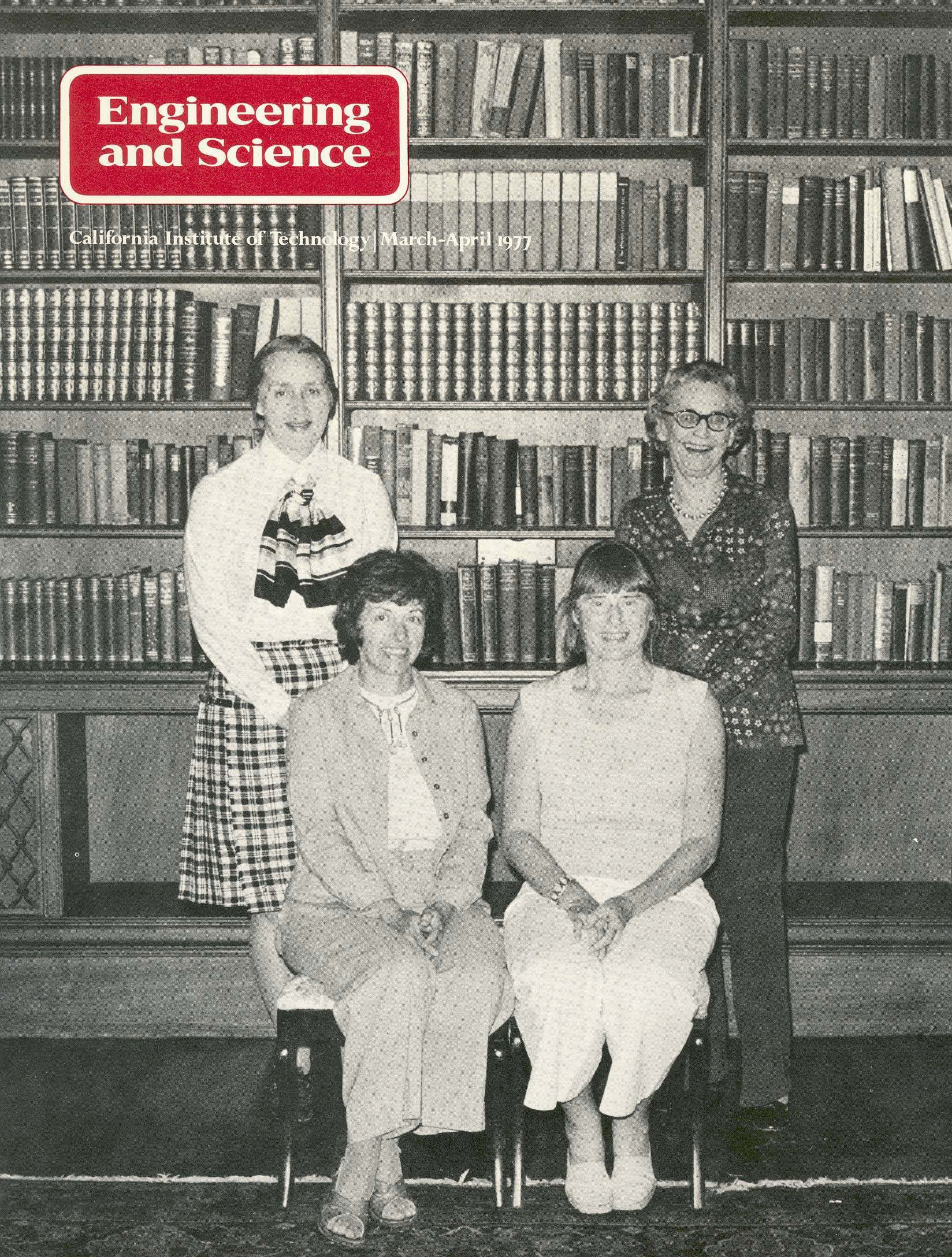
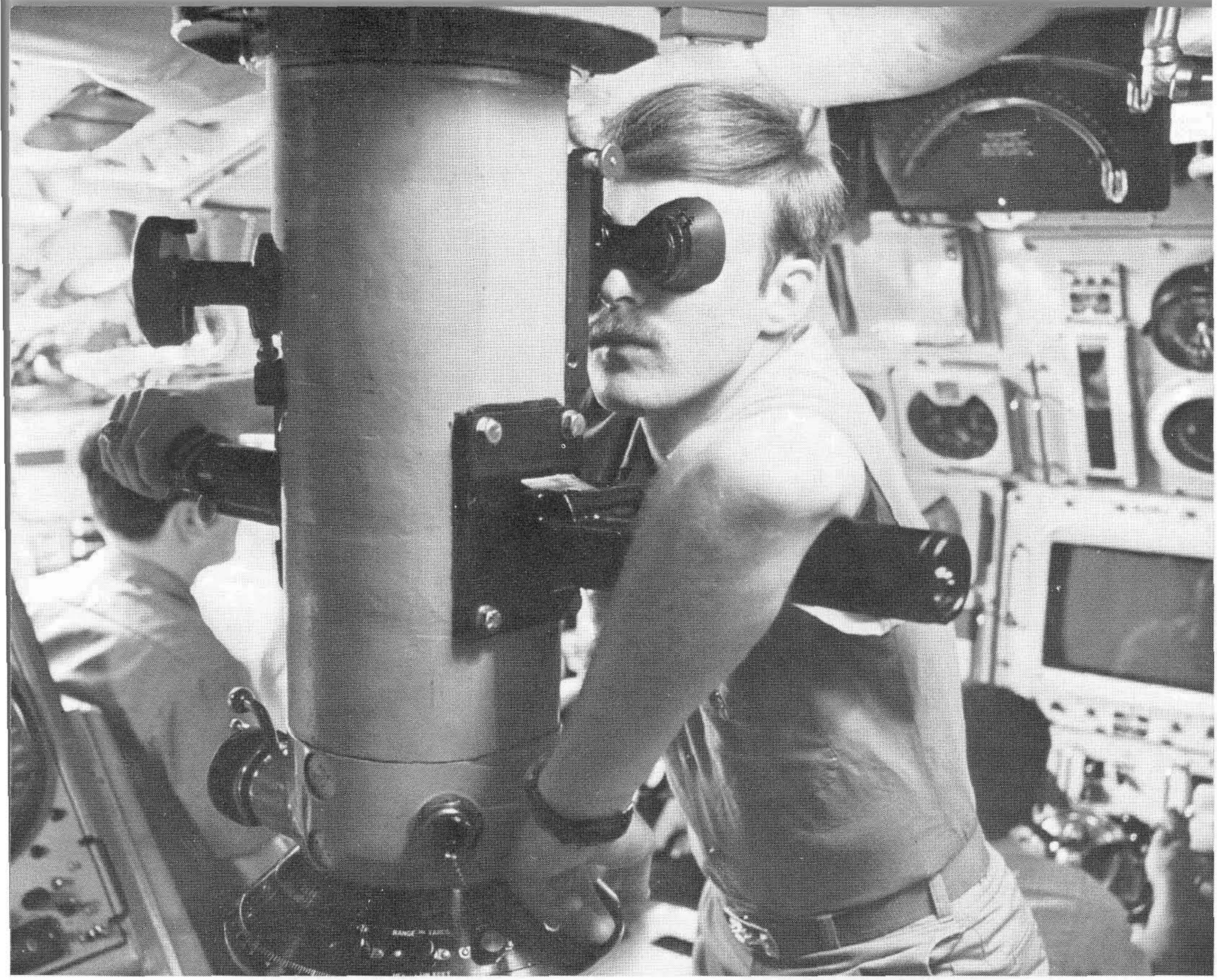
Engineering and Science

California Institute of Technology | March-April 1977





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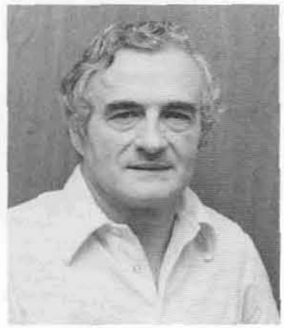
March April 1977/Volume XL/Number 3

In This Issue

Wives' Lives
On the cover — the four members of a very special group in the Caltech community, the wives of the faculty Nobel Laureates. Earlier this year the Caltech Y arranged a program in which graduate students' wives were given an opportunity to meet and talk with two of the Nobel wives about what life is like for the wives and families of such notable scientists. Only Margaret Gell-Mann and Manny Delbruck were able to be present on that occasion, and they are shown below on either side of Eileen Hahn, who acted as chairman of the meeting. The questions and their responses turned out to be so fascinating that *E&S* asked the same questions of the other two — Lorraine Anderson and Gweneth Feynman.

"Maybe You Know All About Him — But What About Her?" on page 13 is our adapted and enlarged version of those interviews and your opportunity to meet these ladies — shown on our cover from left to right, standing, Margaret Gell-Mann and Lorraine Anderson; seated, Gweneth Feynman and Manny Delbruck.

Who's In Charge?
Bruce Murray, professor of planetary science at Caltech, has a distinguished record of vision and achievement as a scientist — two qualities that are now standing him in good stead in his new position as director of the Jet Propulsion Laboratory. But Murray is not a man to limit his inquiries to his immediate professional concerns, and thus he has increasingly applied those same qualities to taking a thoughtful look at the challenges of the future. His Watson Lecture on December 8 examined some of man's possibilities for a new world view. "Are We Going To Rule Our Own Technology — Or Will We Be Ruled By It?" on page 3 is adapted from that talk.



Hard Questions
Continuing to ask hard questions about science and public policy must often seem an endless task to the concerned scientist — a situation that removes none of his sense of responsibility. Robert Sinsheimer, professor of biophysics and chairman of the division of biology, has become well known as one of the most thoughtful and articulate spokesmen for accountability in scientific decisions. It is a pleasure for *E&S* to print "Whither Molecular Biology?" on page 29, which is adapted from a speech he made recently at the Argonne Symposium.

Straight Talk
One of the unofficial but valuable talents for seismologists is the ability and willingness to explain to the public what's happening in the earth — insofar as possible. Caltech is fortunate to have a number of such articulate scientists, and recently radio station KPFK interviewed three of them for the benefit of its listeners. Clarence Allen, Don Anderson, and Hiroo Kanamori responded to questions by John Kotick and Diane Moye on the subject of "Predicting Earthquakes," and on page 10 *E&S* presents an adaptation of that interview.



STAFF: *Editor and Business Manager* – Edward Hutchings Jr.
Managing Editor – Jacquelyn Bonner
Photographer – Floyd Clark

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Are We Going To Rule Our Own Technology–

Or Will We Be Ruled By It?

by BRUCE MURRAY

THE IDEAS of science have had a tremendous effect on the human race. Especially in the Western societies, they have eroded man's ties with tradition by undermining the sometimes traditional bases for authority and morality. It is therefore true that, as we go through these difficult centuries — and our children and children's children go through what is obviously going to be some great climax of the human drama — that science must become an integral part of the world view of all human beings. Somehow there will have to be a reconciliation between the feelings of man and the facts of science — a harmonizing of rationality and morality.

It is also true that the handmaiden of science — technology — has completely changed almost every aspect of human and social activity, from birth to death. New technology is needed to help create living patterns that reconcile material needs with the psychological structures all of us inherit and possess. Somehow there must be a harmonizing of body and soul.

It must be apparent that I consider the present to be unprecedented. Man has nearly saturated the surface of this planet, and that transition to saturation is taking place in an extremely brief time. Along with this has come an enormous growth of economic activity, unprecedented interdependency of human beings, and increase in scale of political, economic, and social institutions. There is no precedent in history for this, and many contemporary problems basically derive from our mega-economic, political, and social structure.

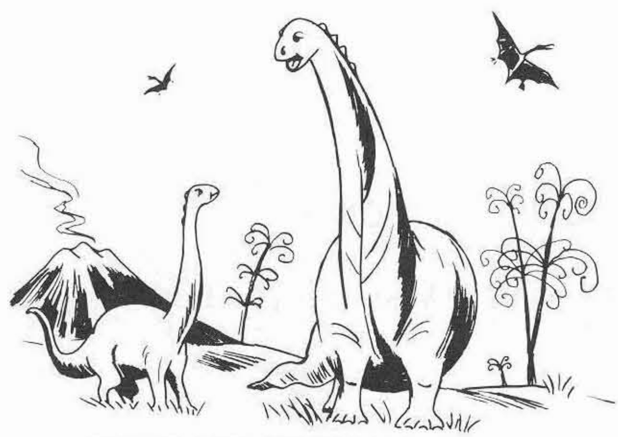
Man the toolmaker still exists (computers, for example, are just very sophisticated tools). But he has constructed more and more elaborate tools — we call it technology — and he has now reached the point where he is either going to be the master or the slave of those tools. But along with this technology has come an ever-increasing division of labor, more specialization — and enormously more interconnectedness.

As the scale of economic enterprises has grown to global proportions, the individual feels more and more isolated. He is in every sense — socially, economically, politically — part of larger and larger structures, and therefore he feels less and less a sense of community. He feels unable to influence his own destiny.

The irony of this fantastic growth — and affluence in the industrialized countries — is that the mighty have become vulnerable to the humble. The wealthiest people in New York are hostage to the vagaries of the teamsters and the garbage collectors. In Socialist countries, where the strike is forbidden by repressive measures, the response is the sullen neglect of quality — passive resistance of a different kind. Furthermore, the rich and the poor, the powerful and the weak, become subject to ever more penetrating governmental interaction, which is deemed necessary to keep the global mega-economic system from breaking down. First comes propaganda in which the citizens are persuaded to believe that what exists is really good and they should be happy and appreciate the opportunities they have. When that doesn't work, repression is used, and if *that* doesn't work, there's even the prospect of behavioral engineering: Change the human being to better fit this mega-economic structure.

Needless to say, this kind of accelerated growth has produced a tremendous reaction against scale — against anything big, whether it's government, economic enterprises, cities, or universities.

There is also a specific reaction against technology because it is perceived that technology is integrally related to these mega structures; therefore it must be somehow responsible. So, if one could simply kill technology by putting a wooden stake through its heart, maybe the rest of the problems would go away.



"LOOK KID, WE'RE AWARE OF THE PROBLEMS BESETTING OUR SOCIETY. WE'RE WORKING ON THEM"

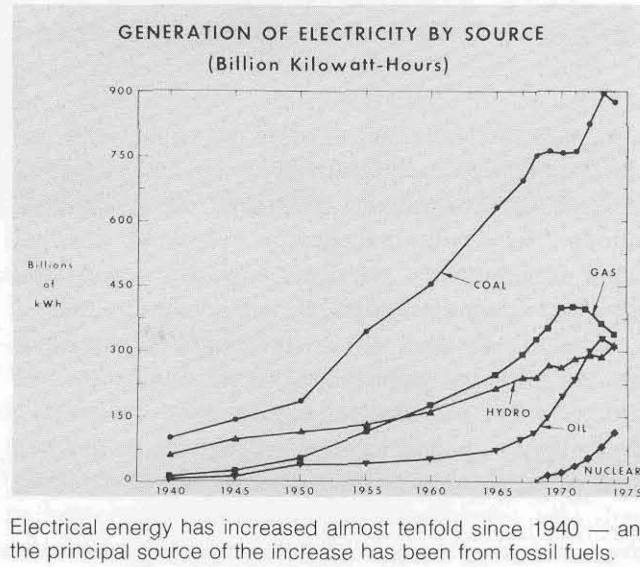
The antiscale attitude bubbled to the surface of national and international consciousness several years ago with a book by an MIT group called *Limits to Growth,* which presents the thesis that there are finite limits to resources and to what environmental burdens can be maintained. Hence, it is argued that the world is in imminent (about 50 years) danger of going into a situation where adequate natural and human resources will not be available. In addition, overpopulation will fatally damage the ability of the planet to sustain itself. The counter argument against this doomsday view is that technology will always come through with a substitute that will take care of the problem. Thus, the debate between the technological optimists and pessimists continues.

Another, and I think more perceptive, attack on scale is a book by E. F. Schumacher called *Small Is Beautiful,* in which he argues most effectively for the benefits of a conscious policy of decentralization and regionalization. I have come to find this view attractive.

Representing a fringe beyond Schumacher is Ivan Illich, a very thoughtful person who has provoked considerable controversy by attacking such institutions as the educational and medical systems in this country, claiming that they are obsolete and no longer serve their original purposes. In his 1973 book, *Tools of Conviviality,* he develops the concept of a "radical monopoly." For example, he points out that in an earlier period, in order to travel between where one lived and worked, one could walk, ride a horse, or take a wagon (if you were rich enough). When cars were first introduced, there was an additional choice as well. Now, if you live in southern California, there is only one choice — the automobile and, usually, the freeway. That is worse than economic monopoly; it is a limitation on ways to live your life. So even though we are richer, and better off in many ways than our grandfathers, we actually have more limited choices of how we do some basic things — including transportation.

Beyond people like Illich, who are attacking the institutional base of the country but still are intellectually credible, there are violent revolutionaries — real anarchists — who have decided that the only way to cope with this enormous problem of scale is to destroy "the system." Incidentally, there's another kind of anarchist — the "establishment anarchist." These are the people who, even though they are political figures, are running against big government. Consider four names from the recent presidential election: Reagan, Carter, Ford, and Brown. They had one thing in common — they all ran against the government! How can a professional politician be against his own institution? But they were. This is a political manifestation of the reaction against scale.

In considering the growth of antitechnology, I can note that, within my professional lifetime, the scientist and engineer has gone from hero to antihero. That's an interesting transition to live through. Congress now has an Office of Technology Assessment, which is a political statement that not all technology is necessarily good, and that we must try to anticipate the consequences of new technology. This pattern was certainly apparent in the debate that developed in the recent election in California over Proposition 15, an initiative to slow down or stop altogether the development of nuclear power. Proposition 15 was novel. It concerned technology, not civil rights. It was negative, because it would have restrained one form of technology (in this



Electrical energy has increased almost tenfold since 1940 — and the principal source of the increase has been from fossil fuels.

case nuclear) without offering alternative means of providing new energy or reducing the demand for it. The fact that it lost is irrelevant; that it was such a political issue is significant.

Will there be a change? Will there be a shift away from the path we seem to be on? I would argue, yes. Not just because people sense the need for it, but because the large-scale economic system we have is built upon shifting shoals — the shoals of cheap oil. Cheap oil is going to run out, and that is going to drive us away from the overcentralized mega-economic society.

Since 1940 in this country there has been an increase of almost tenfold in the use of electrical energy. The principal source of this increase has been the combustion of oil especially to run power plants.

JPL, incidentally, has some work going on to understand how to extract coal more economically and perhaps how to process it more efficiently to convert it into useful products. And we have just recently started a program to try to understand some of the consequences of waste disposal from nuclear power plants.

The interesting thing about this enormous increase in the consumption of oil is that, under normal economic conditions, the price of the product would be expected to increase during the same period of time. But in 1947-48 the price of oil was actually slightly *higher* than in 1972, if compared in constant dollars. Oil actually became cheaper — until the energy crisis of 1973, when the price of imported oil quadrupled.

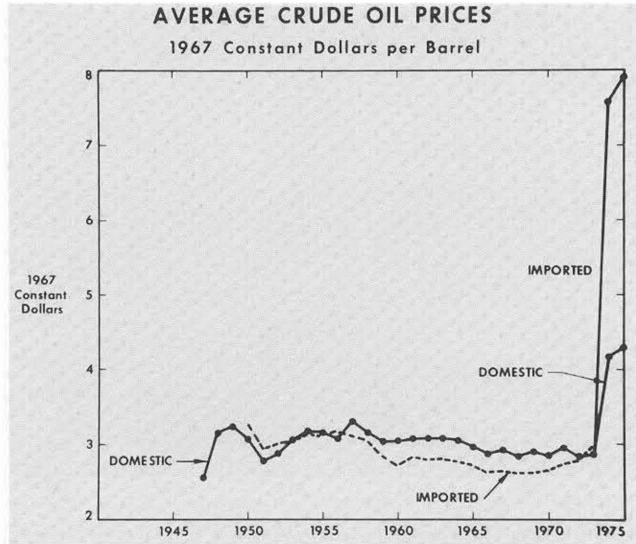
Obviously, the price of oil should have been rising as consumption increased. But it remained low until it finally broke loose. Had it been coming up steadily all those years, it's unlikely that we would have developed such an enormous dependence on it. So in many ways the recent jump can be regarded as necessary and long overdue.

What are the consequences of this relentless increase in oil consumption? World production has been growing in order to support that tremendous increase; U.S. production, on the other hand, peaked in 1970, and we have been pulling all the oil we possibly could out of the ground since then. Having passed the peak in domestic oil production, there is no way to reverse that process. As a consequence we have imported more and more oil. Despite the fact that prices have jumped up and the outflow of capital is enormous, this country has still not been able to come to terms with that fact.

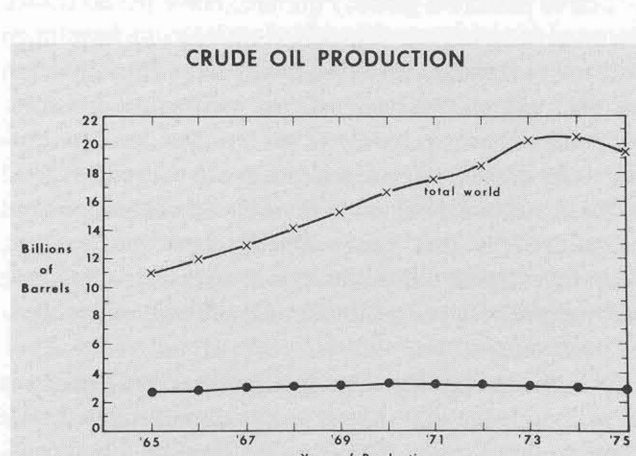
Where does this lead us? If pessimistic estimates are right, world oil production will peak a little before the year 2000; if optimistic estimates are right, it will occur slightly after the year 2000. The difference is slight. The fact is that the whole world will have to make a fundamental transition in about 25 years — at the time when babies now being born are becoming parents. Then the world's oil production will start declining. But it will become much more costly long before then; yet even so, we are still increasing our consumption of oil. This is a reckless course that can't lead anywhere but to a crisis of proportions difficult to exaggerate.

Regardless of whether alternative sources of energy are developed, we are going to run out of oil, and that will cause the economy and our way of doing business to change. Most importantly, the way we transport people and things will change, because that process really depends on oil.

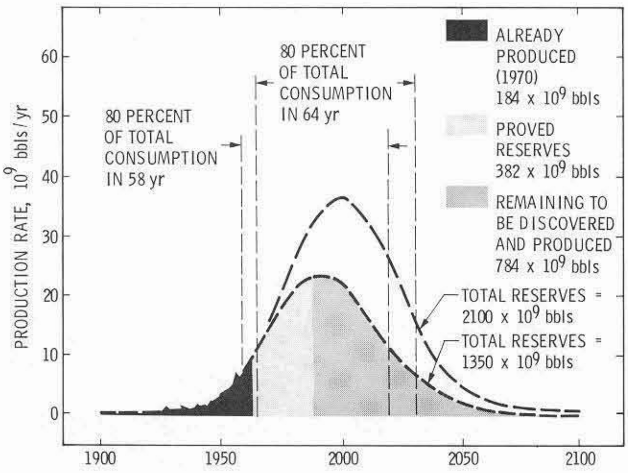
JPL is trying to develop a good program in electric vehicles for this reason. There is no well-established



Despite the enormous increase in the consumption of oil and energy, the price of oil actually became cheaper, in constant dollars, until the energy crisis of 1973.



U.S. oil production has passed its peak (1970), and there is no way to reverse this trend.



We are still increasing our consumption of oil, even though world oil production will start declining around the year 2000.

industry in this area, but it seems inevitable that electric vehicles will have to provide a substantial part of our personal transport. What one does about jumbo jet liners is another matter. You can't fly those planes on batteries.

Looking back at these turbulent times, archeologists of the future may well regard us — the people who lived in the latter half of the 20th century — as "the planetary exploration culture." We were the first in the whole of human history to explore the nearby planets and the moon. But these same archeologists, when they're listing things in their equivalent of the *Encyclopedia Britannica,* will also note that our society guzzled up virtually the entire world's supply of oil in an incredibly brief period of time, without any evident concern for the consequences to ourselves, much less to our descendants. Those two things, in my mind, will stand out most sharply from this century.

I have painted a gloomy picture. Have we no choice but to give up high-unit productivity? Do we have to go to a lower standard of living to get away from it? High productivity is what has made our intellectual and artistic achievements possible (a society that has low productivity doesn't have much to leave to the future, good or bad). Are we destined to revert to an endless peasant existence? Is this confrontation between energy, economics, and the environment irreconcilable? Are we trapped between an intolerable present and an irrelevant future?

I think the way out of such pessimistic projections is to acknowledge that the present is unprecedented. We cannot circumscribe the future by limiting it in accordance with our perceptions of the past. Somehow we have to acquire fleet, faint glimpses about what can be novel and different about the future. Only in this way can we begin to discern some alternatives to what otherwise is a terrible dilemma. So I am going to try to describe some of those fleeting glimpses of the future — more as possibilities than predictions — to illustrate why I think things can be very different.

The first step is to think about the concept of regionalization — about making economic patterns work in one place without worrying about whether identical patterns will work for the whole country. A good example is geothermal energy development. This form of energy occurs in only a few places; furthermore, much of the available energy is not transportable, because it is in the form of low-grade heat. So, if its value is considered only in terms of the value of electricity that can be produced and pumped into a central grid system for wide distribution, it is not competitive with what is still relatively cheap oil. However, if the low-grade heat can be used in the same area as the geothermal deposits are found, the economics change. This has happened in Iceland and New Zealand, both pretty cold places where it is easy to find a use for the heat. If adequate energy simply isn't available, local geothermal deposits may become "economic" despite the cost comparison.

Another difficulty with geothermal energy is that in many deposits a very salty, hot brine is released, which, if spilled into the water table, can do great environmental harm. So the best thing to do, though it costs a great deal of money, is to recycle the brine back into the deposit and make it a closed system and just extract the heat and some of the pressure energy that's involved.

Why should environmental impact be given priority in new geothermal deposits when we don't seem to give it much priority in many other cases? The answer to that rhetorical question is very important. When a resource is of a regional nature and is used locally, the people who use it have to face the consequences of the environmental impact of developing it. That's not a bad way to have to frame complex decisions. Contrast this possibility with California's Proposition 15, which, in an uncharitable way, could have been phrased: "Should we have nuclear energy in California where the environmental effects, if any, will affect the people receiving the electricty, or should we instead have more coal-fired plants out in the Navajo Indian Reservations where there aren't many voters and we users will not be directly troubled by the ash and other kinds of deleterious fallout associated with large-scale coal power plants?"

There is an interesting lesson here — that regionalization tends to force a much more equitable comparison between environmental and economic issues. It provides a better basis for making those kinds of trade-offs.

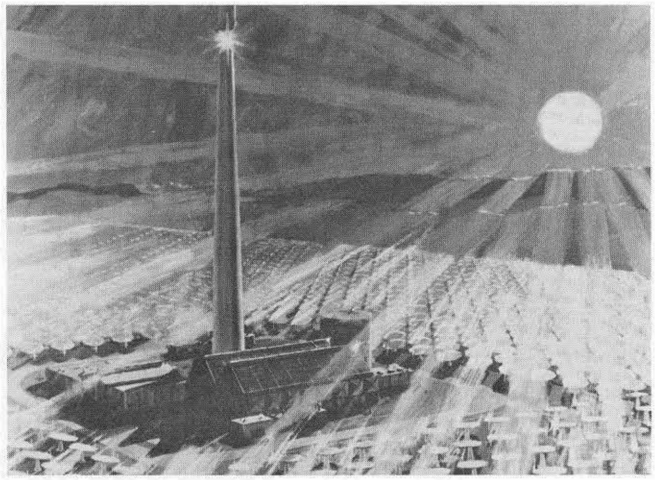
Geothermal energy is only locally available in such places as the Salton Sea, the Geysers area of northern California, and the eastern Sierra. Though it does not offer much for the country as a whole, it is still an interesting prospect for California, and JPL is trying to help in the technology and planning of that.

The real solution to energy and material needs still must involve renewable resources, which — sooner or later — the U.S. and the rest of the world must come to depend upon. A society that recycles its sources of energy and raw materials must develop. The problem is that such resources are intrinsically dispersed. Oil, coal, and uranium, for example, are highly concentrated in their natural states. One can build a power plant to extract the energy in one place and transport it widely as electricity. But solar energy, in contrast, is dispersed all over the globe. It's very difficult to concentrate it appreciably.

Sewage waste, which ultimately must become a source of basic chemicals, is as dispersed as the population. One can hardly imagine taking all the sewage, putting it in tank cars, and carrying it to the Four Corners, where huge treatment plants would extract all its goodies. If it is ever going to be useful, waste recycling will have to be done in a distributed way, in tens of thousands of municipalities throughout the country.

When we consider how technology can help us utilize our renewable resources, we must first realize that how we choose to use technology is to a large degree both a political and societal choice. One of the big jobs I see for us as a nation — and in that sense leading the world — is to try consciously to make a choice about where we're going in technology and therefore in economics.

As I said, solar energy is a dispersed resource. One would think that the right way to deal with it is to figure out how to use it regionally. But at the present time much of the major national effort is not in that direction; rather it is intended to collect sunlight in hundreds of dishes and focus the energy to a single point at the top of what's called a power tower. The collected heat will then drive a steam turbine to produce electricity. So it seems to be our national policy to treat new kinds of renewable resources in the same manner as we treat highly concentrated fossil energy deposits. I think we should indeed do this sort of thing, but we should direct a comparable effort at learning how to use solar energy and other resources in a distributed way.

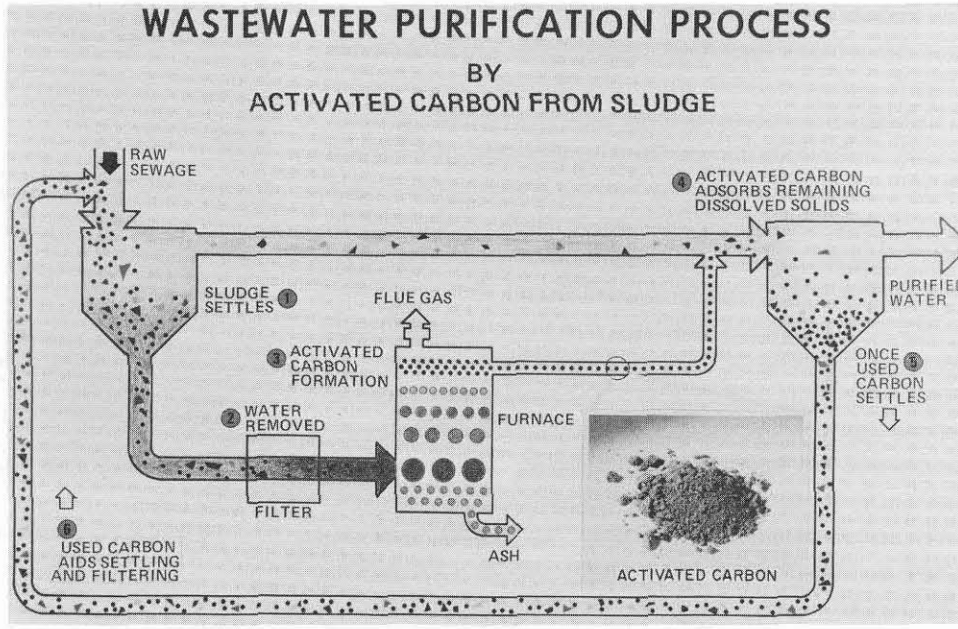


In solar energy, our major national effort is to collect this energy in hundreds of dishes and focus it to a single point at the top of a power tower.

There is another way in which solar energy impacts our society. Natural gas, one of the endangered species of energy, is used in agriculture to dry food products. A substantial amount of energy is consumed in this way. But it is possible to use a solar collector to heat water to a high enough temperature to accomplish the drying, or at least supplement the use of gas for the purpose. This is again a decentralized effort; the energy would be gathered and used in the same locality.

Another idea that's been suggested at JPL has to do with the production of ammonia fertilizer. One of the big problems to be created by the disappearance of oil is that ammonia fertilizer, which is basic to modern agriculture, is made from petroleum. As oil gets more dear, fertilizer gets more dear, and the cost of agriculture shoots up over the whole world. The green revolution is based on an energy *deficit*. It takes more energy to grow food in the green revolution than actually is extracted by eating it. This is another consequence of our binge of guzzling hydrocarbons. A novel approach would be to make ammonia in situ, perhaps in a small farming community, by using a solar energy collector to make electricity, which would be used at the same site to dissociate water into hydrogen and oxygen. The hydrogen then would be reacted with nitrogen in the air to make ammonia. All the constituents of ammonia are contained in air and water, but it takes energy to recombine the molecules. This kind of process could work all year long, though the ammonia normally is used for only perhaps four to six weeks in the planting season, when the fertilizer is put down.

Accumulating the energy chemically throughout the year, and using it only once is a clever idea that would have additional savings, because at present a significant



Orange County has just finished building a million-gallon-a-day plant using this method of processing waste.

limitation to using ammonia fertilizer is the rail transportation system. Since the ammonia is used over large areas at the same time, it is very difficult to supply it properly. If it could be produced where it is to be used, an enormous revolution could take place, perhaps as important in some ways as the green revolution. Obviously this kind of technology, if developed for the U.S., in time would be available for other places in the world.

Another way that modern technology can make an unexpected breakthrough in using renewable resources is in the area of processing waste. JPL has many people who know a lot about burning things, because they grew up in a rocket laboratory. Some years ago these people became interested in a process of sewage treatment that involved taking the sludge, heating it up, and making carbon out of it, then using the activated carbon to filter the water, resulting in a closed-cycle system. Orange County has just finished building a million-gallon-a-day plant using this process.

I've been talking about fairly mundane applications of technology, things that we can see how to do right now without having to solve any mysterious problems in physics or biology. On the other hand, there are great possibilities in what we can dimly see, or, of course, can't even envision. For example, in the case of fertilizer, if mutant strains of bacteria could be developed that were especially successful in fixing nitrogen, then the enormous global demand for synthetic ammonia fertilizer could be reduced. The same thing is true in waste processing. The ideal waste-processing system would involve a bug that loves to eat sewage and produce from it the feedstocks for petrochemicals, and thereby provide an alternate source to oil. It is not an impossibly large further step to consider.

Suppose that the coming oil crisis, and the rejection by society of large centralized economic systems, combine to produce a more dispersed and fragmented country. Does that mean that society will break apart into isolated independent groups? Is this really a pathway back to the Middle Ages — or the Balkanization of the U.S.? And does this mean the end of the tremendous global unity that has been developing in the 20th century? I think the answer is no, because another kind of technology is on the ascendancy — communications and transfer of information.

Communication will grow simply because we have the technology to do it so easily. One example I had personal experience with began in 1965 when Mariner 4, the first probe to get to Mars, flew by and returned data to Earth at 8 bits (or dashes) per second. When Mariner 10 went by Mercury in 1974, not even a decade later, the data came back 10,000 times faster!

Another enormously impressive development to me is the minicomputer, based on a microprocessor in which the guts of a computer are put almost literally on the head of a pin. What this means, for those of you who work in science and technology, is the end of the tyranny of the central computing system. We don't have to work together any more; we can each do it our own way. This result has come about through serendipity, not from any conscious planning by the government. But what it means, even more importantly, is the beginning of an individualized computer tool system.

Almost every sales person in a modern store now is beginning to operate a cash register that is not a cash register but a computer terminal. It not only takes care of financial charges; it also makes an inventory and does other things. The sales person is now a computer operator.

What I see coming is a communicating society in which information is going to be transferred rather than people or materials. A good example is the postal system, a structure that is clearly obsolete. We are not far now from the point where we will no longer physically write on a piece of paper, have it hand-manipulated by different people, have it transferred physically 3,000 miles, then hand-manipulated by people again, and finally read. For people who have a special nostalgia for this process, the old system will be available, but at its true cost, which is substantial. Instead, facsimiles of whatever it is you want to send will be transmitted electronically anywhere in the world. And someday people will look back upon the post office as something left over from the Middle Ages — and certainly from a labor-intensive society.

There is research right now at USC on a concept called telecommuting, the idea of decentralizing employees from their work by having a video link between different employee functions. This is a slow process because it is difficult to learn how to integrate the people and machines really effectively. But eventually it will be done — especially when we run out of oil. It will be a lot easier to transmit pictures of faces speaking than it will be to take the face physically and move it 20 miles, or 100 miles, or 1,000 miles.

So I see us moving into an intrinsically new kind of society, one that is not closely related to past experience. It is not easy to forecast what that will be like. I can imagine that good ideas will spread very rapidly, but the application of the ideas will be local — depending on local conditions, traditions, and attitudes. In my view, in that kind of world a great diversity could be developed and maintained, and the current global pressure toward homogenization might be reversed.

Another conclusion from this is that understanding of science and technology must be dispersed through the whole of society and become part of most individuals' consciousness. The idea of "priests" with special knowledge — like today's doctors and scientists — simply isn't going to stand up. That is a product of an overcentralized system. The whole ethic of a scientist being something special and different is hardly a hundred years old, yet sometimes scientists and laymen both act as though it was given by Moses. It will probably not last another hundred years. The knowledge that scientists have, and the handling of quantitative information, I see becoming very generally applied, and becoming part of the substrata of individuals or small groups.

In this sense I differ very much from Schumacher and Illich, who seem to look at decentralization as going back to simpler things. I see it as going on to more sophisticated technology — more advanced technology leading to a kind of life we don't yet know.

So where will all these technological changes lead? Society must change and evolve. My expectation is that on some time scale the coming communicative society will lead to communication with intelligent societies elsewhere in the galaxy among the billions of stars that are there — if we are not alone. Of course we don't know. But if there are others — if the growth of life and intelligence is a fairly common process in the universe, ultimately we will communicate with them.

What then will be the nature of this world that must emerge from this incredible and unprecedented century in which we are immersed? I see us in a distorted and overstimulated collision between the past and the future. I see us carrying out an epic struggle to rule rather than be ruled by our tools — our technology.

Dimly what I see coming — perhaps colored by optimism — is a world in which quality will grow over quantity, because the limits to growth will be reached. These will be real physical limits, not simply those of inherited ignorance or superstition. But as such limits are reached, the idea of materialism will become less significant; the idea of quality will be restored in a medieval sense to the center of man's vision. The ethics of those living then will be much more frugal and restrained, I think, than ours. As part of that indifference to materialism, I see science and technology becoming internalized by society and by individuals, not institutionalized. I think the present institutions will be looked upon in both bewilderment and amusement by our descendants. And I see a growing trend toward small-scale operations — decentralized, but using technological innovations to achieve them. Certainly if that happens it will accompany a profound change in our traditional economic structure and systems. It is my hope, and my expectation, that machines will extend human capabilities, but not eliminate human functions.

A communicative society will arise that will be characterized by local diversity and innovation but also by intercommunicating global intelligence, and eventually by communication with cosmic intelligence, if there is such. There, in my view, lies our destiny, our direction, our destination — the end of the process of which we are but a part. □

Predicting Earthquakes

Whether earthquakes ever will be predictable is increasingly a matter of public concern. In an effort to bring their listeners a rational viewpoint about it, Diane Moye and John Kotick of radio station KPFK recently interviewed three distinguished seismologists from Caltech: Clarence Allen, professor of geology and geophysics; Don Anderson, director of the Seismological Laboratory and professor of geophysics; and Hiroo Kanamori, professor of geophysics. "Predicting Earthquakes" is adapted from that interview.

Gentlemen, our topic is the prediction of earthquakes, and I'd like to ask you to give us an idea of where we stand in relationship to that possibility.

Clarence Allen: First of all, there is a lot of speculation among seismologists as to where we really do stand in this effort. We've already predicted some earthquakes successfully, in this country and elsewhere. We've also failed on a number of occasions. It's clear we are still in a research stage in this whole program, but I am optimistic that we are heading in the direction of successful, routine prediction of earthquakes at some time in the future. It is hard to say how far off that time is; my own guess is that it's at least ten years away.

Don Anderson: I wouldn't disagree with the general assessment of where we are right now. We are really just starting a major research effort in earthquake prediction, and we have very few results so far and very little data. We know what to do, and how, and where. But once we get our instruments in, how long do we have to wait before there is an earthquake? We will have to have a variety of instruments in a variety of places — and then possibly wait for some long period of time.

We are in a particularly complicated position at the moment with regard to the public because we are just learning how to predict earthquakes. And when we find something interesting or anomalous happening, we don't have enough experience to say for sure that it means there will be an earthquake. The public and, in particular, the media — television, radio, and the newspapers — will have to be very patient and sophisticated in this learning period.

What techniques are used for attempting to predict earthquakes at this point? Are there a variety of schools of thought on the applicable techniques?

Hiroo Kanamori: Well, I don't think there are, technically. I don't think there is any major difference among different groups. The difference is more sociological than technical. In Japan they have been doing extensive work in this area for the last 30 years or so, and they are improving the technique. But most of the information they have has been based upon experimental and paper results. There are still many things to be known theoretically. For example, we know that an earthquake is a very complex structural process, but we still don't know at what stress level an earthquake initiates.

Allen: It's rather interesting, if one looks at programs around the world, that many people are doing similar kinds of things. For example, it was suggested in the Soviet Union some years ago that there might be a systematic change in the velocity with which seismic waves — sound waves — travel through the rocks in the area of an earthquake before the earthquake. And even though that was originally a Soviet suggestion, the method is now being tried in many parts of the world, including China.

Anderson: We're still in the middle of a lot of detailed study; we are accumulating a supply of facts regarding the happening of earthquakes, but we aren't yet at the point where we can correlate them to accurately predict the occurrence of earthquakes.

Is there any way of measuring the forces in a fault in an earthquake zone?

Anderson: Several techniques have been tried, but to get an estimate of the actual stresses in the ground is probably one of the hardest branches of seismology. The upper part of the crust, which is available to us by drilling or by other techniques, is very shallow, heterogeneous, and broken up by faults and joints.

It's not clear that a stress measurement, even if we could make an accurate one near the surface of the earth, would be particularly useful. We are, of course, making attempts. But most earthquakes, particularly in California, initiate at a depth on the order of ten kilometers or so, and it is just out of the question to measure the stress at that depth by drilling.

Earthquakes, themselves, by the shapes of the pulses they radiate, give us some indication of the stresses that are involved. Analyzing these is one of the techniques we are using. At the moment, other techniques are very promising — geodetic techniques such as measuring tilts, strain changes, and elevation of the ground, for example.

Allen: Part of the problem is that we not only need very sophisticated instruments, but we need instruments that are cheap enough so that we can put them out in large numbers over wide areas. Yes, we could drill a hole deep into the earth's crust, perhaps, and put an instrument down there, but we presumably have to have instruments in many locations up and down our fault system. That means that the instruments have to be cheap enough for us to afford a number of them, and easily enough interpreted that we can then analyze the results. Those are requirements that make tiltmeters a particularly encouraging area of investigation. Tiltmeters can now be installed fairly quickly. They're fairly cheap; their results can easily be telemetered to central reception points.

Tell us a little bit about a tiltmeter.

Kanamori: A tiltmeter is an instrument that measures change in the tilt of the ground. If a stress or a force builds up within the earth's crust, the crust deforms, and a part of it might tilt.

Anderson: The simplest way to think of a tiltmeter is to think of the bubble of a carpenter's level. A carpenter's level measures the tilt of the ground. If you build a wall, of course you use a carpenter's level to make sure it's level. If you keep the carpenter's level there, eventually you'll see a change in the tilt.

How do you know whether the tilt is significant in the indication of an impending earthquake?

Allen: That's the whole problem, of course. There are many ways in which you can get tilts. Most obvious of all is some sort of settling of the instrument itself. As a matter of fact, for the first few months — or sometimes even years — after you put any instrument into the ground, it sort of settles and gives you signals that might not have any real significance. And so the problem we face is how to distinguish between the noise and the signals, so to speak. But there have been a sufficient number of rather encouraging results for us to be optimistic. For example, the recent prediction by the U.S. Geological Survey of an earthquake up in the Hollister area — as I understand it — was based upon prior tiltmeter records.

What do you take as the starting level for a tiltmeter? You look at the variation in the angle of the earth's surface in relationship to what? To the position of the earth at the time the tiltmeter is placed? Or to some absolute measuring standard?

Anderson: Actually, you're measuring relative tilts. You install an array of tiltmeters along a fault, and then you look at the changes that occur. Now, if you have a very sensitive tiltmeter and it's installed deep underground, you'll see a daily periodicity of tilt just due to the attraction of the moon and the sun, the tidal variations. You'll also see variations due to changes in weather, such as rainfall and barometric pressure fluctuations. However, if you have a string of these tiltmeters along a fault and three or four of them start to behave erratically or to shift in a common direction — and those that are some distance away do not show any perceptible change — and if you can rule out weather influences, then you're probably seeing something that's truly a tilt in the ground.

Clarence brought up a point that's common not only to tiltmeter observations but to all methods of prediction. There are many phenomena that have been reported as seen before earthquakes. We don't have statistics, however, to know how often these things happen without an earthquake following them.

Allen: As we look back at the record, particularly in places with long histories, such as Japan and China, it's very clear that there are well-documented examples of major earthquakes being preceded by peculiar events that were noted by people at the scene and reliably reported. The changes in ground level, the islands that came up out of the water, say, a few hours or days before an earthquake — there's no question that there have been physical precursors to earthquakes. The whole problem is whether they happen consistently enough so that we can use them as a tool that will allow us to make relatively reliable predictions. Will the tilts that precede one earthquake, for example, be the same kind of tilts that precede another in some other part of the world? And will the effects that precede small earthquakes also precede big earthquakes? After all, it's the big earthquakes that we're really concerned about in terms of saving lives and reducing damage.

Anderson: It's further complicated by the variation in geological strata from place to place. For example, a tilt in southern California, similar perhaps to one in Japan, might not be indicative of the same condition because of the different structure of the earth there.

Kanamori: Actually, there is a fairly large difference in the mechanism of earthquakes in different parts of the world. Some of the earthquakes in Japan are very different from those in California or in South America, and even in Japan there are different kinds of earthquakes. We know there is a difference, but we still don't really understand what it is. The question of what precursory phenomena occur before an earthquake can be answered only if we really understand the physical mechanism of an earthquake.

Anderson: This is where the theoretical studies also come in. Coming from observations, data are, of course, empirical. It's the interpretation of the data and building up a physical model of the earthquake that are absolutely essential. The more data we accumulate on the way the earth changes its tilt before an earthquake, the closer we are to being able to build up a theoretical model for what's happening in the ground. Once we can do that, we can take information from one area and, we hope, utilize it in a different area.

Allen: One question we still haven't resolved is whether — even in a given area — different earthquakes give the same kind of precursors for use in earthquake prediction. For example, we know that the San Fernando earthquake in 1971 was caused by movement on what we call a thrust fault. The mountain block to the north was thrust up and over the valley block of the south. That's very different from the kind of movements that give rise to the earthquakes on the San Andreas fault, where the two blocks move horizontally. And because the mechanisms of these two kinds of earthquakes are very different, it's not yet clear that the precursors will turn out to be the same.

What other techniques are being explored in the prediction of earthquakes? You mentioned the Soviet technique of analyzing the speed of sound waves through particular geological strata. How does that function?

Kanamori: All of these techniques are basically measuring a change in the physical properties of the crust before an earthquake. If the rocks are compressed or squeezed, then the hardness of the rock might change as a function of time. And that change may be reflected in a change in the seismic-wave velocity propagating through the rock. So by measuring seismic-wave velocity very accurately, we may be able to detect a possible change in the stress in the crust. Of course, there are many other techniques, like measuring electrical resistivity, ground water level, and some geochemical properties.

Allen: This change in velocity in seismic waves is not easy to measure accurately, but Hiroo is doing a very interesting experiment on that right here in southern California.

Kanamori: Well, rather extensive earthquake prediction studies were initiated in southern California after the San Fernando earthquake, so it's only five or six years since we started. We are using the many large quarry blasts in southern California as a seismic source. We set up many stations to monitor them. The wave velocity from these blasts, measured very accurately, can vary up to 1/100 of a second along various paths from the point of the blast to the stations. Since we started this experiment, there has been no large earthquake, say exceeding 5.5, so we haven't had any definitive example of the way in which velocity changes preceded an earthquake. But by accumulating precise data from blasts, we can trace a change in the crustal condition as a function of time, and these data might be very useful for predicting an earthquake in the future.

Allen: As a result of Hiroo's studies, we now realize how many quarry blasts there are in southern California every day — something we never really appreciated before. In fact, we find it a little embarrassing to go back and look at some of our old records and discover that we actually thought some of these events in past years were earthquakes.

Anderson: This particular experiment brings up another point. You have to be patient in this kind of science because if there's no earthquake, you're not going to learn anything about precursors, no matter what you measure.

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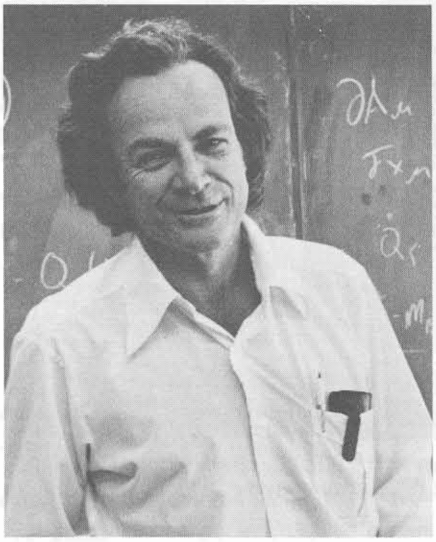
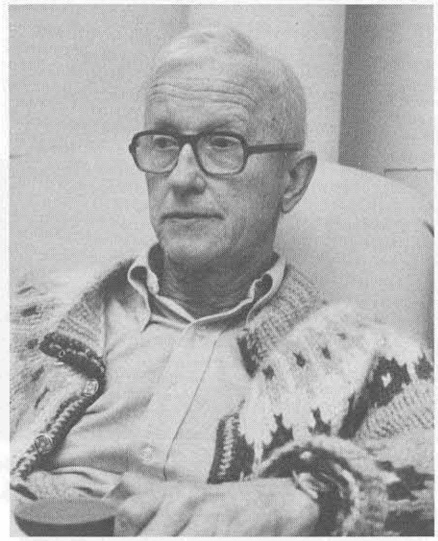
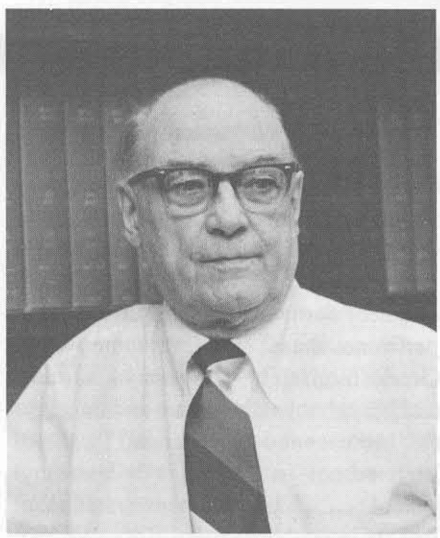
Maybe You Know All About Him—







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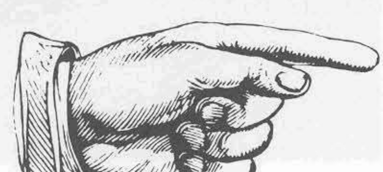
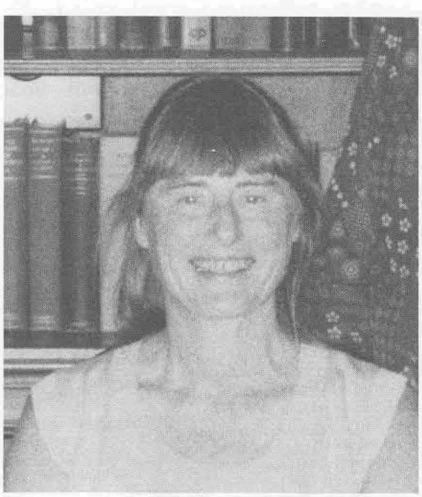


Caltech's Nobel prizewinners in 1970: Carl Anderson (Physics, 1936), Max Delbruck (Physiology and Medicine, 1969), Murray Gell-Mann (Physics, 1969), Richard P. Feynman (Physics, 1965) —at a Caltech faculty dinner honoring the 1969 prizewinners.

The Life of a Nobel Wife

Earlier this winter the Caltech Y presented a program especially designed for graduate students' wives in which they got an opportunity to interview and question the wives of some of Caltech's Nobel prizewinners. (At the moment, there are four – Lorraine Anderson, Manny Delbruck, Gweneth Feynman, and Margaret Gell-Mann.) This article has been adapted from that program. We start with Manny Delbruck.



Manny Delbruck

Manny: I think I'll begin by telling you about myself from childhood on. I grew up on Cyprus Island in the Mediterranean, where my father was a mining engineer and was manager of the copper mines there. Our first home was a Greek monastery. I went to a one-teacher school at the mine, and then my two brothers and my sister and I went to high school in Beirut — a boarding school — which was also small. We had six in my high school class. We had to create our own traditions.

After I graduated from high school, I came to California, partly because the company that owned the mines was located here. I immediately entered Scripps College, which was terrifyingly large to me — 200 students. I went happily through Scripps, and at the end of that time I met my husband, who was a Rockefeller Fellow at Caltech.

As soon as we were married, we went to Nashville, Tennessee, where Max was teaching physics. During that time he taught physics, but gradually he was worming his way over into biology. We had seven quiet years there while he did that, and the war went on, and I did various jobs like being a newspaper reporter and working in a hospital. At the end of that time Max came back to Caltech as a biology professor and stayed here from that time until now.

I was thinking a little about the four of us here, and in many ways we are quite a bit alike. Margaret and Gweneth and I all grew up abroad. We all like to go camping. We all like home life and children. None of us has been hell-bent on a career. We protect and pamper our husbands. We have all been happily married for some time. Gweneth and I even both organized libraries at our childrens' schools.

If any of you have children in school, that's one thing I would like to recommend. Take part in the schools! It's fun and helps you to get into the community and to see people outside of Caltech. I've done that a lot, and I'm still doing it. You'll notice, the children who succeed, and the schools that succeed, are the ones where the parents take part.

We have four children. The oldest boy is 29, and then a girl 27. After a while they seemed to be disappearing, so we had two more children. They are 16 and 14, and they are in public high school in Pasadena and still very much at home.

What did you study at Scripps?

Manny: Humanities. Everybody at Scripps studies humanities, mostly. Besides that I studied social science to the extent that I decided that was something I certainly did not want to go into. I did go on and take a Master's in the History of Art and enjoyed that, but I didn't make a profession of it.

You must be able to use a lot of your education in connection with your travels.

Manny: Yes, we lived in Germany for two years at one period, and at other periods for three or four months. We go to Europe often. Max was born in Berlin. He became American many years ago, but he still has strong ties to Germany, and over the years I have developed strong ties there, too. We all speak German and feel very much at home there.

May I ask what first attracted you to your husband? Was it his extreme intelligence and good looks?

Manny: No. It was one of those things that just grew better as time went on. We met at my German professor's home, and what I remember is that Max read Goethe to me, and I thought, at least life wouldn't be dull. I knew I was taking a chance.

Lorraine Anderson

Lorraine: I remember that I was attracted to Carl because he was very handsome and very intelligent and had brilliant, brown, sparkling eyes. And we had many things in common — Swedish ancestry, tennis, bridge, riding bicycles, and we both liked poetry and he quoted a good deal of it to me.

Tell us some more about you.

Lorraine: OK. When I was in high school, I wrote an essay on "Nobel Prizewinners in Literature." I certainly never knew then that when I grew up, a Nobel prizewinner would ask me to marry him. Let me say quickly that that's not the reason I married Carl. I met him five years before he received his prize, when he was a postdoctoral fellow at Caltech, and with me it was love at first sight.

You grew up in southern California?

Lorraine: West Los Angeles. I was one of a dozen or so of his friends who met him at Burbank Airport when he arrived from New York after his voyage on the Queen Mary, home from the Stockholm ceremonies and festivities. That was in 1936. On an early date he brought me to Pasadena to meet his mother. They lived at the corner of Granite Drive and Lake Avenue. We three had dinner in the Athenaeum, and I was so thrilled. On another of our dates he took me to see his cloud chamber on the third floor of Guggenheim. This was during depression days, when dates were simpler. It was during our engagement that I met Robert and Greta Millikan, and I became very well acquainted with these grand people.

After some time we were married following an eight months' engagement. His mother wasn't well enough to attend a large wedding, so Carl and I decided to get married in Santa Barbara. So, one Sunday after a wedding breakfast my mother and father gave for us and Carl's mother, we went off to Santa Barbara to find a church open and a minister to marry us. I told Carl about two weeks before that we should go up and pick out a church and a minister, but Carl said it wasn't necessary because all the churches were open on Sunday. Well, we discovered all the churches were closed on Sunday afternoons, but after a few phone calls, Carl found a Seventh Day Adventist minister in his church, who was willing to marry us. After that phone call, someone greeted Carl on his way back to the car. When Carl got in, I said, "Ask your friend to come to our wedding." He did, and the man and his wife followed us to the church. That's where I first met Jim Page, who was then chairman of the Caltech Board of Trustees, and his wife, Kate — and a Mrs. Wilson, whom the Pages were trying to entertain that Sunday afternoon. The Pages invited us to their Montecito home after the ceremony for a champagne wedding reception on the balcony overlooking their estate.

By the time we were married we didn't have the money problems that young graduate students have. But when Carl was a graduate student and a young postdoc he had a mother to support, on a tiny income. I had a very good relationship with Carl's mother; I was very fond of her. She taught me to knit and crochet, and we went places together.

We have two children. Our elder son is a mathematician who works for a government agency and lives in Maryland. Our younger son is a physicist and works for Aeroneutronics and lives in Newport Beach.

Carl spent a lot of time with our sons when they were growing up — fishing, swimming, hiking, playing baseball, and most of all, being with them and discussing all kinds of things.

I think this is an interesting thing: Carl is the middle link in a chain of professors and students who won the Nobel Prize in Physics. Carl is Millikan's student, and Donald Glaser was Carl's student. One day while Carl was talking to Don on the phone, he pointed this out and said, "Let's keep the chain going, Don." Don said, "Wait a minute," and after reviewing a list of

Gweneth Feynman

his current graduate students, said, "I'll try, but I'm not at all certain it will be successful."

How about you, Gweneth? When your husband won his Nobel prize and you went to Stockholm with him, what was it like there? What experiences did you have?

Gweneth: Richard didn't want to accept the prize, you know. He really didn't. He just didn't think he could face the ceremony and all the big starched speechmaking.

He just wanted them to put it in an envelope and send it?

Gweneth: Yes. So he was in a terrible state. Before we went and when we got there, I had to tread on eggs all the time. He was a nervous wreck. He had to give a 10-minute acceptance speech, and a scientific lecture. The scientific lecture didn't bother him. He knows what he's talking about. But the acceptance speech — he agonized over that day after day.

He knew ahead of time he'd have to do all these things? Do they send you the schedule?

Gweneth: Yes. He had like a diary with everything written down. I think we were probably there eight or nine days. I enjoyed it. It's not true that Richard didn't enjoy *any* of it. He sort of liked it after it was over — and he loved the thing where they blow the trumpets — these long, long golden trumpets. That was kind of fun. And then the students entertain you. They have a ball, and that he enjoyed very much, because it was very loose and we were dancing all over the place. Afterwards they took us into a beer cellar somewhere and the group sat there and drank beer until six the next morning. Our next appointment was something like 8:30.

We had to be dressed up almost every night so Richard bought a new tuxedo, which was very elegant. I think he's worn it once since. We do get invitations to things I'd very much like to go to, but since Richard will not wear a tuxedo we just don't go, because I refuse to get dressed up when the men are just in suits. No argument, I just say I don't want to go.

Were there any members of either of your families there?

Gweneth: No. Just the two of us. You don't really have time to think very much. They take care of everything. You have your personal attendant, a junior diplomat, who is always married, so the wife took care of me. He's like a sheep dog — he makes sure you get there, where you're supposed to be. You have to go for fittings for your tails — come on let's go to the tailor, kind of thing. And he always reminds you of the next thing to do. They'd just come in and out. We did go to their house for dinner. They were a delightful couple — really very nice. Very young — just at the bottom of the ladder.

Manny: We went to Stockholm at the same time as the Gell-Manns. It was a big party. There were two other co-sharers of the prize with Max — Luria and Hershey. Max had worked with both of them. They were the friends of our young days. Max brought me and our daughter Nicola, and his three sisters from Germany. The Hersheys and Lurias each brought their son.

Margaret: We just brought ourselves . . . Oh, and Murray's brother, that's right.

Manny: All together we occupied the whole top floor of the Grand Hotel and gossiped as we got into our gowns and tails. The Swedes have dramatic talent; they take four days to build up the show and then let down just at the right rate. Often prizewinners who go there don't know each other. Worse than that, they may be deadly enemies. We were lucky that way. We'd get together after the official parties and discuss what the King said to us, and so on.

I think both Margaret and I had expected something either boring or rather stiff, but it turned out beautifully. One reason is that the Swedes all had such a good time.

Margaret Gell-Mann

Margaret: The long winter is just setting in, and they're going to have three months of it — and to hold a celebration in December is a good idea. Also, the Nobel ceremonies are so much a habit with the people who organize them, it's natural to become a pawn in the game — say the proper thing and wear the right clothes. As Gweneth said, they also supply a young watchdog from the foreign service for each Nobel prizewinner — to make sure your husband has all his buttons on and the right tie. Ours was a very pleasant young man. One day when we were supposed to take some official trip, Murray said, "That's simply enough ceremonies for this week; I'd rather spend the day in the country." Our Foreign Office man said that he was a farmer's son and had a little place in the country, and why didn't we come and spend the day with him. Well, what he had was a 5,000-acre estate with a 17th-century mansion. We spent a splendid day there watching birds and eating delicious sandwiches that they'd brought out from Stockholm, and examining the runic tombstone they had on the property.

What was required of the wives, generally? Did you have to do anything in particular?

Margaret: One thing that was required of me on that occasion was to sit between the old King Gustav Adolf and the then Crown Prince, who is now King, at a formal dinner. So I went around asking everyone I met at the hotel what the king was like, and what you said to him. They said, "Oh, he's a dear; you'll absolutely love him" — and they told us stories like this: He was doing some Christmas shopping at a department store near the palace in Stockholm, and as it was hot in the store, he had taken off his overcoat and someone was standing apart holding it. He was just wearing an ordinary suit. Some very indignant lady came up to him and demanded, "Are you the floorwalker here?" He took it very politely, and never explained who he was.

And indeed he *was* a dear. I sat down next to him after hearing myself formally introduced, and the first thing he said was, "I'm very deaf, so you'll have to shout." I thought, Can I bring myself to shout at the king? But it was a question either of that or of having no conversation at all, so I shouted and we got along very well. He was an archeologist by avocation, and became king late in life. He had done field work in archeology and we had some acquaintances in common, which was a fact I knew because I'd done my homework. That made conversation very easy. He had something sensible and intelligent and very much to the point to say about everything — any subject in or out of what you might think was his ordinary province.

Gweneth: I sat between the king and Prince Bertil — the one who just got married a few weeks ago. The king was marvelous, he really was. Conversation was no problem at all. He's so practiced he could talk to anybody. He did ask me how many members of the House of Representatives there were. I told him, I'm sorry, I don't know. I think it's something like 500.

Richard enjoyed sitting next to Christina, the present king's sister. But he did have a little trouble with the *old* king's daughter-in-law, Sibylla, who was the epitome of a queen or a princess. White hair — not a smile — extremely dignified — ramrod straight. She really wasn't his kind of — girl.

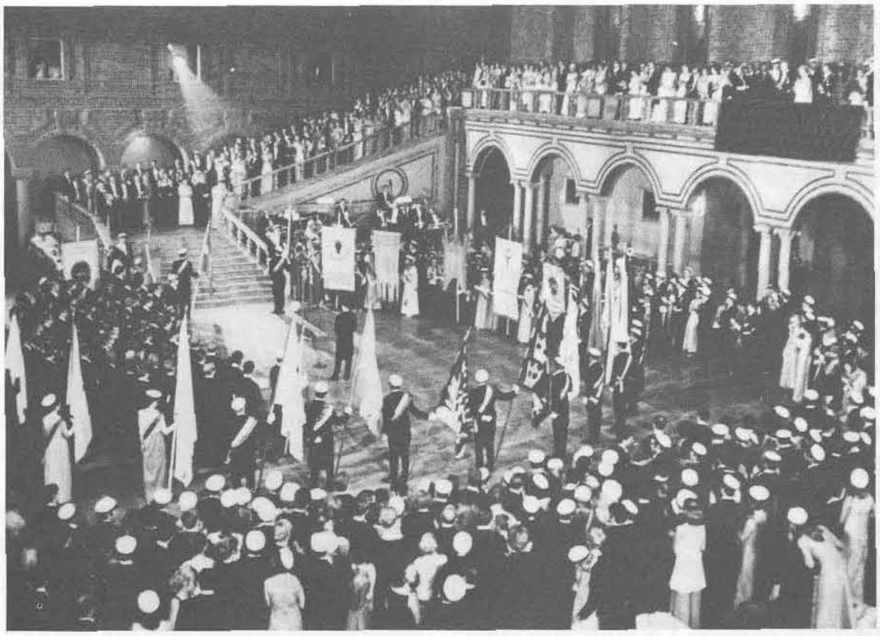
Margaret: In 1969, Murray was seated between Princess Christina, aged 26, and her mother, Princess Sibylla. In his right ear Princess Christina was complaining of the length and tedium of Nobel banquets, which she must attend every year. She went on to tell him of one exhilarating occasion when a new laureate, instead of making his way around the immensely long head table to make his acceptance speech, had summarily scrambled over it, to her great delight. At the same moment, in his left ear the dignified Sibylla was recalling in tones of shocked decorum the banquet at which some unmannerly lout of a prizewinner ("An American, I think.") had actually climbed over the table on his way to the podium. Even Swedish royalty has its generation gap.

Did your husbands prime you at all on things to discuss?

Margaret: No, I think we had a much better time than they did because we were not completely scheduled. We never thought of Max being anyone's younger brother until we saw his three older sisters actually arrive. Murray's brother, who is a very pleasant fellow, was there, and he and I did a lot of things together while Murray was off being presented with scrolls and such, and making speeches at various functions. I think the families had much more fun than the laureates themselves.

Gweneth: I enjoyed the trip we took to Uppsala. That was beautiful. It was the first time we saw the sun in seven days. There was a constant cloud cover; it felt like it was ten feet above your head. It was piercingly cold, with snow on the ground, and it was dark, dark, dark, all the time. Depressing. But as soon as the sun comes through those clouds, it just

Nobel banquet in Stockholm's City Hall, December 10, 1969. Margaret is next to the King.



Nobel banqueters are received with some ceremony at the university students' dance.

changes the whole world around you.

Even though you weren't in on the Stockholm festivities, Lorraine, you've probably heard all about them.

Lorraine: Yes. Carl's told me a great deal. One of the things I remember distinctly is that King Gustav V's salt and pepper shakers and the back of his chair were taller than the salt and pepper shakers or the back of the chair of anyone else at the table. That must be the true meaning of king-size.

Carl's family came from Sweden, didn't they? When he went to Stockholm, did he meet any of them?

Lorraine: Oh, yes. All of them. His father's twin brother, as tall as Carl's father was. Way up there. More uncles than you could count — and 26 cousins.

Did they all come to the festivities?

Lorraine: No, the festivities weren't all that open. But he saw a lot of them. One of the most amusing memories Carl has of the affair involved his trunk. It didn't arrive in time, and he had bought a whole outfit in New York — top hat, tails, and all. So, at the last minute, he had to rush out and rent clothes in Stockholm. And all over Europe afterward, the trunk followed him. It didn't catch up until after he was home again.

What happened in your household, Manny, on the day your husband was nominated, or won the prize?

Manny: I was sound asleep — everybody was sound asleep. It was about 4:30 in the morning when the telephone rang, and I finally woke up and went to the phone and thought, Oh my God, it's probably my mother dying or something like that — so I rushed to the telephone full of fear and a voice — from Stockholm — said, "This is Reuters. Your husband has just won the Nobel Prize!" So I went back to bed and told Max. Max said, "Good! Let's go to Moscow afterwards." (He wanted to try and help a Russian friend in political trouble.) We did go on to Russia for two weeks and did see this old friend. Those first weeks after the news were exciting. Everybody you ever knew in your former life responds. Later you have to avoid being put on a pedestal; family democracy helps.

Gweneth: It was really funny how we heard about it. We'd been to a party the night before — I guess it was a Thursday morning. We didn't get to bed until half-past two, and at four o'clock the phone rang. Of course there are occasions when the phone rings at three and four and five. Not often, but sometimes students in high spirits decide to call Richard. Big joke. Also, some are desperate — they're on a downer and really need help. It has happened. So when the phone rang on Richard's side of the bed he picked it up, yawned, said, "Call me back in the morning," and put the phone down. So I said, "What was that?" And he said, "I won the Nobel Prize." I said, "Oh, go to sleep!" Richard has a very dead-pan way of making jokes and 9 times out of 10 I know that what he says is not so. But this was one time it was true. Then the phone rang again, and he said, "It's four o'clock in the morning. Call me back later." And I wondered who it was who would call him back again. So he put the phone down, and I said, "Is it true?" and he said, "Yes." Then the reporters started to call from New York. So we were lying there wide awake after an hour's sleep with the phone off the hook and Richard said, "I have to get up." When he's thinking he always has to pace the floor. He went downstairs to his study and banged around for a while, then he said, "I think I'd better put up the phone" — and RING!!!

So then we decided we just had to get up, because the *L.A. Times* called and the *Star-News* called and reporters were on their way. So we got up. Those reporters were there at five. Of course it was a full day afterwards. There was a press conference and the phone was going all day long and people were coming over and we didn't get to bed until midnight. There was a long day.

Margaret, how about a little background on yourself?

Margaret: I was born in England and went to Cambridge University where I studied first classics — Latin and Greek — then archeology. I was lucky to have as a fellow student the daughter of a distinguished archeologist who was excavating in Mycenae after the war, so I spent two summers when I was an undergraduate in Greece, digging up among other things the first Linear B tablets at Mycenae. It was one of the most exciting experiences I've ever had or probably will ever have.

Toward the end of my time at Cambridge I began to wonder what I should do next. At that time people with Classics degrees had great trouble getting jobs in England unless they were willing to teach in the high schools, and I didn't think I wanted to do that, any more than I wanted to sell men's neckties at Harrod's, which seemed to be the alternative. So I went to see the job counselor one wet day when I had nothing else to do. When I went to her office, she threw up her hands and said, "Oh my God! Not another Arts degree? . . . Why don't you go to the U.S. for a while?" — trying to get rid of me. She handed me some forms to apply for scholarships, which I did, and I ended up spending a year at Bryn Mawr as a graduate student in archeology. At the end of the year there I hadn't learned much archeology; I was too busy gaping at the American scene and trying to learn to speak the language. The only thing I realized was that I hadn't really encountered anything which you could call typical of the U.S. At least I *hoped* Bryn Mawr wasn't typical of the U.S. I wanted to see a little more, so I got myself a job at the Institute for Advanced Study at Princeton as a research assistant to an elderly archeologist who was writing a long book, and while I was there, Murray and I were introduced by a mutual acquaintance. Somehow we took to each other instantly, although we're very, very different people and always have been, both then and now, and sometimes have trouble finding the same wavelength to communicate on. But we were very much interested in each other from the very first meeting.

I was running the cash register in the Institute lunchroom, which I often did to earn a free meal, when Murray first appeared. As I checked his order, he started to tell me about his upcoming trip to Scotland, and how, as well as attending the physics conference, he planned to go bird watching in the Hebrides. So I drew him a sketch of a puffin, which is a quaint black-and-white sea bird that nests on some of the Scottish islands. I was the first girl he had ever met who knew what a puffin looked like. He still has the sketch.

Later that summer I had to go back to England because I had a cable from my father saying that my mother was seriously ill and if I wanted to see her I should go home. So I made arrangements to give up the job at least temporarily and fly to England. It so happened that Murray was also traveling to London from New York that day, and as he passed through New York from Chicago he had an hour or two between planes, so he called up the Institute at Princeton and asked to talk to me. He got hold of a friend of mine who said, "Well, she's just left for England today." He asked why and she said, she didn't really want to tell him. But he gathered that I was likely to be in the airport at the same time he was. So he had me paged and left a message for me, which terrified me, because I had no idea what he wanted. I was sure it was some further terrible news. It was my first plane trip.

We were on different planes and he arrived in London an hour ahead of me, and he met me at London airport and held my hand while I called up my father and found out the news. And then two or three weeks after my mother's funeral, when his meeting in Glasgow was over, and my immediate crisis was over, he telephoned me and said, "Why don't you come to Scotland for a while?"

Now, this kind of thing, in my family, was absolutely unheard of — going to Scotland with a young man that you hardly knew. But I took advantage of the fact that my father and brother were in a state of shock, and I went. And from then on there just wasn't any doubt that we were going to get married. We did a lot of bird watching in Scotland — and I should say that we were extremely decorous throughout the trip! We only saw one puffin.

In due course I went back to Princeton and he also was on the East Coast that autumn, so we got engaged, and subsequently married.

I had wanted to get married in England for sentimental reasons, but that proved to be difficult because Murray's a very impatient person, and always was, and he didn't want to sit around my ancestral village while the vicar read the banns for three successive Sundays. He couldn't think of anything he'd do for those three weeks, so he said, "Well, what can we do to shorten this process?" Someone said, "You could get a special dispensation from the Archbishop of Canterbury, but you have to have a very good reason." At this point my family got rather nervous that anyone would even think of bypassing the usual means of doing things. Anyhow, we were finally married in Princeton, and we didn't have any honeymoon at all. (We've been trying to make up for it ever since.) We set out the day after our wedding to drive across the country and that was how we spent our "honeymoon," driving our possessions from the East to the West Coast.

Manny: Well, I can tell you that Max took a whole week off from his experiments to get married. He couldn't wait to get back to Cold Spring Harbor.

Margaret: We didn't feel very comfortable in southern California then. We still tell people that though we've been here for 21 years they can expect our departure at any moment, although one of our children has grown up here and gone away.

Manny: I happen to know that Dick Feynman first saw Gweneth in a polka dot bikini at a beach in Geneva. Afterwards he told me he thought he was going to marry this English girl — he said, "Murray's had such good luck with *his* English girl!"

Margaret: It wasn't only the English girl, it was also a small brown dog that we had. When he acquired Gweneth he *also* acquired a small brown dog — which turned out to be a much better bargain than Murray made, in the matter of dogs, at least.

Is this all true, Gweneth?

Margaret Gell-Mann and King Gustav Adolf

Gweneth: Yes it's true. It was summer. Richard was in Geneva for the Atoms for Peace Conference. And I was — well, I'd better go back to the beginning.

I had a very uneventful life — very straitlaced — in Yorkshire, England. Nothing really happened, I went to school and started to be a librarian, and then I would go to the continent for summer vacations. I really became somewhat dissatisfied; I thought, is *this* all I'm going to be? Am I going to get married and then stay here in the same town all my life? And I couldn't take that, so I got a one-way ticket to Geneva. I didn't take enough money to buy a ticket home — on purpose — so that I had to find a job in Switzerland. And I did, and that's where I met Richard.

Were you there a long time?

Gweneth: Almost two years.

As a librarian?

Gweneth: No, I was an *au pair*. I had a good time there. Then my idea was to move on and eventually go around the world — just wherever I felt like going.

And what changed your plans?

Gweneth: Well, Richard can be a very smooth talker. He told me all about California, how nice it was. I didn't realize then that you have to get a sponsor to come here, and it's not too easy to find one. I didn't particularly care about coming to the U.S. I was thinking of going to Australia and staying a couple of years. So I really — he found me a sponsor, and that's why I came. But I had no intention of marrying him. There was nothing between us. We had dates, but . . .

What does finding a sponsor mean?

Gweneth: You have to have someone who will sign a contract that you should never become a public charge. In order to get an immigration visa you have to have a sponsor and they have to be of good moral character and enough money. You can get a visitor's visa, but then you can't work. And I absolutely needed a job. So, I came over to Pasadena. I came in June and we were married a year later in September. I had boy friends here — I had a marvelous time. I would date Richard from time to time, but not regularly or anything. Until he suddenly, out of the blue, proposed. I was never more surprised in all my life, and I had to think about it. He wanted to get married the next week, and I said, "No, I absolutely cannot do that. It's bad enough I have to write home and say I'm going to marry someone that nobody's ever heard of. And so we must have a Methodist marriage, because a civil marriage doesn't mean anything to my family in England. It's terribly important — it just wouldn't count."

This was hard, of course, because Richard is very much an atheist and I'm an atheist, but I wanted to do something for my family. It was going to be very difficult for them, because nobody had ever left home before.

So you were married here?

Gweneth: Yes. We found a Methodist minister and had a little talk with him beforehand to get around the theological thing. We had a very nice wedding.

Was your family able to be here?

Gweneth: No. We went back the next summer.

Did you study to be a librarian in England?

Gweneth: Yes, and I worked at it a bit before I went to Switzerland.

Did you work here in Pasadena?

Gweneth: I worked as a housekeeper.

People don't usually have the gumption to do something like that.

Gweneth: It worked fine. You know, when I first felt in England that I was going to take off and go to Switzerland, I had lots of friends who said, "You're mad," and others said, "I'd like to do it too." But nobody else did it.

It's quite a step.

Gweneth: It was — particularly 20 years ago. People where I came from just didn't do that. It was a very small-town kind of place. I didn't have any job when I went to Geneva. I didn't get one until I actually arrived there. I had the address of two agencies that I got from the Swiss embassy before I went. So I did have two contacts. I got $25 a month — 15 hours a day — 6½ days a week. I didn't have a half-day. I had three hours off on Thursday afternoon and three off on Sunday afternoon.

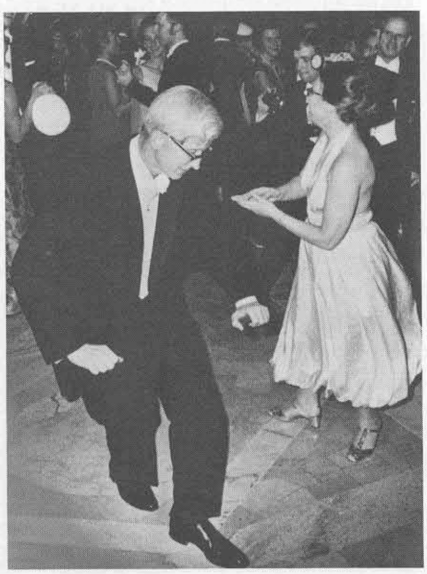
Of course, I couldn't live on that. I had to buy winter clothes and things

Gweneth Feynman and King Gustav Adolf

like that. I had to buy a candy bar and an English newspaper occasionally. And I had to go to the dentist — and that was just awful. How am I going to pay for it? So I had to keep writing to England, and at that time it was illegal to send money out of England. I'd say, send me some of my money; I need it. They would put the notes in personal letters. Once they had it opened, and a very stiff note was returned saying, "You must not send currency out of the country." But they just kept on doing it.

Lorraine: You know, Carl was a young bachelor of just 31 when he received the Prize, and after that he was quite a celebrity. He was sought after by everybody; he was lionized. So I'm very pleased that he chose to marry me. You know, of the four of us, I'm the only one whose husband asked her to marry him after he received the Nobel Prize.

Even though we were married after Carl received the Prize, we've still been involved in all kinds of special occasions because of it. Dinner at the White House in 1962 when President John Kennedy invited Nobel prizewinners and their wives. Lunch with Princess Margaret and Lord Snowden in 1965. Gweneth and Richard were there. Lunch with Princess Christina of Sweden in 1972. Dinner at the Hearst Castle — a dinner for Ladybird Johnson given by Governor Edmund G. Brown. That was a fun evening. Dinner on the terrace, lots of luminaries. Gweneth and Richard were there too.



Max Delbruck in a solo turn

I remember one thing about Kennedy's dinner. They put the wives and husbands at separate tables, and Carl looked over the place cards at his table and saw that one said, "The President," with Mrs. George Marshall and Mrs. Ernest Hemingway on either side of him. Carl sat on the other side of Mrs. Hemingway. She talked to the President quite a bit, and then she turned to Carl and apologized. The Hemingways had spent many years in Cuba, and this was just after the Bay of Pigs incident. She said she was telling Kennedy about Cuba; she thought he ought to know a little bit about it. After dinner Kennedy picked up Mrs. Marshall's and Mrs. Hemingway's place cards and put them in his pocket. So Carl thought if Kennedy could do that, then he could pick up *his*. So he did. He still has it. And he still has the Havana cigar Kennedy offered him.

Margaret: I want to say something serious. There are obviously a certain number of demands associated with being married to someone who is passionately and intensely engaged in any pursuit at all — particularly a creative one like science. But when I started thinking about what I would say tonight, what I really wanted to express was that it is perhaps the most inspiring thing in the world (next to doing it yourself), to be associated with somebody who is fiercely and intensely busy with questions of truth. This is something which absolutely compels a profound respect, and it's a very deep and abiding satisfaction to live with such a person, as I'm sure Gweneth and Manny and Lorraine agree. It's nice to tell stories, but I think this is a very imporant dimension of being married to a scientist, and it makes worthwhile some of the things you find yourself giving up.

I started thinking about the Nobel Laureates and their wives when I was asked to talk — and doing some very miniature statistics because actually I don't know many Nobel Laureates. I

The Feynmans at the students' ball

can think of only one whose wife has a really serious career of her own and that's Mrs. Luria. Certainly all the ones on the Caltech campus have more or less accepted staying home and fitting into their husband's lives, because what he's doing is very important. You find yourself making some sacrifices as you go along, postponing the pursuit of certain interests of your own for hours, or even years.

Manny: I don't think we made our husbands successful, but we could certainly have prevented it. You can so distract a man — so limit or push or annoy him that you can ruin his career.

Margaret: Yes, I agree. And probably most of you have already noticed this, but I'll say it anyway. The process of scientific thinking — the working of a scientific imagination often seems rather erratic, but in the long run there is a pattern that recurs. For example, Murray has an idea — when he first has it, it's a typical bright light bulb going on in the head — it's very important — it's going to synthesize everything that everyone's been working on for the last decade or century — it's going to solve the whole world — and then of course it goes through various phases as he starts to examine it more carefully. He may find he's lost a minus sign or a factor of two, or something; and the idea refuses to explain absolutely everything — there are some experimental results that won't fit. And there's a long period of working out, and gradually the mood gets scaled down to this very practical level and the whole thing becomes less exciting in a way. Although, each time

Richard Feynman receives his prize

some small piece works, there's again tremendous excitement. And finally it's whipped into a reasonable form and you hope no one else has been working on exactly the same thing — and is going to publish it before you do. Then comes the awful moment when it has to be written up for publication, with all the minus signs in the right places, all the footnotes, and so forth. That for Murray is a time of acute suffering, and therefore for everybody else in the house too, because he finds it very hard to write, being a perfectionist.

As I said, this process fluctuates enormously, and I find I have to go along with all the moods; I have to be enthusiastic when he's enthusiastic, depressed when he's depressed. I don't know, Manny, whether you can see the same sort of pattern, or whether Max's way of working is completely different.

Manny: Yes it is. He's rather calm, but I think I have to be able to be on my own and even enjoy being on my own — I've not only observed this from my own experience, but I've spent many summers on Cold Spring Harbor, which is a biological station where many biologists spend a period. There are meetings or short courses — for the men or women who are there. Their whole being has to go into intensive learning. Sometimes I've seen wives or families who came along being miserable, and they have certainly made their husbands miserable by expecting to have their attention at the same time. Expecting to carry on their family life when really that was not what they were there for.

Margaret: Yes, you have to provide a clear space in which they can work without distractions from you, and without a feeling in the back of their minds that perhaps you're restless. But you also have to be ready to stop being by yourself at any moment that your husband wants to go out to dinner, or go for a trip, or whatever. We've just found, perhaps because of our personalities, that we arrange our lives around theirs.

Gweneth: I think when Richard works it's very similar to what Margaret said. When he gets a new idea he's way off. He's just so excited. He paces around, and he talks aloud too. He worries day and night, and then suddenly — "Oh! It doesn't work!" And then if it really and truly doesn't work, that's it. But he sometimes can salvage part of it and then it gets more even. But on the rare occasions when something works — it doesn't happen very often — but then it's really something.

And you provide the trays and keep the children out of the way? And make sure the clean shirts are ready?

Manny: Let's admit it, we do shelter our husbands by making life peaceful and pleasant and fun.

I *like* to organize meals and parties and camping trips and holidays and daily schedules, especially I like to do it *well,* having decided it's not what you do, but how you do it, that matters. I do run the house and garden and guide the children and look after the cars. Even looking after the world and local politics are considered mostly in my realm, and at times I spend much time and imagination on this part of housekeeping. I manage most of the business and do the income taxes. With a moderate effort I've learned a lot and become financially independent.

I do hope you will all have the satisfaction of feeling you are, or could be, independent, and for this I guess a profession is a good beginning. I too might have enjoyed a profession, but I never found any field engrossing enough to sacrifice all my other interests and our freedom.

Lorraine: I wasn't that dedicated to what you'd call a career. When I was 8, yes, I wanted so badly a pair of ballet slippers because I had dreams of becoming a great ballerina. And I didn't get them, of course. Instead, I got vocal lessons and piano lessons.

Do you feel you've had any effect on Carl's work?

Lorraine: No. I don't think so. Not at all. Of course, I've tried to be understanding and to sense when I thought he had something important that had to be done, and I gave that top priority. Though he did a lot of physics after we were married, I think maybe if he was ever ornery it was before we were married.

I think that answers a lot of questions for some of these young wives here. I was going to ask whether you feel that your husband needs any kind of support from you, in view of the type of work he does – but I think you've all answered that very well.

Manny: I'm not scientific and I don't try to give help in that — but there are many decisions they don't want to make alone — many decisions about persons they're working with, and other types of decisions. We have been very close to students and co-workers and postdocs, so that in the end they become almost like your own children. Many have been around for six years or so. And lots of them have many problems, where I, or even the whole family is more helpful than Max alone.

Max Delbruck receives his prize

Do you ever discuss your husband's work with him – and what level of communication does this usually fall in?

Margaret: We probably have rather similar answers to that. I've long ago decided it's not really a good idea to try to learn seriously about the subject, because Murray's particular field is a very abstruse one in which you have to have considerable background in math, which I don't have and have been too lazy to acquire. However, he does talk to me. He tells me what he's thinking and what the new idea is abost and its relation to what's come before, and I find it very flattering that he'll talk to me for half an hour when he's in the throes of a new idea. I'm not sure what he thinks. He thinks I understand more than I actually do. What I really understand is what's going on with him.

I've got to the point where I can give quite a respectable basic lecture on high energy physics without really knowing what it means — I know all the right words and phrases and can put them together, but you mustn't call my bluff. The communication is much more a psychological one than a technical one. That just happens to be true in my case because his interest is totally unrelated to my own field.

How about you, Manny?

Manny: Yes, about the same. And also, he'll say what shall I do about this person I've offered a job to and he's in a nervous state, so I'm sending him over to you to try to help him make up his mind; or shall I go give this lecture here, or is it worth going to this meeting? All those decisions are actually joint decisions. I think Margaret and Gweneth and Lorraine and I have husbands who appreciate us for being nonscientific — something complementing; the opposite of what they're intensely involved in.

Gweneth: Richard doesn't really discuss his work with me but he's superb at explaining a complicated thing simply, and I feel I really understand it — though I could never tell a third person what I think I understand. But he's really very good at explaining. He's quite aware of how basic he's got to be when he talks to dumb people.

I get a feeling he must like teaching too.

Gweneth: Oh, he does. And he loves talking to children. He has always had a very good relationship with Carl, who is 14 now — almost 15. And he is perfectly happy, which is really quite something for someone 15. Carl's mind is very much like Richard's.

How about your daughter?

Gweneth: Well, Richard doesn't have as close a relationship with her because she won't let him. She's a great tease, and she knows that he adores her, and wants to hug her and do everything for her, so she sort of teases and keeps him at a distance. Not all the time, but just enough so she knows exactly where she's got him. She's 8 — and a complete female. She has all the games already. I can just see what she's going to be like when she grows up — she'll be devastating.

What kind of activities have you found that you and your husband enjoyed most doing together?

Lorraine: Carl and I enjoy traveling. With our children we've been to Alaska, Hawaii, Jamaica. Carl and I drove a car 3500 miles through Europe — to Yugoslavia, Italy, just about every place. We enjoy being with friends, being alone together, we like to read, we enjoy entertaining, barbecuing. We had a great time raising our children. We have a small vegetable garden, and we're very proud of our asparagus. We like to swim together, here or at the beach, and we enjoy *our* small brown dog.

Manny: Actually we enjoy practically everything we do. Camping, playing tennis, backpacking, traveling, having children, meeting a lot of people, going to plays and reading books we both seem to like. It's hard to tell, when you've been together so long, which is your independent discovery.

After the award ceremony—Max and Manny Delbruck, their daughter Nicola, and Max's three sisters from Germany—Hanni Brauer, Lene Hobe, and Emmi Bonhoeffer.

Margaret: We also share a great variety of interests — travel, and anything out-of-doors — bird watching, animal watching, hiking, camping, river trips. We're interested in art and music — not so much as performers, but listeners — and languages. Although we have very different academic backgrounds, we both like things with structure. We enjoy the structure and nuances of languages. Murray is a lot more patient about learning new ones than I am. We play word games with each other. We like the same kinds of reading matter. We have a house in Aspen, where we spend almost all our summers; it's an old house, which all four of us are very fond of, and we spend a lot of time working on it, when we aren't in the mountains. Of course, a lot of physics is accomplished there too, at the Aspen Center for Physics. Murray is always enthusiastic, too, about my special interests — which nowadays include weaving and medieval calligraphy.

Gweneth: I love traveling and we travel a good deal, and we like to go backpacking, camping — and just *out*. And when we go out to a place we like to go to places on the back streets. We don't like staying in big hotels; we like little unusual places. And we both love to travel in the van — it's so comfortable, with the table and all. The seats fold down into a bed. It's a Dodge Maxivan; the biggest one there is. The four of us can sleep in it, if we have to. We prefer sleeping bags outside.

You do have a place in Yorkshire?

Gweneth: Oh yes. Everybody else is there. I'm the only one who ever left. We've been back two years in a row. Richard loves it; he can never get enough of it. The countryside is just — you can set out in any direction and just walk wherever you want, and with you go the dogs, of course. Richard runs, you know, every morning. He's got halfway to Mt. Wilson now — four miles straight up. And he runs when we're in England, which not many people do. He wears his little orange shorts and runs seven or eight miles, and word filters back from the country people around: "*That man's* back again. I saw him three miles over here." "Oh, did you? When I was driving my car at five o'clock yesterday morning he was over *there.*"

I have a question here that may be deeper than some of the others. How have you four achieved feelings of self-importance without being overshadowed by your husband's work?

Manny: Well, I don't know that I have a feeling of self-importance! When I was younger I did feel uncomfortable about the whole world of men doing science — a bit left out. I've seen this in younger women, sometimes it was too much for them, it broke up their marriages. Especially since women's lib started. Then these men and this life made me so welcome I decided to relax.

Lorraine: You know, the Nobel Prize has enormous glamour and prestige, but I don't think Carl or most of the people who have one feel that way at all. Carl has always been very modest about everything, but especially about the Nobel Prize. I feel that way too. He always says he thought he had a lot of luck on his side.

Gweneth: I don't have the feeling that I have to be important. I'm typically happy with what I do, and I don't feel I have to compete. I don't feel a shadow — I'm perfectly happy and I know I make him happy — not by being a servant to him — we get along very well. I know he's happy because he says it. When he comes home at night he says, Oh it's so nice to come home. Like on a rainy winter day when we have a big fire in the fireplace and the curtains drawn and good smells coming out of the kitchen. I don't do it just for him — I do it for the family, and I like it — I like to be comfortable. This is where my satisfaction lies and I don't have to feel important. I do things that he doesn't do, and I do them well.

You do volunteer work in schools?

Gweneth: Yes. I spend a lot of time and get a lot of satisfaction out of that, knowing that I'm doing something that's worthwhile. It's in the public schools; setting up a library. We set one up in Edison school about six years ago; they had none. And we started one in my daughter's primary school. This is the second year, and this was the last primary school in the whole district that didn't have a library. The books were there — stuffed in a closet for a whole year, and nobody knew about it.

And then of course I sing with Margaret. We have a group called the Arroyo Singers — between 16 and 20 of us. We sing all kinds of things. We're doing some haiku set to music by Persichetti, and we commissioned some work to be written for us. We do Bach and Brahms, and all kinds of things. The last time we performed was at the Music Center on Christmas Eve.

Margaret: I think we all know in the back of our thoughts that we're very important to these men whose activities we have enormous respect for, and as Manny said, we can't make them, but we could certainly break them. It's rather a definite policy not to do that. A lot of things we can do a lot better than they can. Like hanging pictures — or cooking. Murray and I love food and wine.

There's one thing that I still find rather exasperating, and that is, if at some social gathering I happen to make a remark that gets a general laugh or is considered to be worth repeating, almost invariably it's attributed to Murray. I've concluded that people simply want to say that they've heard Murray Gell-Mann make this remark. Murray is extremely fair about it, and if he knows I've said it, he'll always say, "Why Margaret said that" — but it doesn't make any difference at all to the people who want to quote him.

I've just got to the stage where one child is off to college and the other is in junior high school and I am rather wondering about my own life. I was talking to my 20-year-old daughter a few months ago and I said, "If I were you I wouldn't play it exactly the way I have done — that is, I would make sure I had a firmer hold on a career before I deliberately gave it up in order to marry someone, no matter how much I was impressed by what he was doing, no matter how serious and important I thought it. Because, if not now, perhaps later when your children are grown, you'll need something to go back to." I said, of course, with me, I would have found it rather difficult to have a career — whereupon she broke in and said, "Oh yes, I do understand." She's lived with her father a long time and studied him rather carefully!

She said another interesting thing: "One thing I've learned from Daddy is to recognize and to hate shoddy thinking." Then she looked very sad and said, "But there's so much of it about — and so much of it is mine." I know just how she felt. But I said, "You can't really say it's a bad thing to have learned to recognize shoddy thinking — it's really a very important asset in your life."

Being the child of a celebrated father is something of a problem. I don't know how many of you have children and whether they all worry about these things. Our kids have handled it in different ways. Our son says rather bitterly that everyone expects him to be very smart because of his father, and that's quite a burden to carry. We tell him, of course, that no one expects him to the the same person as his father — but the feeling is there.

Manny: I think anybody who has a strong father, until that father dies, is not really free. □

Allen: That's only part of the problem. Even if we're successful in predicting small earthquakes (and I think we have been in a few instances), will those same techniques work for large earthquakes? We'll never know until we have a chance to observe a large earthquake, and that may not happen in the next ten years. That's one reason why we would really love to do experiments not only in California but in other areas where the seismicity is even higher, such as parts of Turkey, perhaps, or in China, where we could instrument an area with a greater probability of seeing a large earthquake during the period of the experiment.

Is there any international cooperation on the measurement of earthquakes?

Allen: There is a worldwide network of seismographic stations that has been established for a number of years, and it has given rise to a lot of data that are shared. But I think the most significant thing is that in the field of earthquake prediction itself there are a number of joint research projects between different countries — for example, between the Soviet Union and the United States. Some of our people are in the Soviet Union right now with seismic equipment in the field. Some of their people are in this country; one of their scientists has recently visited Caltech. There are also cooperative programs between the United States and Japan.

Anderson: There have also been several exchange visits between Chinese seismologists and American seismologists. Clarence has been to China on one of these exchanges.

Allen: We don't yet have any cooperative research programs with the Chinese, but there's been a fair amount of communication.

Kanamori: In terms of flow of information, there has been a very extensive exchange. Through the scientific literature, we know what is happening in other countries, and by reading many journals they know what is happening in this country.

Allen: I was amazed in China by how up to date their scientists were on what is going on in this country. They knew a whale of a lot more about what we're doing than we have taken the pains to find out about what they're doing. One result of what was apparently a successful prediction of a major earthquake in 1975 has been a great deal more interest in this country in finding out what the Chinese are doing.

Kanamori: Clarence mentioned this worldwide seismographic network system, which now has nearly 120 stations. Those stations all have the same type of instruments, and they gather similar information. So we can study in detail earthquakes that occur in other parts of the world. For example, we have collected all of the records of the Chinese earthquake in July of last year. By studying those records, we can acquire information that may be relevant to future California earthquakes.

Allen: It's rather interesting that this particular Chinese earthquake was a really major disaster, no question about it. The loss of life, reportedly, may have been as much as 600,000 to 700,000, which would make it among the major disasters in world history. That earthquake, insofar as we can tell, was not predicted by the Chinese, and they do not claim to have predicted it. This is rather interesting because they have made claims of some 10 to 15 successful predictions of previous large earthquakes, some of which are no doubt valid claims. We don't yet know the full meaning of this lack of prediction because we have not heard reports directly from China, and no American groups have been in China since that earthquake.

That would seem to be an indication of the difficulty of predicting different types of earthquakes in different circumstances. What techniques are being utilized by the Chinese? Do they differ from those used, say, by the Soviet Union?

Allen: Well, many of the techniques are the same kinds of things we're trying here, and the Japanese and the Soviets are trying. They involve such things as velocity changes, tilts, and measurements of radon, which is a radioactive gas that sometimes seems to increase in ground waters before earthquakes. But I think the one unique aspect of the Chinese earthquake prediction effort is the great dependence upon observations — by amateurs in the countryside — of such things as animal behavior before earthquakes.

Anomalous animal behavior has been subject to a great deal of joking, even in China. It's not clear that they really believe it in every respect, and yet they are not willing to dismiss it. And I don't think we should either. At any rate, in China there are hundreds of thousands of people in the countryside who are making various kinds of observations of, say, the level of water in their backyard wells, or the behavior of their animals, or of any other sorts of anomalies that could be observed by amateurs. I don't know of any other place in the world where there is this kind of dependence upon local people.

Apparently these observations have played a fairly large part in their predictions, and they may well have been significant. Even if 95 percent of what they're doing is not scientifically meaningful, that other 5 percent still represents a more massive effort than is going on anywhere else in the world. I think we have a good deal to learn from that effort.

Is any research like that being done here at Caltech?

Allen: Not that I'm aware of — though there is some interest in animal behavior in this country. I think it's important to note that there's a great difference between our country and China. We have the opportunity to put out sophisticated instruments, we have the funds (at least, I hope we will have the funds), and we have a smaller area to work in here in California. In China the situation is quite different. They have a tremendous resource in peasants out in the countryside.

Anderson: The Chinese farmers routinely monitor water-well activity. With lots of small farmers it is possible for them to monitor unusual animal behavior in a much more systematic way than we can. Also, the government can tell people to observe things — and to get out of their houses.

Allen: In China, Mao's philosophy espouses the wisdom of the broad masses of people. The Chinese are convinced that the people have been predicting earthquakes for 3000 years, and this is something you don't question.

What could you ask people in Los Angeles to monitor?

Allen: There are probably things we could do that we haven't been doing. This problem of the level of water in wells may be very important among events happening before earthquakes. We're not making many observations on this sort of thing, and perhaps we should be. We might also try to enlist the aid of a relatively sophisticated group of people — like the radio ham operators, who have a good deal of experience with sophisticated instrumentation.

One of the peculiar aspects of your occupations is that part of the success of your research depends upon the existence of earthquakes. You probably look forward to earthquakes as a research tool, but how do you feel personally about living in an earthquake area?

Anderson: Earthquakes are fascinating things, and the more you understand about a subject, the less you fear it. A great deal of beauty goes along with earthquakes — such as the rugged coastlines we have in California and New Zealand, beautiful mountains and valleys. These are all part of the on-going geological process.

Allen: You *are* an optimist, aren't you?

Anderson: Of course, if you are right in the center of an earthquake, there's a great amount of fear when it does strike. It's one of those natural phenomena that will never be predicted to the precise moment, so it's quite a thrill when it happens. As soon as you get over the initial shock, you, as a scientist, start analyzing the vibrations you're feeling. Naturally, we immediately come down to the laboratory to go to work.

Allen: I can assure you, though, that during the San Fernando earthquake at six o'clock in the morning, we were experiencing the same kind of fright and trauma that other people were. We weren't jumping out of bed saying, "Great! We're going to have another scientific experiment." Now, admittedly, it did provide an opportunity for us to try to understand earthquakes better, and we tried to take advantage of it. But at the time of the shaking, our reactions were probably not grossly different from those of anyone else.

I think it's also true, though, that those of us who study earthquakes don't have the blind fear that many other people do. People tend to fear what they don't understand. I think we realize that, yes, the next great earthquake will be a disaster, but it will not be a cataclysmic kind of thing. Parts of California are not going to slide out into the Pacific. This is ridiculous.

What is the Palmdale bulge?

Kanamori: The U.S. Geological Survey studied geodetic data in the past, and they discovered that the ground in the Palmdale area was uplifted about 25 centimeters, or about 10 inches, sometime in 1962. Then during the subsequent period of time, this uplift spread out into the Mojave Desert.

Of course, there are many examples of uplift in the past. Probably the most famous is the uplift prior to the 1964 Niigata earthquake in Japan. About five years before that earthquake there was a rather extensive uplift in the epicentral area, and in 1964 a magnitude 7.4 earthquake happened. In this particular case, the uplift was a precursor to this rather disastrous earthquake. Obviously, there is some concern that this Palmdale bulge may be one of the precursory phenomena prior to the next major California earthquake. However, there are some other cases in which uplifts happened, but no earthquakes.

When we talk of earthquake predictability, are we talking in terms of predicting for a range of time or for a certain date? How accurate could earthquake predictability ultimately be?

Allen: Well, since we don't know what the finest point could be, I think the important thing right now is that anyone who is offering a prediction should specify a bracket of time, over which he thinks the earthquake will occur.

There are, of course, all kinds of predictions; for example, we have said a major earthquake is likely to occur in the San Andreas fault in the next 100 years. In a sense that is a prediction, but it's not a very meaningful one in terms of doing anything other than work on building codes.

The Chinese prediction of the earthquake in 1975 started out five years before the earthquake. At first it was merely a prediction that that area was of some concern, and it had no time limit at all. Then about a year before the earthquake the Chinese specified that they expected an earthquake with a certain magnitude within about a year. As the time got closer, they successively refined that prediction, and, finally, 5½ hours before the earthquake, they announced that it was so imminent that people should get out of their houses. In China, when they say that, people get out.

Will we, at some point, be able to predict magnitude?

Anderson: Yes. There are several ways. One is the duration of the precursor, the length of time it takes between the onset of something that appears anomalous and the actual event. It now seems that the longer the time the anomaly builds up, the greater will be the earthquake. Also, in the area that is being affected by the anomaly — such as a tilt anomaly or a radon gas anomaly, or a ground uplift anomaly — the larger the area involved, the more likely it is that the earthquake will be large. The trouble is we have limited data before large earthquakes. Most of our information has been obtained on very small earthquakes, and it might be dangerous to extrapolate to larger earthquakes.

As for the time of earthquakes, there are what we call long-term precursors and short-term precursors. The long-term precursors might start years before the earthquake. As the stresses continue to go up and we get closer and closer to the earthquake, we anticipate that more dramatic things will happen, such as an increased number of foreshocks or, perhaps, an increased squeezing out of radon in the ground, or an increased activity of the ground deformation. This might allow us to say it's now getting much closer, and we might be within a week.

As far as southern California is concerned, a prediction won't really be very useful if it says in the next year we're going to have an earthquake. That doesn't tell us to do anything more than we should have been doing all along anyway — just by the simple fact that we live in an area of the world that is prone to earthquakes. However, when we get the prediction down to the point where we can say there will be an earthquake within, say, the next two days, then you can take such emergency preparations as the evacuation of people, or at least having people leave their dwellings and sleep outside. This is what the Chinese did. If the prediction is vague to the extent of even a month, you're not going to be able to save many lives. In the case of China, perhaps, they could have ordered people out of their houses for a whole month, but that would clearly not be possible in Los Angeles.

Allen: It is not even clear that 5½ hours would do it here.

What does earthquake predictability mean to the people of Los Angeles? If we could predict an earthquake accurately, what would we then do in any major city? Is it best to evacuate people? Or should we take preventive measures regardless of prediction of earthquakes?

Anderson: There's a wide variety of opinion on the social implications of earthquake prediction. Since we live in earthquake-prone country, all schools and emergency facilities, such as hospitals, should be built up to earthquake standards. These are things we should do regardless, because there *will* be an earthquake during the life of these particular buildings.

My personal opinion as to what to do when we can predict an earthquake within a very narrow time and space limit is that all emergency people should be notified — the police, the hospitals, the civil defense, and the National Guard. These organizations should be on alert; their vehicles should be parked in such places that they won't have their garages collapse around them. People should be told to stock emergency supplies, particularly food and water (because there will be water interruptions), and medical supplies. Every family should be relatively self-contained. I'm not sure how much further one should go.

Allen: Well, of course, we've identified 14,000 buildings in Los Angeles alone that are basically deficient in terms of earthquake design. The City Council is now trying to find some way of bringing these buildings up to standard or getting rid of them. We know darn well those 14,000 buildings are dangerous.

One obvious thing to do is at least to try to get people out of those particular buildings because that's clearly where there is going to be major loss of life. But I don't think that applies to most people in their own homes. Single-family dwellings as we know them in southern California are relatively safe places to be. That's not true in China, where most people have been killed in their own homes because of the way the homes are built.

What activity is government — city, county, or state — engaging in as far as the analysis of fault structures prior to housing development? Is there any effort to look at existing fault systems and prohibit developments along fault lines?

Allen: There's a fair amount of effort in this. Over the years we have gotten progressively better in our land-use planning with regard to earthquake hazards. Just about three years ago a bill was passed in Sacramento that identified special study zones along our active faults, and if you propose to put in a housing development or a subdivision within that special study zone, you must satisfy the authorities that you have taken into adequate account the active faults that exist within that zone. Many of our cities have been relatively progressive in terms of land-use planning in regard to landslide hazards that may be related to earthquakes. We are making progress.

Is there something you feel is essential for people to understand in relationship to earthquakes?

Allen: I would just re-emphasize what Don said; that is, that the next ten years are going to be rather rough ones for us in this area. I'm firmly convinced that earthquake prediction in the long run will be beneficial to people, save lives, save property loss. But nevertheless, in the process of achieving that prediction capability we are possibly going to have some false alarms, and it is going to be a rough ten years for us all. I hope the public is going to be a bit patient and recognize that this is a scientific development, a research development, that's going to have many problems as we develop our capability.

Anderson: And it's going to be particularly rough for the media, because a lot of people are going to be coming out of the woodwork, predicting earthquakes with no scientific basis whatsoever — just to gain publicity. If the media don't become very sophisticated in dealing with this kind of phenomena, they are tampering unfairly with the fears of the public.

You are suggesting that the media should take earthquake predictions with a grain of salt unless they come from reliable sources.

Anderson: Yes. This is one case where selling newspapers and getting prime-time TV just for the sake of sensationalism is not in the public interest at all. Because of the fear of earthquakes, this particular kind of sensationalism is extremely dangerous.

Allen: Many of us are rather unhappy with some of the TV coverage of unfounded predictions. It is almost like crying, "Fire!" in a crowded theater. I think a certain amount of discrimination has to be used by the media in the treatment of these stories.

To large numbers of people, the concept of an earthquake is very threatening. Perhaps we need to stop looking at the earth as a solid, static object and start seeing it as a constantly changing one.

Anderson: That is exactly right, because the earth *is* in constant motion. We are always going to have earthquakes — many of them damaging to people and property — but knowledge of the real risks and judgment about safety measures can reduce that damage. Scientifically responsible earthquake prediction may help all of us to live more comfortably with our shifting planet. □

Whither Molecular Biology?

by ROBERT L. SINSHEIMER

It is precisely the unprecedented potential of molecular biology to reconstruct the world of life, so long accepted as given, that requires us to reconstruct our way of life

I AM REMINDED of one of Sherlock Holmes's celebrated cases in which the critical clue lies in a series of seemingly misspelled words. Here, too, it may be that the critical question will be, not *whither* molecular biology, but *whether* molecular biology? As has become the case with nuclear science, questions of molecular biology have become issues of public policy.

Ordinarily, an attempt to foresee the future of a scientific discipline such as molecular biology would begin with a tour of the current frontiers of the field. In the nature of science such an excursion would inherently include a description of adjacent contours recently crossed. One could then proceed, as adroitly and imaginatively as possible, to project the likely future contours and paths, out of the internal logic of the discipline.

In this case, however, it is not at all certain that molecular biology will be free to develop solely according to the dictates of its internal logic. Molecular biology is simply too important. Its insights and its techniques impinge too directly upon too many vital public concerns. Thus, external forces may well channel its future in directions not entirely congruent with those defined by its internal logic. Indeed, I suggest they already have. And the form and consequence of such interactions are not so easy to foresee.

Let me, then, attempt first to sketch some of the directions in which I think molecular biology *would* develop out of its internal logic. Let me then attempt to describe the external forces which I suggest have and will impinge upon this development, and then let me attempt to foresee some likely consequences of these interactions.

The central theme in molecular biology to date has been the recognition that the genes serve as the information bank and command center for the cell (and thereby in part for the organism). We have come to appreciate the constant reference to, the constant involvement of, the genes in the life of the cell. We have established the outlines of gene information storage, replication, and expression. In bacteria we have now a substantial understanding of the modes of control of gene expression.

In cells of higher organisms, however, with an order of magnitude and more greater genetic content, the genetic control mechanisms are necessarily more intricate — and we still lack an authoritative understanding of such mechanisms and their interactions. Various plausible proposals have been advanced for the regulation of genes in batteries corresponding to various states of differentiation. The validity of specific proposals remains to be established.

Varied proposals for control processes effective at other levels — between making of DNA and syntheses of protein — have been advanced with more or less compelling evidence.

The magnitude of effort expended in this field and the steady development of technique and insight make it virtually certain that we will achieve a growing understanding of the mechanism of gene control in higher organisms. This understanding will carry within itself the keys to the understanding of preprogrammed differentiation and development, as well as of cellular response to all manner of external stimuli — from hormones to drugs, from carcinogens to narcotics, from antigens to transmitters, from radiation to cell-cell contact.

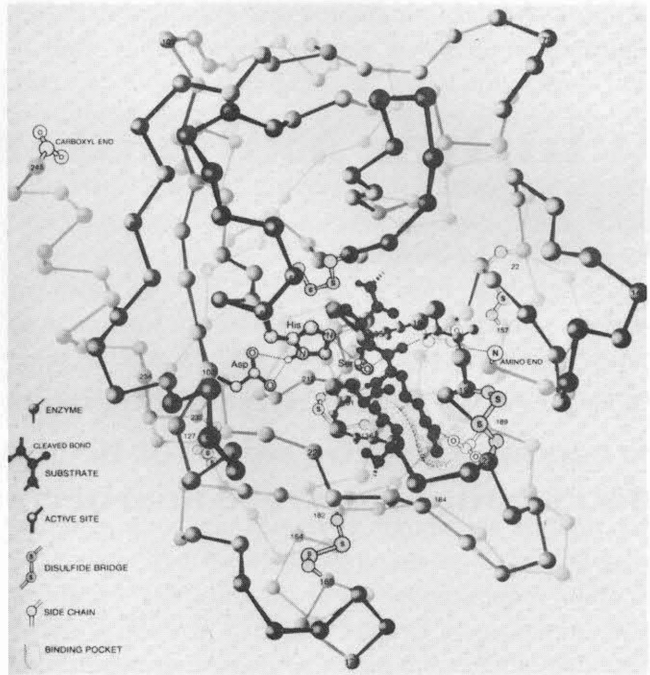
And out of this understanding will develop the opportunity to intervene in the state of differentiation of the cell for varied purposes. Because we have not had such opportunity, I expect we most often tend to accept the particular differentiated state of a cell of a higher organism as given and fixed. We thus forget that each cell, bearing the entire genome, has potentialities far, far beyond those it expresses at any one time. The understanding of the control system would permit us to unlock those potentialities, in whatever combination we might choose. The clinical impact of such a capability must be profound.

Understanding of the control mechanisms may also clarify the current state of confusion as to the function of much of the genetic material — the DNA — of higher organisms. Current insight only permits us plausibly to account for perhaps 10 percent of the DNA of, say, a mammal. Various hypotheses suggest that the bulk of the DNA is involved in the command and control mechanisms — or alternatively, that much of the DNA is currently functionless, free to mutate, and is thus a reservoir of future genes, a glimpse of evolution in process.

In view of the critical importance we attach to DNA it is clearly essential that we achieve a firm understanding of the role of the bulk of this substance. Somewhere, wrapped also in this enigma, lie the keys to the understanding of other current conundrums — of the origin and maintenance of assemblies coding for families of structurally related proteins, and of special DNA sequences that may be involved in duplication and recombination of genes, as means of gene permutation and gene amplification.

But information storage and expression and control are hardly the sole business of a cell. Cells must survive and reproduce — they must literally do things. And for these purposes they need and have machinery. And a second great accomplishment of molecular biology has been to provide a growing insight into the nature and function, the architecture (above), of the molecular machines that do the work of the cell.

We have come to appreciate that even single-enzyme molecules are intricate machines — skillfully adapted to grasp their substrate, to draw it into an en-



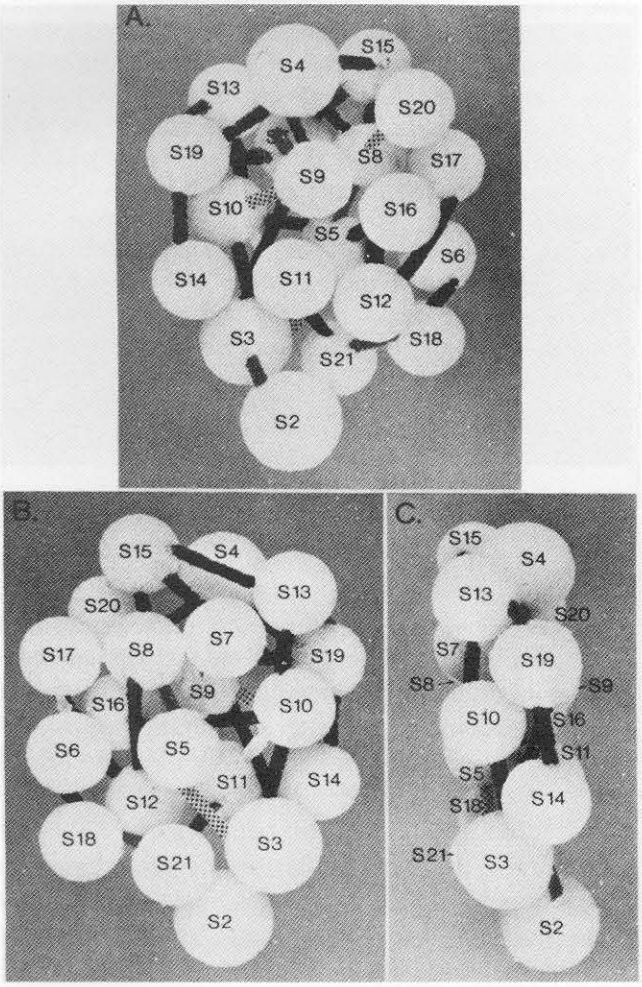
Even single-enzyme molecules are intricate machines — as shown by this molecular model of the interaction between the enzyme trypsin and its substrate.

vironment favorable to the catalysis, and then, the deed done, to release the fragments while returning to the initial state ready for the next cycle. We have learned how the activity of an enzyme can be controlled by simple gating mechanisms which control access to critical regions of the molecule — mechanisms that can be irreversibly displaced as in the activation of an inactive proenzyme (such as chymotrypsinogen), or reversibly modulated by the biochemical addition and removal of small blocking groups (such as phosphate or acetyl or adenogyl).

Multienzyme assemblies are even more evidently machines. For instance, a complex involving 10 or 12 proteins mediates DNA replication; its structure is yet to be elucidated.

The special architecture of the chromosome must certainly relate to its varied roles in the several stages of the life cycle; the nuclear pores, those gatekeepers of the inner sanctum, must monitor and regulate nuclear-cytoplasmic traffic in response to unknown commands.

The ribosome involving some 50-70 proteins and three RNA molecules is a machine of extraordinary versatility, able to translate *any* RNA molecule with appropriate recognition signals (the password, as it were) into its corresponding polypeptide chain. The details of its intricate organization, the structure of its



A three-dimensional model, seen from the top (A), bottom (B), and side (C), illustrates the spatial relationships between the proteins of a ribosomal subunit.

active sites, the mechanisms of its processes (above) are all under intensive study.

As an instance, the extraordinary action of complement — the multienzyme machine found in blood and employed by the immune system to destroy the invader, once it has been recognized as alien — is now well understood. Following fast on this is the appreciation of the clinical consequences of deficiencies or defects in one or another of the elements of this complex defense mechanism.

The achievement of motion — contraction or transport or propulsion — requires an elaborate multimolecular machinery to convert chemical energy into mechanical work. The essentials of this machinery in its more enduring forms — as in cilia and muscle — are now increasingly clear (right). We have also come to appreciate that more transient machines are assembled and disassembled on short notice within the cell to achieve changes of shape, amoeboid motions, ruffling, streaming, and so on.

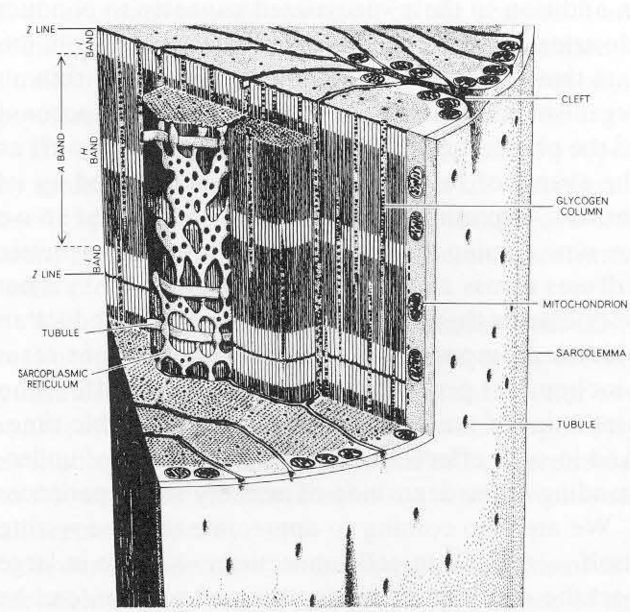
The provision of energy requires even more intricate machines — the mitochondria and the chloroplasts. These discrete organelles segregated within the cell retain for obscure reasons small satellites of autonomous genetic material together with their own protein-synthesizing machinery.

The membranes of the cell are quasifluid machines of extraordinary surface-to-volume ratio. They contain the gatekeepers, the sensors, the packagers, and exporters — the elements involved in the interaction of the cell with the outside world. The surface structures must in some way communicate with the command centers of the nucleus, but the mechanisms of such communication are at present unclear.

Our admiration for these extraordinary devices is enhanced by another circumstance. *Our* machines must be built; molecular machines can self-assemble. This is both self-evident and astonishing. Each component must not only perform its task but must also contain the appropriately designed connecters to couple it to its proper neighbors — and to nothing else within the cell.

The self-assembly of the ribosomal subunit can be reproduced in the test tube, and such studies have provided considerable insight into some details.

Virus particles are elaborate machines, and their



Cutaway drawing of a striated muscle fiber. The achievement of motion requires an elaborate multimolecular machinery to convert chemical energy into mechanical work.

self-assembly has been studied in elaborate detail.

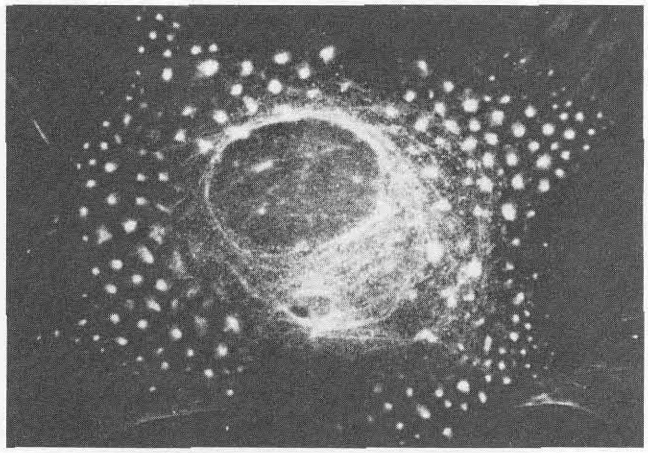
Almost all of this intricacy and elegance is the expression of preprogrammed patterns. We need only recall how an entire bird develops within the egg with no outside intervention to realize the potential latent in such preprogramming. But in the course of evolution Nature has developed more flexible patterns of growth, more open-ended patterns of development capable of adaptive response to environmental circumstance. These mechanisms, which we understand much less well, culminate in the central nervous systems and their associated sensors and detectors — and particularly, of course, in the central nervous systems of higher organisms, birds and mammals, primates and man.

These structures and their capabilities return us to the concept of information receipt and processing and storage, but on a very different level as regards diversity of input, ease of recombination of information elements, and flexibility to develop integrated patterns of processing and response.

Through these organs, these machines of a different order, the external world becomes represented in the internal, and the resultant interaction plays a potent role in the individual development and reactivity.

The evident importance of the electrical signals in these quite different machines and the extraordinary intricacy of their architecture — the wiring, the connections — for some time diverted attention from the evident fact that the elements of these machines are living cells, with the many capabilities of living cells, in addition to their specialized capacity to conduct electrical impulse. It also diverted attention from the fact that this adaptive machinery resides within an organism and that it must recognize and take account of the physiological state of the organism as well as the events of the external world, as reported by its sensors, to produce an adaptive response. And so we are now coming to appreciate that when a transmitter diffuses across a synapse to an adjacent neuron it not only causes that neuron to produce and conduct an electrical impulse — it also initiates longer-term biochemical processes that may in turn affect the properties of the neuron for some considerable time. And in such effects may lie the beginnings of understanding of the deposition of memory and experience.

We are also coming to appreciate that the wiring itself — the cell-to-cell connections — while in large part the result of a preprogrammed pattern, can be varied, certainly functionally and perhaps anatomically, by the effects of early experience, and indeed that the organism has specifically provided for this



Use of a fluorescent antibody reveals the nodes of the filamentous network that forms the cellular cyto-structure.

opportunity in critical periods.

It is also becoming apparent that there are neurons in critical sectors that monitor and are responsive to indicators of the body physiology — to hormones, to factors inducing that mysterious change of psychic state we call sleep, and very likely to many others. Very recently it has been recognized that there are endogenous substrates (polypeptides) for the opiate receptors — those molecules on the surfaces of certain neurons to which the opiates bind to thereby produce their extraordinary effects. The function and possible pathologies of these endogenous substrates will surely be of the greatest interest.

Such belated recognition of the significance of biochemical traffic between the central nervous system and the body, in both directions, is certain to have major implications, both clinical and psychological. This is, of course because the central nervous system is more than a processor of sensory data — it is the seat of mood and affect, sensation and thought.

That the brain is chemically differentiated, that it is conceivable chemically to influence different sectors differentially and thus affect differentially sense and mood — even perhaps memory and lucidity — suggests the basis for the development of a molecular psychobiology of a significance to rival molecular genetics.

We *could* then project a future for molecular biology as a logical continuing development, providing increased understanding of these systems for information storage and processing and expression at varied levels, and increased resolution of their associated biological machinery. Such a projection could be reasonably straightforward and offer fascinating vistas.

However, such projections ignore two great changes which are now taking place and which, I suggest, are very likely to introduce major discontinuities into the smooth evolution of molecular biology.

One change is a qualitative transition within the science itself. Molecular biology has crossed a threshold from a purely analytical science to a synthetic science. I refer, of course, to the recombinant DNA technology which permits us to explore biological processes by construction and innovation as well as decomposition. The other change is in the public perception and valuation of science in general. The larger society is both more appreciative of its need for science and more apprehensive of the fruits of science — and is thus increasingly insistent that it play a role in the direction of science.

Molecular biology has crossed a threshold from a purely analytical science to a synthetic science

These changes clearly interact and reinforce each other. For the invention of synthetic biology, the capability literally to design new organisms, greatly augments the power of biology, both to meet the needs of society and to stir its apprehensions.

With respect to the former, we have already seen the tidal pull upon the patterns of scientific funding, and thereby research and training, exerted by the public concern with certain diseases and disabilities. To the extent that the tide has moved in the same direction as the stream of molecular biology we have been able to flow with it — as in those studies of cell biology which clearly relate to the cancer problem. In other areas, as in heart and pulmonary diseases, the synergism has been much less effective — and still other areas of, for instance, bacterial molecular biology have been left as dwindling tidal pools.

I think it likely that this trend and the associated pressures will continue. We may expect molecular biology to contribute, and to be expected to contribute, ever more effectively to the relief of the infirmities of the human organism — disease and aging, addiction and depression, and indeed, to all the defects latent in the machinery of the body and, increasingly, the mind.

Molecular biology may be expected to contribute more importantly to the relief of certain infirmities of the social organism — to population control and mental health. Further, as society begins to appreciate that molecular biology can contribute significantly to other critical problems — as in agriculture and, very likely, in the field of energy — strong pressures will develop to deflect the flow of molecular biology into directions appropriate to those technologies. We would surely benefit from improved understanding of photosynthesis or nitrogen fixation. It does not seem inconceivable that with imagination we could learn to employ biological energy transducing systems (such as that found in the purple halobacteria) to convert solar energy into usable chemical energy on a massive scale. Such ventures would attract attention and resources to areas of molecular biology currently in relative neglect.

More broadly, while surely inferior to the ideal of support of science for its own sake, I think the development of multiple sources of support and understanding, albeit mission-oriented support, for molecular biology will be to the good. While it is graceless to deplore the hand that feeds you, I believe the predominant support — even though it has been truly enlightened — of molecular biology by a single agency dedicated to medicine has, in fact, already distorted and limited our perspectives.

However, the input of societal pressure will, I expect, not only be directed and positive; it may well be, in certain fields, negative and restrictive — at least to the degree that we may well be required to seek alternative modes to advance our science. I am thinking again of the recombinant DNA area although similar issues may arise one day out of molecular psychobiology when that develops.

The recombinant DNA technology was developed by molecular biologists as a means to solve their scientific problems. However, as a technology it has many, many other applications and implications, some of vast import.

With but modest extrapolation recombinant DNA technology literally makes available to us the accumulated gene pool of the planet to reorder and reassemble as we see fit. It makes this capability available not only to scientists but to entrepreneurs, to flower-fanciers, to militaries, to subversives — to all sectors of society.

In our consideration of the potential hazards of this technology, as exemplified by the Guidelines of the National Institutes of Health, we have predominantly been concerned only with the potential for immediate health hazards that might arise in the course of scientific investigation. I believe this is a very limited perspective which arises in part, inadvertently, out of the sustained impact of the NIH role as a major source of research support in this field. In truth — and we should be aware — we have but little knowledge of the resilience, the coherence, of the intricate web of life support systems of the planet. The possible environmental and evolutionary consequences of this development — the numerous and varied societal consequences — have not yet been adequately addressed. To make my point, one may ask would we have developed the same guidelines under the aegis of a different sponsor?

I think the larger society may well, for its own good reasons, impose major restraints upon the introduction of recombinant DNA technology with its almost incalculable consequences. If so, then molecular biology may well be required to develop alternative ways to achieve its objectives, scientific or applied. Such means are by no means inconceivable, if likely more difficult. If there is a shortage of pancreatic insulin, must it be made in free-living organisms? Chemical synthesis, ribosomal synthesis, tissue culture synthesis are all conceivable.

I suggest we can foresee biology becoming increasingly, although not wholly, a molecular science

Fractionation, and DNA synthesis methods comparable in power, if not elegance, to cloning are also conceivable. The deficiencies of our existent techniques need not oblige us to take risks, small risks perhaps, but in truth incalculable risks, with the only biosphere we have.

Restraint need not mean prohibition, but rather a more thoughtful and orderly progress across a dangerous terrain.

Thus, I suggest we can foresee reductionism triumphant — or nearly so — and biology becoming increasingly, although not wholly, a molecular science. And we can see, emergent from this scientific progress, a new and most powerful applied science — a biomolecular engineering, of which genetic engineering is but the first form — intended to shape the world of life to human purpose, as we have already done to so much of the inanimate world.

But that history will not simply repeat. Society is now aware of the process — the progression from science to engineering to technological and social change — and is, determinedly, groping toward some measure of control over the direction and pace of the sequence.

And, unlike inanimate matter, living matter will not stand still after we have reshaped it. It will reproduce itself and evolve as it has always done, in ways probably beyond our skill to predict.

The external world — the larger society — thus is certain to impact upon the future of molecular biology. If I may draw an analogy, the development of the central nervous system provided a means for internal representation of the external world, past and present, which could then help to shape the development and reactivity of the individual organism. This accomplishment clearly proved to be advantageous and adaptive. We shall have to develop analogous means to represent the external world — the larger society — in the development and reactivity of our science, while maintaining at the same time our scientific integrity. And vice versa, we shall have to make known to the larger society the needs, the capabilities and, with insistence, the intrinsic worth of our science. Such developments will provide increasing and varied opportunities for molecular biology to contribute to human welfare.

As scientists we have not been required to think much beyond our immediate scientific problems — and they are often difficult enough to consume our energies and efforts. If we are called now to realize a larger vision, it may be some comfort to realize that it is our own success that has brought us to this horizon. It is precisely the unprecedented potential of molecular biology to reconstruct the world of life, so long accepted as given, that requires us to reconstruct our way of life.

We are becoming creators — makers of new forms of life, of creations that we cannot undo, that will live on long after us, that will evolve according to their own destiny. What are the responsibilities of creators — for our creations and for all the living world into which we bring our inventions? These are novel questions to ponder.

It is but 32 years since the discovery of the chemical nature of the gene. That we are today discussing how best or whether to deploy genes is the measure of how far we have come and how fast.

We may look in the index of the future under "whither molecular biology" but, with concern — and without conceit — I suggest the answer may be found under "whither humanity." □

Jesse W. M. DuMond

1892-1976

A Tribute by Felix Boehm

JESSE WILLIAM MONROE DUMOND, professor emeritus of physics, died in Pasadena on December 4 at the age of 84. He had been associated with Caltech since his freshman year in 1912.

DuMond was born in Paris in 1892 into an American family of artists. He spent his childhood in Paris and in Rochester, New York. In 1916 he graduated from Throop College of Technology, as Caltech was called then, with a degree in engineering, the only option offered in those days. The following two years were spent as an electrical engineer at General Electric in Schenectady, New York. In 1918 he enlisted in the U.S. Army in France in a sound-ranger regiment commanded by Colonel Lyman, the famed physicist from Harvard. After a brief year with Thomson-Houston Company in Paris and at the National Bureau of Standards he rejoined Caltech in 1921, the year Robert Millikan took charge, and earned his PhD in physics in 1929. So great was his idealism and his devotion to Caltech that he turned down an associate professorship at Stanford to remain here as a research fellow, although Caltech did not pay him a salary until he was appointed associate professor in 1938. He became a full professor in 1946.

During 1937 DuMond extensively visited the laboratories in Europe and the USSR where the new physics took shape. The great pioneers of x-ray physics such as Bragg, Maurice de Broglie, Auger, von Laue, and Ewald left a strong personal imprint on him.

DuMond's early scientific life was under the influence of Millikan, the Chief, as he was teasingly called by his associates; this influence was the basis of DuMond's deep involvement with the fundamental constants in physics. His fascination with the constancy of the laws of nature in relation to each other bore its first fruits in his important work on the Compton effect, and the determination of the Compton wavelength, followed by the determination, from the continuous x-ray spectrum, of the relationship between Planck's constant h and the electron charge e. "Here at Caltech is a rather young physicist, DuMond by name, whose work, in my opinion, ranks among the most significant accomplishments in experimental physics of the last few decades," wrote Albert Einstein in a letter following a visit to Caltech in 1932.

Improving the accuracy of the physical constants required the development of new instrumentation in spectroscopy. DuMond pioneered this development by inventing several unique spectrometers and — resorting to his engineering skill — carried out design and construction himself. The most famous of these instruments is the focusing curved crystal spectrometer, at present referred to as the DuMond spectrometer. Copies of it were built in many laboratories all over the world.

In the fifties and sixties DuMond's spectroscopic work branched off into nuclear physics. His spectrometers in the West Bridge laboratory provided the key to today's understanding of nuclear energy levels in rare earth nuclei. During this period and until past his retirement in 1963 DuMond continued his active role in the field of physical constants in which he was the world authority for 20 years, and for which he was honored by a doctor *honoris causa* from the University of Uppsala and another from the University of Manitoba. DuMond was a member of the National Academy of Sciences. By the time of his retirement he had built up a laboratory with 20 associates, students, and employees.

There was not only DuMond the physicist, but also DuMond the humanist and man of letters. He owed this enthusiasm to his great teacher Clinton Judy, who was for many years chairman of the division of the humanities at Caltech. He delighted his friends and associates with his vast knowledge of French and English literature, reciting poems by Victor Hugo or quoting from Shakespeare. His unpublished autobiography is a literary masterpiece full of subtle reflections on mankind and written with a Proustian sensitivity.

DuMond had strong convictions and an intense feeling for intellectual integrity and justice. He abhorred the spirit of the "establishment" and of the power seeker. "Physics had become big business and with it had come a new type of personality, the man who 'makes history' even though at the same time because of his grossly inflated prestige may also be making colossal mistakes," he wrote, describing the developments after World War II. He was critical of our materialistic society, reflecting in his autobiography, "It is human, though illogical, to evaluate all acquisitions, goods, and services on the basis of what they cost, rather than what they may truly be worth."

He leaves his wife, Louise, and his daughters Adele Panofsky and Désirée Wilson.

Felix Boehm is professor of physics at Caltech.

J. Niles Puckett Jr.

1943-1976

A Tribute by Martha Chivens

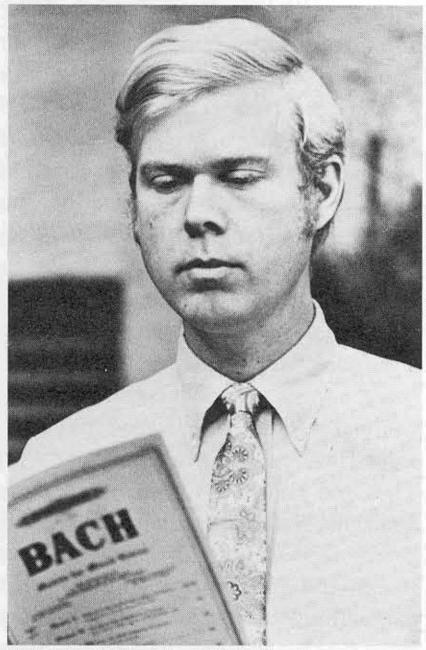
I HAVE READ tributes in *E&S* to people who have made contributions to Caltech throughout their distinguished careers. I wonder if you would consider honoring a Caltech graduate who made contributions to Caltech as a student and would have continued to make them if he were alive today.

J. Niles Puckett Jr. died on May 13, 1976, after a three-year struggle with cancer. He was 33. In 1961 he was a freshman at Caltech from West High School in Phoenix. Although his field was electrical engineering, he was active in the Glee Club and Madrigal group, and in Cinematech. He was interested in photography and languages, and he cultivated numerous Caltech friends. In 1975 at the class's ten-year reunion, Ricketts House had a large representation because Niles had made a few phone calls. No one seemed to notice that at 32 he walked carefully with a cane.

His three Caltech degrees were in electrical engineering — in 1965, 1966, and 1971. The names Hardy Martel, Carver Mead, Tom McGill, Cary Lu, and Steve Kurtin came out in conversations with him during those last five school years. Another special person was named Anna Maria Heneis, from Austria. She became Mrs. Puckett in April of 1970.

Dr. Stephen Kurtin wrote the following tribute about Niles's career: "By education, Dr. Puckett's expertise lay in the fields of circuit theory and electron statistics in semiconductor devices. Upon joining Lexitron, at its formation in 1970, Dr. Puckett was a major contributor to the invention and development of Videotype equipment. In this pioneering development, Dr. Puckett's primary responsibility was the display subsystem, but his influence was felt throughout. The display hardware which Dr. Puckett developed and patented in 1970 and 1971 is still today unequalled in capability and cost-effectiveness. Subsequently, Dr. Puckett designed complex text-processing hardware and led the development of other state-of-the-art advances."

While Niles was working at Lexitron, Dr. Hardy Martel worked hard to encourage him to join the faculty at Caltech. It was an opportunity that Niles wanted to take advantage of, an opportunity that would have realized his potential, but he had work to finish at Lexitron and time was too short. Like a detached scientist watching a rat in an experiment, he watched himself, never complaining.



Niles had a gift for seeing the best in people and saying the right thing. When he died, his friends created a perpetual fund at Caltech to support special Madrigal and small choral ensemble projects, which Niles had helped to start while he was at school. The J. Niles Puckett Jr. Memorial Fund will help the song to be heard that his friends and family sing in their hearts for him.

Martha Chivens is married to Don Chivens, friend and fellow student with Niles. Her father, C. K. Parks, worked in Caltech's accounting department for over 25 years.

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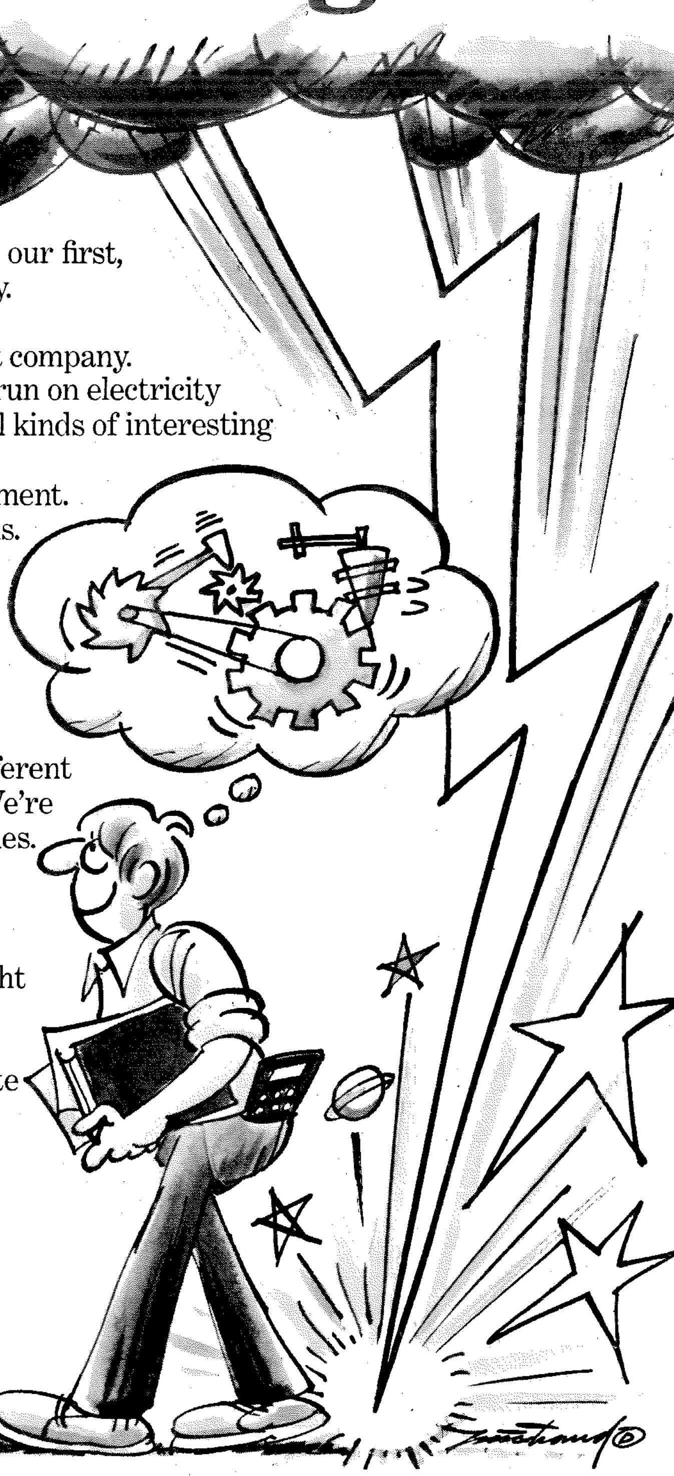

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