Some Dilemmas in Science

by LEE A. DuBRIDGE

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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