buried in information. I get 30 pounds of mail a week, in addition to 40-50 calls a day. If I take five minutes on a phone call, that's about five hours a day, plus the mail, plus three to four hours a day of meetings.

continued on page 29

The Italian Connection

by DAVID L. GOODSTEIN

A Caltech physicist finds an unexpected island of tranquility in the tumultuous sea of Italian university life



David Goodstein, professor of physics and applied physics at Caltech, made his first trip to Italy in 1967, when he spent a postdoctoral year at the University of Rome as a National Science Foundation Fellow. That was the year (February 1968) when Italian students at the University carried out the first of the "occupations" that have plagued Italian universities ever since.

On a number of subsequent trips to Italy, as a visiting scientist at the Frascati National Laboratory, Goodstein has had an opportunity to observe the continued erosion of the Italian university system. Last summer, at the Free University of Trent, he saw the first ray of hope for the future. This account of his discovery has been adapted from an article prepared for an Italian journal.

In the tiny village of Povo on the hillside above Trent, a lovely little city in the Italian Dolomites, there is a building—converted from a hotel school—that houses the Institute of Mathematics and Physics for the Free University of Trent. It is a small operation: 10 full professors, perhaps 60 other postgraduate academic personnel, and 500 students. Begun in 1972, its newness is obvious in the clean, bare walls, the still uncluttered and unfinished laboratories. One can sense an air of seriousness and purpose in the classrooms, in the library, and in the laboratories. To the visitor it seems an unexpected island of tranquility in the tumultuous sea of Italian university life. Its existence is so surprising that it prompts speculation on its prospects in the Italian university system.

The Italian system, like Italy itself, staggers under the weight of its traditions, but is also sustained by them. With virtually no visible means of support, Italy's balance sheets perennially spell disaster. Viewed from the outside, it seems a country forever poised at the edge of chaos, but chaos never quite comes. There is something in the Italian genius that cannot be accounted for on the balance sheets.

There are, of course, real problems both for Italy and for her universities. Ten years ago, the Institute of Physics at the University of Rome was similar to the physics department at any good American state university. There were differences, certainly, such as the small number of full professors and the excessive power concentrated in their hands, but learning went on, and young people came up through the system, learned to be physicists, and produced scientific research that was usually competent, sometimes brilliant.

But those years were the now famous 1960s, and life was not to remain tranquil. In February of 1968, to apply pressure on behalf of legislative university reform, the Institute of Physics was "occupied" by a group of students, with sympathetic support from many of the younger postgraduate personnel.

Such "occupations" were not unprecedented at Rome. In fact, like everything else in Italy, they had a history that carried with it traditions, rituals, even courtesies. The student occupiers, meeting under a banner that proclaimed "Down with Bureaucracy," soon organized a bureaucracy every bit as subtle and complicated as the one they had replaced. It was decided that the occupation would stop all teaching activities, but not research. These were, after all, physics students, who knew the value of research. Thus one could enter and leave the building on research business but not on didactic business. Needless to say, decisions were appealed, meetings were called, influence was used. It was all very Italian, the scenario for a funny movie.

During the ensuing ten years of sporadic occupations, disruption of the normal routine has become the normal routine. The students, or nonstudents, who formed the nucleus of the protesters seemed to become, as time went on, increasingly stubborn and intransigent, and less sensitive to those traditions and courtesies that made the earlier occupations seem almost friendly. Now nearly everyone has grown tired of the situation, and the professors and their assistants seem less afraid of being called (to put it delicately) insufficiently leftist if they stand up to the protesters.

In the meantime, a provisional university reform law has been passed. Academic salaries and the number of professorships have been increased drastically. In many cases, the personal lives and prospects of academics below the level of professor have brightened considerably. Learning and research, some of it very good, have continued to go on. Not all of the problems have been solved of course. Professors in outlying universities often prefer the more stimulating atmosphere of the principal cities, and consequently seldom visit their putative places of employment. Moreover, the best of the Italian scientists, those with international reputations, have tended increasingly to do their research abroad, although they have also tended not to break entirely their ties in Italy.

Today, however, there is a problem more serious than reform of the academic profession. A university degree in Italy today has become practically useless for the purpose of finding a job. One reason may be the degraded quality of the degree itself; employers sometimes ask applicants whether they received their laureates before or after 1967. My own theory is that the situation, like many of Italy's problems, has something noble in its cause—and is nearly hopeless in its effect.

Italy is now in the course of becoming the first major country in Western Europe to switch from a system of elite higher education to a system of mass higher education. The fraction of young people between 18 and 22 who are university students has long been much higher in Italy than in England, France, or Germany, and it has increased rapidly in the last few years. The same change, to mass higher education, took place earlier in the United States, where now more than 50 percent of those between 18 and 22 are college or university students.

The trouble is that Italy is making this noble change without the immense economic power and industrial base that allowed the United States to absorb all of those graduates, not perfectly, but reasonably well. Italian students who come from a modest family background and go to the university do not do so because they want to become a part of the educated masses. They go to the university to become part of the elite, and Italy cannot yet afford to have an elite that consists of half or a quarter of her population.

There is another difference between Italian and American higher education. Italy's unified national system lacks the resilience and flexibility of America's complex, pluralistic system. There are 200,000 students who want to attend a university in the city of Rome. Rome has one university, built for about 30,000 students. Nevertheless, there are 200,000 students at the University of Rome. It is simple, inexorable, disastrous.

All of which brings us back to the Free University of Trent. It is not (yet) a part of the national system; it is financed and run by the provincial government of Trent. All of the professors, all of the postgraduate personnel, all of the students in the Mathematics and Physics Institute are there full time, every day. The research tends to be applied rather than pure (some of it is financed by Fiat, the giant automobile manufacturer). It is just possible that the students who write theses and graduate in physics will turn out to have marketable skills.

The applied nature of the research arises out of a decision made when the new faculty was instituted to specialize in material sciences. Physics research elsewhere in Italy is relentlessly pure, and in fact, physicists and engineers in Italy practically form separate camps, with some animosity and little communication between them. Sometimes engineering faculties in Italy have their own professors of physics and physics courses so that their students need not be contaminated by contact with pure physicists.

At Trent it was decided, instead, that the new science faculty would be closely associated with a two-year pre-engineering program and its growing faculty. The engineers that emerged would absorb a bit of the scientific spirit, and in return, the scientists would do research rooted in the real world. A typical example of work going on there now is a study of why and how ion bombardment of metallic surfaces leads to improved corrosion resistance.

Very probably the Free University of Trent will soon become part of the national system, since it is now straining at the modest limits to which the province can afford to finance it. But some organizational steps have already been taken to protect the autonomy and independence of the science faculty in the event the university becomes national. Given good will, clever maneuvering, and a lot of luck, it could just possibly survive with its distinct identity intact.

Four hundred years ago, the Council of Trent launched the Counter Reformation, and thereby changed the history of the world. Nothing happening today in Trent will have so great an impact. To be sure, graduates of the new university could be influential in spite of their small number, which is even less than the number of undergraduates at Caltech. But that brave little group of students and their teachers at Trent can hardly serve as a model to solve the massive problems at Rome and Naples. Instead, Trent could prove by example that a new beginning, an independent course, is still possible in Italy today. That would be worth quite a lot. □

An Acceptable Nuclear Future . . . *continued from page 7*

Strengthened security. In a world beset by terrorism, we are confronted with unsatisfactory alternatives—terrorism with few constraints on individual rights, or order with considerable restraints. Yet I believe we can have order without losing freedom *on a large scale*. We can cope with terrorism, sabotage, and diversion by strengthening security at vulnerable spots, without having to impose repressive measures upon the entire society. Thus, physically isolated nuclear sites lend themselves to being made secure by virtue of their isolation. But this may not be enough. We will probably have to reimpose on our nuclear plants the same kind of security we imposed on them during the war. In effect, we will have to buy order at the expense of freedom, as has been the case throughout man's history, but we confine these encroachments on freedom to 5,000 square miles and perhaps 200,000 people—the cadre that operates the nuclear plants.

Professionalization of the nuclear cadre. In the final analysis, the safety and integrity of the nuclear system will depend on the caliber of the staff that mans the system. If one concedes that the nuclear system is special, because a malfunction could cause very great harm, and of a kind that our society has not become accustomed to (and may never become fully comfortable with), then it follows that a higher degree of professionalism and dedication is required to manage and operate the nuclear system than is required for the conventional power system. The responsibility borne by the superintendent of a nuclear power plant is at least as great as that borne by the pilot of a 747. This must be recognized—by the superintendent, by his management, and by the public.

I believe this sense of professionalism is enhanced by the siting scheme that I propose. I draw this inference from our experience at the existing large nuclear sites—Hanford, Oak Ridge, Idaho Falls, Savannah River. Each of these places, somewhat isolated, somewhat self-contained, tends to create a cadre, a sense of professionalism and of commitment, that is possibly less easy to create in smaller sites that give less scope for a cadre of critical size to develop.

Establishment of nuclear generation consortia. When nuclear reactors were first introduced, they were viewed simply as replacements for conventional boilers. This was a mistake. The nuclear system is a far more complex and demanding enterprise than is a coal-fired boiler. The responsibility inherent in and the complexity of a nuclear system, particularly a breeder, go much beyond that involved in a fossil-fuel system.

The breeder system, with its intrinsically closer coupling between chemical reprocessing and reactor operation, places demands on the utility industry that go even further beyond that industry's tradition. Indeed, the split between chemical reprocessing and reactor operation inherent in the ceramic-fueled LMFBR was to a degree encouraged by the utility industries' disinclination to go into the chemical reprocessing business. But it is by no means clear that ceramic-fueled LMFBRs will always dominate the nuclear enterprise. As the technology is rationalized, other breeder systems in which chemical reprocessing and reactor operation are more closely coupled could well come into being.

It would seem therefore that in the ultimate system the generation of nuclear electricity, as contrasted with its distribution, ought to be placed in the hands of utilities that are specifically geared for this job. A siting policy such as we have enunciated would lend itself to just such separation. Generation would be the responsibility of the company or consortium or government entity established specifically to carry out this task. Consortia of this sort already operate the nuclear power plants in New England, and governmental generating entities such as the Power Authority of the State of New York or the Tennessee Valley Authority are also examples.

The nine existing Reliability Councils in the U.S. conceivably could serve as nuclei for the creation of such generating consortia. However, our purpose here is not to restructure the electrical utility industry in order to rationalize the distribution of electricity; it is to create entities equal to the responsibility imposed by the *generation* of nuclear energy.

Not the least of these responsibilities is the handling of a serious accident, even one that does little or no harm to the public. One can never simply abandon a nuclear plant, least of all one that has suffered a meltdown. Yet what assurance do we have now that the utilities operating reactors are robust enough to withstand the stress imposed by an accident; and if a utility goes bankrupt, is the nuclear plant assured the resources needed to keep it from causing damage? Thus, along with the restructuring I speak of, I would contemplate some means of preventing responsibility from lapsing, in the event an accident causes bankruptcy.

Longevity of institutions responsible for nuclear energy. The nuclear system, once started, can hardly be abandoned. Who, for example, is responsible for cleaning up once a nuclear fuel chemical plant has gone out of business? This residual responsibility is not unique to nuclear energy. Abandoned strip mines continue to cause acid drainage, and no one can be held responsible. The difference perhaps is that we know in advance that a nuclear plant cannot simply be abandoned; the same knowledge was not so apparent in the case of acid mine drainage.

We have already recognized that waste disposal, which involves some surveillance over a long time, must be a governmental responsibility—as much as anything because of all our institutions, it is the government which is longest-lived. Probably other elements of the nuclear system will also demand such longevity and possibly stronger governmental involvement. At the very least, in the ultimate system we must be assured that whatever entity is responsible for the operation of the reactors is likely to remain viable as long as the reactors contain appreciable amounts of radioactivity.

THE ACCEPTABILITY OF PHASE I AND THE TRANSITION TO PHASE II

The requirements we lay down for an acceptable Phase II are stringent: physical isolation and collocation, strengthened security, professionalization of the cadre, establishment of *generating consortia*, and longevity of the operating entities. These requirements seem to me necessary if the ultimate nuclear energy system is as large as 500 or 1,000 breeders in the United States, and ten times that many in the world; and if the system is to be with us for an indefinitely long time.

How much of this is required to make Phase I acceptable? I would argue, not very much, because Phase I is limited in size and duration. Again, assuming the 3.5 million tons for the raw material in the United States, we compute one expected meltdown in all of Phase I. And it is reasonable to expect that the incremental improvements in the technology of safety will reduce this expected number. The likelihood is that Phase I will pass without any serious meltdown. The same argument holds with respect to the other issues. Because Phase I is limited, incremental improvements in security, in professionalization of the cadre, and in ensuring financial responsibility ought to be sufficient.

On the other hand, it seems to me that Phase I ought to develop along paths that smooth the transition to an acceptable Phase II. Of major import is the siting policy. Ought we not establish the sites for nuclear energy now, and adopt the principle that only sites so committed, most of which will be occupied first by LWRs, will be used for the breeders; and that we shall keep the number of such sites to a minimum? We may achieve this policy de facto simply because it is becoming so hard to license new sites. C. Burwell estimates that 80 of the 100 existing sites could be expanded into large centers.

Would it not be prudent to confine the future nuclear system, including the rest of Phase I, essentially to the existing sites, and to adopt this as our national policy? Reprocessing and fabrication facilities should be collocated —all seem to agree on this. I would go further and urge that chemical complexes be collocated with existing nuclear sites. Though I believe eventually *all* such reprocessing complexes ought to be collocated with breeders, I am prepared to leave this question open for the time being. Simply do not put new complexes anywhere except in places where nuclear energy centers already exist or are planned.

The key question with respect to Phase II is the rate at which it develops, and the speed with which breeders are introduced. The original plan for introduction of the breeder as outlined in the 1962 White Paper of the Atomic Energy Commission (AEC) was predicated upon a much faster growth of electricity than we now consider plausible—an electrical demand of 70 q by 2000 and doubling every 15 to 25 years thereafter. No wonder the U.S. drive toward fast deployment of the breeder seemed so obviously sound.

But more recent estimates of our demand for electricity are much more modest—for example, at the Institute for Energy Analysis we now project demand for nuclear electricity of 300 to 400 GWe (18 to 24 q) by 2000. And A. M. Perry and M. J. Ohanian at IEA have estimated that a total nuclear demand of 400 GWe could be achieved by 2000 even if the first commercial LMFBR were not deployed until the year 2000, and the total uranium resources were only 1.8 million tons. This relatively leisurely introduction of breeders would limit our total nuclear capacity of 400 GWe until around 2020. By then the system could resume growth as old breeders fed fissile material into new ones. In the intervening years one would presumably depend on additional lower grade ore, should demand exceed 400 GWe.

Although most of us in the nuclear enterprise have always believed that the breeder is the essence of nuclear energy, we could never mount a completely compelling argument for introducing it very quickly, except the one based on an extremely rapid and most unlikely rise in energy demand, or a very much smaller uranium ore reserve than we now consider likely. In the absence of such a strong demand or lower reserve our argument for *quick* deployment fell back on economics—we believed breeders would be cheaper than burners, in which case the market would force rapid deployment of breeders.

Eventually this will be the case; but it is not at all clear when, since neither the future price of uranium nor the capital cost of the breeder is known. To be sure, if breeders instead of burners were now deployed, most of the questions concerning separative work capacity and uranium ore would disappear—the nuclear system would have fewer links, and therefore would be less subject to total failure as a result of failure of a single link. Though this cannot be looked upon as a completely compelling argument for rapid deployment of the breeder, it reinforces this fundamental fact: *Nuclear energy based on breeders is much less beset by uncertainties related to our energy demand, our ore reserve, or our separative work capacity than is nuclear energy based on burners.* This, in final analysis, is the strongest argument for fast rather than slow deployment of breeders.

PROLIFERATION—THE BOGEYMAN

President Carter's new look at nuclear energy, particularly his deferral of recycling in LWRs and his deferral of LMFBRs, has been largely motivated by his concerns over proliferation. It is curious that technologists tend to look upon proliferation as a political problem amenable only to political solutions, whereas politicians regard the problem as amenable to technological fixes. Hence the President's call for development of proliferation-resistant reactors. History has come full circle; 31 years ago Dean Acheson and David Lilienthal proposed a plan for control of nuclear energy that depended heavily on a technological fix, the division of nuclear activities into dangerous and non-dangerous ones. The technological key to this distinction was the possibility of extracting power from denatured fissile materials, i.e., isotopic mixtures of ^{233}U, ^{235}U, and ^{238}U, or (as was then somewhat mistakenly believed) ^{239}Pu ^{240}Pu. Thus nuclear activities could be separated into dangerous and non-dangerous ones, the former being supervised by an international authority, the latter being conducted under national auspices. Though a special committee set up by General Groves warned that denaturing could not be foolproof, the Acheson-Lilienthal plan nevertheless relied on such approaches.

Today we again seem to be casting about for a technological solution to proliferation based on denaturing. Now it is certainly true that the world community of reactor developers has never considered building in resistance to proliferation as a design constraint. I suspect that schemes such as Molten Salt Breeder Reactors (MSBRs) fueled with ^{233}U/^{238}U mixtures are more proliferation-resistant than are reactors that use pure ^{239}Pu; and I believe it is useful for the reactor community to examine reactor systems that incorporate technical barriers to proliferation, or at least weaken the link between power and bombs.

But it is clear that institutional arrangements must be part of any proliferation-resistant system, and this certainly was anticipated in the Acheson-Lilienthal ideas for an International Atomic Energy Authority. I would argue that the policy of collocated nuclear energy centers espoused here could itself strengthen resistance to proliferation even without a full-fledged international authority. The point is that such a siting policy, which confines nuclear energy to relatively few large sites with a minimum of transport of fissile material, is in the first place easier to inspect by the instruments of the present International Atomic Energy Agency; and it lends itself to *resident* inspectors who could insinuate themselves into the local situations and detect unauthorized activities much more readily than could non-resident inspectors. Indeed, I should think that once the principle of resident inspection is adopted, the IAEA would have taken an important step toward becoming the sort of international entity conceived in the Acheson-Lilienthal plan.

Let me put the pieces of what I think is an acceptable nuclear future together. My basic point is that if we can devise an acceptable long-range system—that is, Phase II—then much of the opposition to the much smaller and limited Phase I ought to gradually subside, without either a dismantling of Phase I or a rejection of Phase II. The basic elements of Phase II are physical isolation (and therefore collocated energy centers and resident IAEA inspection); heavier security; professionalization of the cadre; longevity of the operating entities; and restructuring of the nuclear energy system, with the establishment of consortia for the generation of nuclear power. In preparation for the transition to Phase II, I would urge that any new reactors, whether breeders or LWRs, be confined essentially to existing sites. I also urge further strengthening of the nuclear cadre during Phase I, and re-examination of reactor systems with a view to hardening them against proliferation.

It must be noted that these proposals are aimed more at improving the safety, rather than the proliferation-resistance, of the nuclear system. Nevertheless collocation should enhance the proliferation-resistance as well as the safety of the nuclear system. Resident inspection is more feasible if collocated energy center sites are adopted. A nation bent on milking its nuclear power plants of weapons materials would almost surely have first to expel its resident inspectors. This would be tantamount to publicly and explicitly going nuclear, in much the same sense that Egypt's expulsion of UN observers from the Sinai signaled its intention to go to war. Moreover, a collocated system—for example, a closely coupled breeder and chemical plant (like EBR-II or the Molten Salt Reactor)—would inherently be more proliferation-resistant and diversion-proof than would a dispersed system since the fuel need never be fully decontaminated.

Is it likely that suggestions such as these will quiet the concerns over nuclear energy enough to establish a "consensual climate" in which the regulatory process can work during Phase I? At the moment I would judge that these proposals do not go far enough for the antis, and go too far for the pros. The situation is ripe for imaginative, constructive thinking. I look on these proposals as tentative first steps—indeed, as an invitation for those who are interested to suggest means of remedying the ills of the nuclear business. Consensual climates in a democracy are not easy to forge, especially when the issues are bitter and important. Yet, unless we establish such a climate, we run a danger of losing the nuclear option. I believe the burden we would thereby impose on our descendants is much heavier than the one they would have to bear in managing an acceptable nuclear future. □

My Life As A Hired Gun . . . *continued from page 23*

Now, you notice I haven't done anything with all this information; I've just received and disseminated it. If that's all I did, I'd be called an Amplifier or a Distributor, but I also summarize data, filter it, analyze, and validate it. I also have to sort the mail, write some letters (and occasional bills), concoct new ideas, assign things to staff, and try to keep track of what they're doing because they do the real work. (I've got three or four technical people working for me, and somehow I'm choreographing for them.) That's the information-brokerage aspect of my job.

The other thing I do is to go out and implement and expedite things. That's the "Hired Gun" approach. There's a bill lying around somewhere, for example, and a decision is made to make it go. People like me make it go. We structure the required hearings and get the hearing records put together. We try to do a balanced job, but I suspect that there may be issues that I don't balance the way someone else would. Most of us do as well as we can, but it's not simple. There are always too many people who want to talk about the issue, and you can usually look at them and know what they want to talk about. For example, someone wants to talk about his company, another has a perpetual motion machine and wants funding; this one has a sound argument, but it's exactly the same as that of someone else who's already testified.

Eventually, you get this thick volume of testimony and statements for the record. Then you take all that information, add in all the data and biases of the members who work with you on your subcommittee, and do what's called "marking up the bill." You take the bill as submitted, put in everything else that should be there, and try to put it all together in such a way that it's politically acceptable—that is, it will pass.

Out of 24,000 bills introduced in the last Congress, about 700 passed, most of which were continuations of programs already in existence, relatively inconsequential changes in the law, or private bills. What it comes down to is that, in any given Congress, there are only a few really new and significant bills.

We've been really lucky on my committee; we've passed several of them. We have a couple of solar energy bills to our credit—the only solar legislation that exists—a geothermal bill, an electric vehicle bill, an energy extension service bill, and loan guarantee provisions for fuel supplies. I'm one of the key staff on some of those bills and the critical staff on others, but it's so much a team process I'd hate to guess whose ideas were where in the operation.

I talk all the time to administration officials, congressional staff, outside experts, international folks, citizens in general, lobbyists of all sorts, and the press. It makes no difference. My office is open; that's my ethic—and for all I know, it's the law. People are calling up all the time, and sometimes they want to come in and tell me about something. Most of the time I just say, "Thank you," and incorporate what they have to say into myself.

Even with all this input (or maybe because of it) I don't know half what I ought to know at a given time. Sometimes I'm given 15 seconds to analyze a problem and give an answer, sometimes a month or two. But in that month or two I get only a day or two to think—and even that isn't like what I used to do at Caltech. That was really thinking. Now I just try to pull together enough to be able to believe what I'm saying.

The 15-second occasion really happened very early in my career. I was asked about a bill that had to do with medicine, and I said, "Look, I'm a low-temperature physicist, and medicine is pretty far removed." The Congressman said, "Should I ask you or the elevator operator?" I said, "I've got an opinion," and I gave it to him, and he went and voted. That man is no longer in Congress, but it was probably one of my more difficult decisions, because when he went out to vote, two or three others from his state followed his lead.

Having opinions is very easy; I always have opinions. Unfortunately, having them is very much easier than acting on them—facing up to the fact that something is about to be enacted into law that won't easily get rescinded and actually will affect the lives of real people. I'm still not considered a conservative by any means, in terms of what I think can and can't be done, but my attitude is now different than it used to be in theoretical political action discussions.

My physical presence in Washington is important. I don't mean *mine* specifically, but that of a representative of science and technology. To many members of Congress, a scientist is their family MD, and most don't distinguish my background from that of an engineer or a biologist. They also don't necessarily know whether a question is or is not technical. In fact, many times they think they're asking a technical question, and it would be easy to couch an answer in technical terms, to hide behind it. But you have to say, "No. The technology is insignificant. That's really going to take a political decision."

We don't have the answers to the CO_2 problem, for example, or the particulate one. Is the world really going to warm up if we use fossil fuels? I'll be happy to give you an answer in 20 or 30 years, but we're voting on it today. So I say, "Here's what we know; here's what we don't know. Given that, do you like coal better than nuclear, or don't you? How much do you want to shift our life styles to use something else if it's not economically competitive?"

So the physical presence of a scientist is important, which surprised me. But serving as congressional staff brings on problems—very personal ones. One is anonymity. It's hard to have to give one of your good ideas to someone else. He gets the credit if it was a good idea. You feel good because you were right, but damn it all, I was used to publishing under my own name in my previous life. Worse is when someone takes your good idea and garbles it. Then if it's accepted, it's a killer. And if it's shot down, you can never offer it again, because people think they've already heard it. You can't correct it, and that guy certainly can't, so you have to start over again.

Another difficult part of the job is that we're really shooting not only at moving targets but at shifting deadlines. On the authorization for the Department of Energy, we were ready to go to the floor for the vote in mid-May. We actually voted on it in October, but ever since mid-May we have repeatedly been told, "Next week." Throughout that period we have had to be ready with the latest amendments, opponents, proponents, and arguments pro and con. We have had statements ready so members could put the material in the Congressional Record to make plain the rest of the legislative history. (The bill isn't always enough; anytime there's ambiguity in interpreting a bill, explanations of what the bill means are looked for in accompanying reports, associated debates, and the like.) Unfortunately, three weeks later the arguments or the players change, so you do it all again. A week later, the President makes a statement; do it again. A week later, something else changes, etc. You have to keep readjusting as the deadlines move.

Finally, we got a deadline for the next week—but the next *day* we were on the floor. They jumped us the wrong way. This time we were ready, but if we hadn't done the work, it could have been a mess. It's a little like the difference between an undergraduate and a graduate or professional career. As an undergraduate you may not be ready when it's final exam time, but it's very hard to convince anyone that the exam ought to be postponed for your benefit. When you're a graduate student or professional doing research, you pretty much do it at your own pace, and you don't publish until it's good, regardless of the pressures. In the Congress I don't have that freedom. I publish, so to speak, when the train comes by, when the vote is on the floor, when the speech is due, regardless of the quality. I think my record is a good one; I would defend it. But damned if it isn't unsatisfying to feel that it's just OK, rather than signed, sealed, and sent off to the publisher as the paper I *want* to publish. Nowadays I just hope I didn't put the pages in the wrong order.

You can probably guess why I do this. In spite of my complaints, I have a lot of fun, and it's very satisfying. My original reasons had to do with the broader aspects of science policy, but I think most people in the technical community do not readily understand that their financial and educational support does not come from government because science is important as an elegant, intellectual achievement of mankind. It comes because of the expectation that something good is going to come out of it when the product comes back. The only exception to that attitude that I've noticed is when we back our technological achievements for purposes of international prestige. So any administration, any Congress, supports science policy, educational funding, and basic research because they think they're getting their money's worth.

The science policy part of my job is to determine why we support science at the federal level, how we do it, and whether we do it well enough. Right now we're trying to establish a framework that shows that basic research pays off. My own support for it is not only because it pays off. I think research is important for other reasons—that intellectual achievement by itself is important, as long as everybody isn't working at that and leaving nobody tilling the fields. But if I can make the payoff argument honestly, I'm not unwilling to use it.

As I mentioned, the other thing I've been up to my ears in is energy policy. I'm sorry to say that, because it's still such a mess. So it's obvious I don't have a whole lot of effect on the way the country is run. But first and foremost, energy policy is mostly a political or societal question. It's not a physical resource question. We've got, if we're willing to use it, coal for several generations; and if we're going to breeder technology, we've got lots of uranium. Of course, both those technologies are less environmentally benign than we would like, and the nuclear one has awkward aspects of being associated, through some of its technology, with nuclear weapons. This means that if we expand nuclear to use breeders and reprocessing, we have to be very careful that the institutions work right or we may also be increasing the opportunities to acquire illicit nuclear weapons.

Those two technologies alone—imperfect as they are—would sustain our society at a growing economic level at prices not much different from today's. If you don't mind nuclear wastes, weapons proliferation, CO_2, assorted carcinogens, and all of the mining damage—which are political decisions based on the desirability of various tradeoffs—we have no energy problems and won't have for centuries. In that case, there's no point worrying because, long before that time, fusion or solar technology will be economically feasible by today's standards; or because we will have learned to use energy much more efficiently, "economic feasibility" will have taken on a new meaning.

If we spent money properly (and this is *my* definition of properly), we'd spend a lot more money on solar energy than we're doing now. We'd run a lot more risks than we do now, and a lot of experiments would be failures. But we're way behind, and we ought to go after it aggressively. The worst that would happen is that we'd just create a kind of technological job corps. If it works, it will speed things up and we'll have technologies that are more comfortable to live with than coal or nuclear.

Energy policy is what I've done for six years. We put out a task force report in 1971 that said: We need a Department of Energy, the energy problem needs to be discussed in both physical and political terms, energy conservation and environmental protection are critical, and the R&D budget is much too small. Six years later there's a Department of Energy and the R&D budget has gone from $400 million in 1971 to $4 billion today, so our task force report is no longer worth much, but it feels nice to have said it. After publishing our report, we found out there was another task force report almost ten years before that said the same things, but maybe better. So we sort of rediscovered gravity, but it still felt good. And it got attention.

Most of the energy legislation passed and still in process is, in some sense, just fiddling with the details. If our institutions were put together better, we wouldn't have had the energy crisis this way. We would have been discussing options, life styles, technological complexity versus simplicity of operation, the centralized versus dispersed operation, and so on. We still aren't doing that. I have to worry about details, but I'm really basically worried about institutions. I think they need to be changed.

A desirable long-range goal, it seems to me, is to establish a just and sustainable society, which is hard because everybody has a different definition of justice, and sustainable is a long way from coal and nuclear. But that's the sort of goal I'm working toward. I think it's an exciting and worthwhile challenge. It's certainly not the technology I grew up with, which I also think is exciting. In no way do I feel that I may not be back doing some kind of research in science at some point, but now I'm doing this other societal interaction. And I find it fun and satisfying, but a hell of a problem with a lot of frustration because nothing's going fast enough. □

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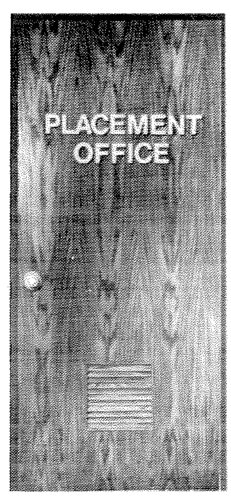

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