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The Global Age: Roles of Basic and Applied Research

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You can recall, I am sure, what we learned about history in junior high school. There was a golden age for ancient Greece, another for the Roman Empire. Then there were the Dark Ages, an age called the Renaissance, and another called the Enlightenment. It seemed history came to us in neat packages; you pulled one down from the shelf, dusted it off, examined the contents for a few weeks, and put it back on the shelf. It was all very simple—and very unreal.

We are wiser now, of course. We know that periods of history overlap, and that societies have almost always been in a state of evolving into something new. We know that each historial period, even at its zenith, was a time of conflicting ideas and competing loyalties. We know that the major ages as delineated by historians are distillations. As such, they lack the complexity and pungency of the ingredients that go into time's steaming cauldron. We also know that historical generalizations usually celebrate the thoughts and accomplishments of an anointed few, and they tell us little of the humdrum details of everyday life for farmers and merchants, parents and children, teachers and pupils. Lacking those details, we fail to appreciate how much cultures of the past were like our own.

Considering these complexities, it is hardly surprising that historians like to wait a few centuries before pinning a label on a segment of the past. The present and the near present defy such labeling, because it is difficult to know what ideas, events, and attitudes signal long-term changes rather than short-term adjustments. It is only with the passage of time, too, that one moment in history differs sufficiently from another to make possible comparative analysis.

On the other hand, it is possible that the game of label pinning can be played with greater confidence in times of transition than during the years of maturation of a major historical period. If we look to the discernible future, and perceive reliably that it is going to be vastly different from today and yesterday, then we may have reasonable grounds for declaring that we are leaving one age and entering another. The trick is in saying what the emerging age will be like.

Although my credentials as an historian do not extend much beyond those junior high school packages, I am emboldened to say that I think we are today living through such a period of transition.

The age we are leaving might be called the age of technology. It gave us the industrial revolution—the steam engine, electricty, the automobile, the telephone, television, the computer, and atomic power. These and other technological innovations—one can hardly leave out the Xerox machine—profoundly altered society.

The age of technology saw the emergence of faith in mechanisms to serve the public good. The designers of our republic believed that by setting up a machine of government—namely, the Constitution—the body politic would best be served. Events of recent political history have severely challenged that faith, but our institutions of government appear to have survived well and even strengthened.

The age of technology also gave us "pure" science and the accompanying article of faith that if researchers in basic science are left alone, there will automatically flow to society technological innovations for the public good. That belief has eroded in recent years, as you no doubt appreciate.

The age of technology has hardly run its course. Futurologists are reluctant to prognosticate beyond the beginning years of the 21st century for very sound reasons: major technological innovations in the past have altered societies in ways that could not be foreseen, and many of the technological innovations of the next quarter-century are themselves unforeseeable.

And yet, I think we are on the edge of a new age. I propose to call it, for want of a better name, the global age. By that title I mean to emphasize the problems of a global scale—energy production and expenditure, population, food production, conservation of nonrenewable resources, and greater equity between rich and poor nations—will be the ascendant concerns of our best thinkers and our social and political institutions. While people of the world have always been interdependent morally, we now foresee the era when we are interdependent literally for our mutual survival in a reasonable world.

I am not very happy with the term "global age." It suggests a finality, a consummation of human history, as though either nirvana or doomsday will be its final curtain. But I leave it to professional historians or politicians to come up with a better term. Certainly it is only in this century that we have begun to think of our planet as one huge piece of real estate which we had better learn to manage well or else face calamity of one sort or another.

Forceful realization of this situation came to international attention several years ago in what some have called doomsday economics. More recently,

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however, a team of economists headed by Nobel laureate Wassily Leontief reported the possibility of a cautiously optimistic scenario for the future of the world. You may share with Leontief the confidence that we can enter the 21st century with ample resources and a declining gap between rich nations and poor, but given the drastic changes in political and social conditions that would have to occur, there is little ground for optimism.

At best it seems we face a precarious future, walking an almost daily tightrope between a host of potential catastrophes. Given this fearsome prospect, granted that no one I know is wise or bold enough to advance total solutions, what generalizations should guide the American science community in the global age? Here I advance three generalizations, hoping to influence you individually and, through you, your organizations and institutions.

Role of Basic Research

My first generalization is that we must reaffirm, maintain, and strengthen our commitment to fundamental or basic research. In recent years many of us have been concerned with a decreasing federal level of support for basic research. Now I am pleased to note that President Ford and the Executive Branch responded to our hue and cry, for the last Ford budget proposed, for the National Science Foundation, an increase of 3 percent above an inflation allowance for basic research.

It is difficult to discuss the value of basic research without lapsing into clichés. But let me quote from Louis M. Branscomb, who speaks to one aspect of the value of basic research: "We know by now that man's presence on earth is having a major impact on the world environment. Whether or not the impact will prove to be benign or catastrophic depends on how well we understand the nature of the impact and how much we can expand the variety of technological alternatives from which our society may choose. The need for rapid technological process will not diminish. But will the institutional structure for science and technology be adequate to the need? Will the pace of scientific discovery continue to expand? Most important, will public confidence in the ability of the human society to work out its going problems be sufficient to sustain the effort required to justify that confidence?" Branscomb continues: "The answer, of course, is that economics cannot change physical law. It can only provide the institutional environment within which the full power of scientific imagination can be brought to bear on the problem. The richness of science, more often underestimated by scientists themselves than anyone else, has consistently proven adequate to the need, once that need was clear and persistent."

It would be tragic if the young, or generations to follow them, refused to support basic research because of a myopic failure to see basic science as an investment in long-term solutions to human problems. The National Science Foundation expresses our republic's faith in the ultimate worth of basic research. The Foundation and other agencies that invest in research must be vigorously supported if the United States is to continue its great humanitarian role, for as I survey the several major world problems, I am struck by the need for fundamental investigation in every subset of them. It is generally true, and certainly emphasized in fashionable essays, that the critical elements in at least first-order solutions—population control, for example—involve basic political and social decisions regardless of the specific science and technology applied. Of course this is a valid statement, but I worry that this realistic view tends to obscure the value of research. After all, the products of research have historically often cut the Gordian knot of many political and social problems. While we now appreciate that science and technology do not provide total answers, basic research is a critical component of virtually every approach to our major problems.

Nowhere are the results of basic research more dramatically evident in our daily living than in the broad field of electronics. And nowhere has the interaction between the basic and the applied been so fruitful for society in general and the individual in particular. A recent special issue of *Science* was devoted to the continuing electronics revolution, and I quote Philip Abelson and Allen Hammond on the subject (*1*):

"The electronics revolution represents one of the greatest intellectual achievements of mankind. Its development has been the product of the most advanced science, technology, and management. In many applications electronics requires little energy. Indeed, one of the factors that guarantees enduring impact for the electronics revolution is that it is sparing of energy and materials.

"With electronics one can control the disposition of large amounts of energy and force, but much in the way the brain is used in directing the action of muscles. In some aspects, electronics can be more subtle, more nimble, more dependable than the brain. In other applications, electronics serves as a great extender of human capabilities by rapidly carrying out routine but complex calculations, thus freeing the mind to make intuitive judgments and find shortcuts to new insights . . . .

"One of the factors favoring the development of electronics has been a comparatively high degree of social acceptance. There have been sporadic attacks on various electronic devices such as computers and there is continuing concern about privacy, but the intensity of criticism has diminished. In comparision to the number of objections raised to chemical products, to the environmental concerns associated with nuclear and fossil fuel energy, or to fears of recombinant DNA, objections to electronics have been few. . . .

"Items that have recently become broadly available, such as the hand-held computer, electronic watch, and citizens band radio, enhance the public's feeling of participating in the benefits of electronics while not bringing with them discernible side effects. In future, electronics will provide many new tools useful to the general public."

In my opinion the argument "science for the sake of science" ultimately always breaks down, for by my definition basic research—the research that made the electronics revolution possible—is always relevant in one way or another. Jacob Goldman makes this point well: "In my humble judgment, the present generation is the first in which the driving, motivating force of science and scientists is basically not science as a goal unto itself, but rather as a means to an end. The entire fabric of support to science today is intertwined with a rationale that it is useful for the solution of practical problems." Society has decided, by and large, that basic research is useful and must be supported by tax dollars. It is up to all of us to reinforce that view by constantly pointing out the relevance of our work in the pursuit of a better, more equitable world.

It is just as clear, however, that this faith in the value of basic research will no longer be blind and uncircumscribed. The continuing controversy over recombinant DNA research has brought us to the threshold of the new age and is a good case in point. While I disagree with Robert Sinsheimer on the subject of the National Institutes of Health guidelines—if anything, the guidelines are overcautious—I agree with him on the significance of the controversy. As Sinsheimer said in a lecture to the Genetics Society of America, "To impose any limit upon freedom of inquiry is especially bitter for the scientist whose life is one of inquiry;

but science has become too potent. It is no longer enough to wave the flag of Galileo.'' I should add, as more than an aside, that the AAAS is very much concerned with the recombinant DNA issue and will provide a platform for reasoned discussion. We plan a major symposium in Washington in 1978 to provide both the public and the science community with a balanced, fully developed presentation.

When I generalize about basic research, two specific points trouble me deeply. In the first place, I am worried about the fiscal health of our leading research universities. It is no secret that many, perhaps most, of these universities are in financial trouble. Because so much of this nation's basic research takes place in a university setting, the health of American science is inextricably intertwined with the health of its universities. If our universities are of high quality, so, too, will be our basic research. And let me add parenthetically that when I say healthy university I mean healthy across the board; that is, academically strong in the fine arts and humanities as well as the social and natural sciences. Furthermore, I maintain that unless the core arts and science departments of a university are strong, its professional schools will inevitably be weakened. The problems of a research university these days are varied and complex, but I am completely convinced that more adequate federal formula grants would be at least one very positive factor in maintaining university integrity and solvency. Money alone is not the complete answer, but every university president I know believes it would be a good start.

A second concern, again under the general rubric of my basic research generalization, has to do with our young scientists, particularly in the colleges and universities. I believe it is time we provided federal funds to support our best young academic researchers in their first two or three years of appointment. As it is now, these young people are ill-equipped to enter national competition, and as a result may never have the released time for research. Funds should be awarded to departments, and they should choose the recipients from among the young assistant professors. After several years of this support, the better scientists can then compete in the normal national peer review process.

Problem-Oriented Research

Basic research, however, is but one facet of the nation's research resources. Another is what can be termed multidisciplinary, problem-oriented research, and this brings me to my second major generalization: the need to accept, support, and reward members of the science community whose contribution is in this less traditional vein. Of course, problem-oriented research involves basic research, but it also involves engineering and applied work. And if we are to meet the major challenges of the global age, research focused on solutions to problems has to be well organized and sensitively managed.

In this regard it should be noted, to the credit of the scientific community, that the present safeguards for recombinant DNA research were devised by the community itself. It seems to me that the scientific community must take cognizance of the new priorities of the coming age, or face the prospect that others will make the decisions for them. The problems of the ''global village'' are so often technological, and if the technological solutions are not forthcoming, society may attempt to ''force-feed'' solutions by shifting funds from basic research into technological development.

The day is coming, in other words, in which the scientific community must, instead of ''waving the flag of Galileo'' and saying that all scientific inquiries are of equal value, decide priorities—giving the push to research on the verge of a socially beneficial breakthrough and shifting to a smooth idle research subjects of less social pertinence. I used to think such distinctions impossible to make, such priorities impossible to establish, but experience has convinced me otherwise. It is perfectly clear that if we do not indicate our priorities, others perhaps less competent will make them for us.

My third major point is that the scientific community must improve the articulation along the route from basic research to technological application. There is, of course, a continuum between basic and applied, although it seems to vary from discipline to discipline. For too long we have lived with an overly strict, sometimes snobbish, dichotomy between the basic and applied sciences. The related segments in each need to know what the others are doing.

I believe a new kind of research enterprise is likely to emerge in the coming age: large-scale operations designed to grapple with problem-focused, rather than discipline-focused, issues. We must learn how to mobilize and manage such enterprises. Some aspects of problem-oriented research will best be handled by universities, others by federal or federally supported laboratories, others by industry. In these realms, too, barriers to communication and articulation must come down.

At the same time, we will need to develop more sophisticated classifications of problems, especially into long-term and short-term ones, and have the capability to assign the most effective institutions to the appropriate tasks. I do not mean to suggest that all basic research be subordinated to problem-oriented efforts, but such undertakings should include a healthy amount of basic research along with interdisciplinary efforts all along the route to practical applications.

Lead Institutions

The best way to orchestrate such undertakings, I will suggest, is through what I call lead institutions or consortiums of institutions. That is, a particular entity—if not a university, then a laboratory in the government or the industrial sector—would be assigned responsibility for research and development on a manageable segment of a societal problem—a segment for which it demonstrates particular strength. It would not monopolize research efforts in a specialty, but foster coordination of research efforts throughout the country. I want to emphasize that these will be very large projects and cost considerable money. Furthermore, it is essential that there be stable funds for a set period of years; I suggest 10 years with an in-depth review after the first 3 to 4 years. If progress is satisfactory, additional forward funding should be granted. Once a grant is made, the consortium—not the agency—must make the decision on the funding of specific research projects deemed essential for accomplishing the total task.

On the surface, it might appear that the lead institution concept would strengthen the Harvards and the Berkeleys at the expense of second echelon universities. Quite the contrary is possible. I can think of one small midwestern university, for example, which has a special research strength—namely, in adapting industrial machinery to the capabilities of the physically and mentally handicapped. There is no reason why such a university could not become a lead institution in this particular area.

Certainly it would seem a wiser allocation of research monies to disperse them among universities according to their special strengths, rather than to continue to feed a system in which so many research universities seek the elusive goal of being outstanding in dozens of fields.

Unquestionably, lead institutions would attract to themselves the brightest students in their areas of strength. This is as it should be. Somehow, too, we must devise a federal support system to retain our best young people in a university or industrial research environment. If the marketplace phenomenon of few job opportunities for scientists and engineers is allowed to reign unchecked (that is, appreciably reduce the input of graduate students), the nation will lose that enormous research effort now contributed by graduate and postdoctoral students. One solution to this problem, as I mentioned earlier, would be an increase in federal formula grants, from which we could support these young people.

A direct effect of the lead institution concept that may cause consternation in research universities is that departments would feel pressure to bend their efforts toward interdisciplinary research, perhaps at the expense of "small science," the kind of basic unarticulated research that has been the lifeblood of research universities. While I clearly do not advocate diminishing such research, the pressure, on balance, may be a healthy one. With some very notable exceptions, the traditional departmental structures of universities often remain as barriers to interdisciplinary research. To help us overcome these barriers—and still preserve the departments as basic academic and administrative units—we need to rethink ways of subsidizing our research efforts in the interdisciplinary, problem-focused mode. If the research universities do not adjust to society's needs, society's dollars for high-priority research may simply go elsewhere.

Ideally, the university should be the focus for both basic and interdisciplinary research on long-range issues. Federal laboratories, corporations, and special university institutes and centers would be responsible for the shorter-term approaches closer to practical application.

It will not speak well of the scientific community if we must be dragged into the global age kicking and screaming, with a debilitating case of future shock. If we can protect and strengthen basic research—and you will recall I made particular note of the health of our universities and the support of young investigators—if we can encourage more problem-oriented research and better articulation between research sectors, then, I believe, we can do better than muddle through my so-called global age. In my optimism I think we are bending in this direction now, but my great hope is that these tendencies will accelerate—for our sake and for the sake of future generations.

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Phyllotaxis and the Fibonacci Series

An explanation is offered for the characteristic spiral leaf arrangement found in many plants.

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The spiral patterns of leaves, bracts, or florets of plants are a familiar mathematical curiosity of nature. Anyone who has counted the spirals which catch the eye on the head of a sunflower, or on a pine cone, will have discovered that their number is generally a term of the series 1, 1, 2, 3, 5, 8, 13, 21. . . . This is the famous Fibonacci series, each of whose terms is the sum of the preceding two. Although the study of phyllotaxis (leaf arrangement) goes back to classical antiquity, the attempt to find a plausible mechanism or a mathematical explanation for this Fibonacci phyllotaxis began more recently. It is perhaps to Richards that we owe the most lucid treatment of the subject. In particular, his paper on the "Geometry of phyllotaxis" (*1*) seems to offer a key to the problem. However, it falls short of an explanation: a suggestive diagram and several pregnant sentences culminate in the assertion that Fibonacci phyllotaxis must inevitably occur, given certain plausible assumptions. It is clear that even Richards' authority has not convinced later investigators, for the problem has continued to be regarded as unsolved. In response to this, Adler (*2*) recently proposed a somewhat elaborate mathematical theory. I show here that such complexities are unnecessary, and that a simple geometric argument, in the spirit of Richards' paper, suffices to explain the phenomenon.

A proviso is necessary, however, since this argument only applies when the mechanism which positions new leaves meets a certain condition: loosely speaking, the influence of existing leaves in determining the position of a new leaf must be short-range. Experimental evidence suggests that this is so (in some plants at least), but it is clearly important to know what happens otherwise. Here, simple geometrical reasoning does not suffice, and I have resorted to a computer model. I find that Fibonacci phyllotaxis persists under a wide range of conditions; an observation of some mathematical interest, whatever its botanical pertinence.

The Phenomena

Two types of phyllotaxis predominate in the plant world. One is the decussate pattern, where a pair of leaves springs from opposite sides of the stem at each level, and successive pairs are at right angles. The other, which is my concern here, is the spiral pattern, where there is a single leaf at each level of the stem, and successive leaves make a roughly constant angle, viewed along the plant axis. This spiral, which follows the leaves in the sequence in which they are created by the growing apex, is called the genetic spiral. Near to the apex, or in a bud, the leaves are often closely packed together, and the genetic spiral may be discerned as the shallowest descending spiral. In this situation, each leaf will generally be pressed against two leaves further down the stem, these being called its contacts. In looking at the arrangement of leaves, the eye will tend to follow the sequence of contacts from leaf to leaf, and so to trace out a spiral, of a steeper pitch than the genetic spiral, called a parastichy (Fig. 1). There are two contacts to each leaf, and therefore two sets of parastichies, wind-

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