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Trends and Prospects in Medical Science

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Trends and Prospects in Medical Science

A SEVENTY-FIFTH ANNIVERSARY

CONFERENCE • MARCH 8, 1976

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Foreword

To BEGIN, I want to thank The Rockefeller University Council for having made possible the International Conference on which this book is based, and which was called to consider some of the problems that the biomedical community must face in the days to come.

We are all aware that, after World War II, health research and education grew at an astonishing rate. That growth came easily and naturally, because there was general agreement that the progress made in the biomedical sciences during the exigencies of war should be expanded to benefit all men during peace. This was the recommendation of Vannevar Bush in his report titled *Science, the Endless Frontier*. The nation responded eagerly by endorsing increased federal support of science, particularly in academic institutions. Perhaps the most dramatic recognition of the high priority given to the nation's health needs lay in the establishment of the National Institutes of Health. The NIH pioneered the tradition of the peer review system for screening competitive grant applications on the basis of their scientific merit; it also planned for long-range research on diseases that were of most concern to the country at large. This expansion produced complex diversification in the growing university-affiliated medical centers.

Political and social pressures that followed the Sputnik era brought about an even larger federal role in support of the biomedical sciences, including fellowships for M.D. and Ph.D. students. But the late sixties and early seventies felt the winds of change. Scientific advisory groups at the executive level in Washington became less influential—in some cases, nonexistent. Inflation and social unrest triggered a shift of federal support away from long-range aims toward short-range goals. Federal funding for basic research, except in a few targeted areas, began to shrink alarmingly.

James A. Shannon, former director of the NIH, has pointed out recently that, as more fiscal restraints are imposed, the management of biomedical research demands more freedom of action. To coordinate such research wisely—at either the federal level or within single institutions—a long-range perspective is essential. True, our history of federal support of research has been short in years, but it has been concentrated and constructive. As a result, it is difficult for us to face the insistent financial strains that today inhibit health research, medical education, and patient care. In addition, these financial strains have made it more difficult for the biomedical sciences to be integrated firmly in universities and their professional schools. Many of these difficult questions of planning and coordinating—and of communicating the results and problems of our work—are discussed by the distinguished participants in this conference.

The dramatic results of research in the life sciences over the past thirty years have brought us to the verge of understanding many of the diseases of man that have, until now, resisted all efforts toward prevention, treatment, or cure. Today, investigators in the biomedical sciences stand ready to use the tools that have been developed over those years in an informed approach to the current frontiers in research as well as to medical education and public health. Those tools—and the talents that created them—are crucial resources for the nation's future and for the welfare of people throughout the world.

To consider how these resources can be replenished and put to the best use for mankind is the primary aim of the speakers and panelists who joined us in March, 1976, during the commemoration of the University's seventy-fifth anniversary.

FREDERICK SEITZ, *President*

I

BASIC RESEARCH: THE NEED FOR NEW KNOWLEDGE

Scientific Quests and Political Principles: The Current Crises of Discovery and Government

GERALD M. EDELMAN

Prologue

RATHER THAN EXPAND this speech and alter its style in the interest of a more balanced written version, I have chosen to leave it mainly unaltered in the hope that, along with its flaws, it at least retains some of its original energies.

It may be valuable to point out here that it is more than fifteen years since C. P. Snow delivered the Godkin lectures entitled "Science and Government" at Harvard University.[1] That impressive and prescient exercise was dedicated mainly to matters of executive decision based on scientific knowledge. Its advice still stands, but both science and the social situation have now altered, so that the difficulties we face as scientists and citizens are more pervasive, more ambiguous, and more closely woven into the web of information we use to make decisions in our daily lives. I therefore felt that the subject needed another look, less practical and more concerned with ideological, juridical, and ethical matters than Snow might have deemed sensible.

The main change in scientific understanding is related to the impact of molecular biological discoveries upon our view of ourselves. But the main reason for an addendum to Snow's views is a kinetic and practical one: the rate of accumulation of discoveries and the ease with which they may be abused have increased greatly. The change in emphasis from military devices to ourselves as creatures and the increase in the speed and ease of uninformed applications of scientific technology shifts moral concern from war as our major worry to that of the whole condition in which we live. And, as Snow pointed out, the problem is therefore proportionally more difficult, because in that case our objectives are not so clear.

On this important anniversary of The Rockefeller Institute and now of The Rockefeller University, I have been given the privilege of considering the need for new scientific knowledge. I have chosen to assume that at one level there is nothing to discuss: in men, the need is as immanent as curiosity itself, and so it will remain. But it is worth discussing, I think, how we exercise that need and against what odds, how we value that need, and how we combat its pathologies. And so I wish to consider the relationship between scientific quests and political customs, and the crises that arise between our need for new discovery and our need for government under law and precedent in a humanistic tradition.

Perhaps the first question to ask is whether scientific knowledge has limits. That transcendent genius and wily commentator on science, Albert Einstein, was, I have heard, once asked whether science could explain everything. He is supposed to have answered yes, but to have questioned the utility of the pursuit. It would be, he reflected, like describing a Beethoven symphony in terms of air pressure waves.

I personally do not believe that the efforts of science are exhaustive of all knowledge. Whatever its limits, however, scientific knowledge is the most intrusive and pervasive of the modern forms of knowledge. As I hope to explain, that pervasive quality has led us to a historically unique predicament that I believe to be one of the most important consequences of the secularization of the modern world. That predicament is the potential conflict between the disciplines of scientific discovery and those of legal practice and government.

One Culture, Two Disciplines

Basic science is now on the defensive. It is being assailed by groups and governments as being costly and dangerous, as being silly or ominous. Now that modern biology, for example, has achieved some mastery over molecular genetics, various scenarios of deliberate genetic intrusion into the human gene pool are being rehearsed, almost always with more anxiety than insight. This is a dangerous state of affairs, not because we scientists may lose our grants if people become frightened enough, but because it reflects upon a larger intellectual failure of modern society. This is not just the failure to appreciate the ethical implications of science, but rather the failure to understand the fundamental process of basic research and its relation to our historical and legal heritage.

Let me make this point trenchantly by a sweeping statement: In no age of Western history has a philosophical procedure been so tacitly accepted and used without understanding as has science by modern governments. As a result, the ever-growing influence of scientific invention and technology has terrified us as much as it has given us peace. Why is this the case and what can we do about it?

I believe that it is the case because the people who govern and who make the laws are no longer in a sensible position to take advice from a Mandarin bureaucracy of scientific specialists and then make wise decisions based on this advice. The legislators are in that predicament because their present education and legal discipline simply exclude any understanding of the procedures and disciplines of scientific research.

But why is *this* the case? I believe it is the case for three reasons. First, science is not so complicated as it is abstract and far removed from ordinary sensory experience. In other words, like law, it requires discipline and training to become familiar with its abstractions. Second, only recently have historians and sociologists of science begun to show us the difference between the generative or creative aspects of science and its formulation and application in teaching.[2] In the absence of their findings, science is usually looked upon as a dead collection of facts, rather than a characteristically human cultural activity. And third, legal precedent, which is the pillar of Anglo-Saxon law, so far fails to reflect the pervasive influence of new scientific knowledge upon our lives and societies.

In the face of these difficulties, we are in a curious situation. For the first time in history, large nations are governed by persons who, in general, share a common belief with those they rule in a rationalistic scientific tradition and its economic consequences, but who are not in possession of a general knowledge of its procedures. Cosimo de Medici had, I would guess, more understanding of the church and its procedures than a modern president has of the procedures of science and its associated industries. We do not have two cultures, but we do have two disciplines, the politico-legal and the scientific, and they very rarely intersect. Indeed, they are in potential conflict.

Science, Antiscience, and Ideological Extremism

This state of affairs is a problem not just of education, but of ideology, and it has given rise to extreme ideological positions. Although there are many, I shall cite just two that I believe represent the extremes of what we may call scientism and antiscientism. One attacks ideology and the humanistic and legal positions in the name of science; the other attacks present-day science and its values in the name of humanity. I shall not hide the fact that I believe both of these extremes are in error.

In the last chapter of his book *Chance and Necessity*,[3] the eminent molecular biologist Jacques Monod proposes that political or religious ideologies that are not based on scientific knowledge are "inauthentic." He proposes, indeed, that we accept scientific verification as the only authentic basis for constructing a political structure, in his case a rational socialism. This extreme view contains within it a serious anachronism, as well as a failure to appreciate the generative nature of scientific inquiry. In confronting the future, we proceed in science, as in everything else, with a surmise rather than a formula, and with no guarantee of success. If we had to wait for everything to be verified in a laboratory as authentic, daily life would come to a halt. Furthermore, the very beliefs from which individual scientists successfully proceed are often ideological[4] and thus inauthentic by Monod's definition. They are embedded within a historical and ideological tradition that is larger than science itself and, given human psychology, I believe that this will always be the case.

The extreme scientism of Monod is, in fact, itself an ideology, and it is in the tradition of a long line of technocratic political prophets, such as St. Simon and Comte, who have attempted to drive their scientific views too far. Given the larger frames of history, and our ignorance of the future, scientists are in no position to run society or even to set its values.

As extreme as Monod's position is, however, it is not so completely misinformed as is the antiscientism expressed by Mr. Lewis Mumford in his book on techniques and civilization, entitled *The Pentagon of Power*.[5] Here we meet the other extreme: Political absolutism, Power, Productivity, Pecuniary Profit, and Publicity (together comprising his Pentagon) are all assailed by Mr. Mumford with disgust as the reflection of a modern disease that results directly from our scientific traditions, from what he calls, in fact, the crime of Galileo and the mechanical world picture of Descartes. The real crime of Galileo is not, according to Mr. Mumford, the one for which the church persecuted him. His crime was "to trade the totality of human experience . . . for the minute portion which can be observed within a limited time span and interpreted in terms of mass and motion while denying importance to the mediated realities of human experience, from which science itself is only a refined ideological derivative."

Mr. Mumford systematically attacks the mechanical world picture first put together by Kepler, Galileo, Descartes, Newton, and Boyle, and he identifies it as the origin of our modern predicament. He then substitutes in its place a vague organicism based on the undefined attributes of the human mind. He gives to the organism as a working whole "in all its indescribable capabilities," as he puts it, the role that Descartes gave to the machine. In any case, he notes that the Galilean crime and the Cartesian poison of mechanism are no longer accepted by scientists since the dethronement of simple mechanism by modern electromagnetic theory. In fact, it is the idea of reductionism, that the world can be explained in terms of its parts, that Mumford attacks so vehemently. This attack is based on the implicit assumption that the properties of the human mind will resist explanation in reductionistic terms.

Such views are not only in error; they are downright dangerous, for they propose that Western science is the main cause, rather than a result, of great modern historical movements and predicaments. Indeed, they place the reductionist scientist in the Star Chamber as the great modern heretic. I am afraid that to agree with this view is to support the forces of mysticism and anti-intellectualism in the name of an apparently moral set of values. Mumford's views are strong and simple: Listen to the scientists and you are doomed. Believe in organicism, vitalism, and a teleological universe consisting of purposeful organization and subjective intentions, and you are saved.

But, in fact, Mr. Mumford's position does not stand up. Modern biology has shown quite convincingly that, so far, there is no limit to methodological reductionism and to mechanism. So far, in biology, mechanism has *not* been dethroned—instead it has been enriched by examples unimagined by physicists, but not contradictory to their over-all view of the world. This biology has shown us that long-chain, information-bearing polymers such as DNA are the molecular basis of genetics and inheritance, and it has provided a molecular basis for evolution and natural selection without a need for additional mystical fields and forces to explain living systems. As for the dethronement of mechanism by modern physics, it is only a metaphor for a larger, more inclusive view that certainly does not imply teleology or a mind that works outside the laws of thermodynamics or a basic procedure that is any different from that of its scientific predecessors.

The Ultimate Search and the Clue to the Brain

What, then, is the difficulty? Why are we faced with so extreme a set of positions in men of good will but of different disciplines? I believe it is because the search for new knowledge has not yet fully committed itself to take on the most challenging and important of its tasks. That task, which I shall call the ultimate search, is to understand the workings of the human brain and the nature of the developmental processes of higher organisms that give rise to that brain and particularly to language. It seems to me that it is because we have simply no idea of how the brain works to produce ideas or how it develops as an organ that there is a constant, erosive, and dualistic conflict in our philosophy and in our understanding of the relationship between science and ideology.

By this I do not mean that the study of the brain should particularly improve conventional psychology or neurology or even philosophy. I mean that the search should be for no less than an understanding of imagination, recall, and perception in terms of the structure of the brain and its molecules. To be scientific, the understanding of these mental processes must take place within the frameworks of the theories of natural selection and evolution, and of modern physics.

We do not know how this ultimate search will come out. Even if it is as successful as modern molecular biology in revealing new mechanisms, however, it does not imply a dictatorship of our sensory existence or a debasement of our human freedom. Indeed, I would guess that such a success would place us in a decent position to understand the relationship between ideology and science without falling upon the horns of such extreme positions as those of Monod and Mumford.

Having gone this far, I would like to hazard a guess as to how the ultimate search will come out. It will come out, I believe, that our brains depend upon selective systems. In other words, like evolution, the immune system, and every other system that has to cope with an unknown future in terms of the recognition of information, our brains will be found to contain an enormous repertoire of arrangements from which those that fit will be selected. And I will also guess that there will be a specific and definable set of circuits quite uniquely evolved to render our brains sentient and self-aware.

Suppose for a moment that this is so. Is a knowledge of this mechanism debasing? On the contrary, human freedom will remain the same if it is discovered to be the case. All selective systems have an enormous repertoire of choices from which particular selections of the best-adapted possibilities can occur. Of course, that means that many things in the brain's repertoire will turn out to be contrary to fact. It is important to understand that in a selective system they nevertheless must be there. Indeed, I would argue that this kind of system is the basis of human freedom—not teleology, but grammar and the imagination. The freedom is in the grammar. If the circuits of the brain operate selectively, then even contrary-to-fact ideas must be in the repertoire and ideology is again made valuable.

But this does not mean that ideology itself must be enthroned; science is always threatening to ideology and an ideology must always yield to demonstrated fact, as Galileo showed. But to carry us on, to serve as the origin of new ideas and of diversity of experience, and to give us hope because we have no choice but to hope, ideology is necessary to our daily lives.

The Fusion of Disciplines: Testimony and Education

When we understand better how the brain works, I believe that we will be able to understand better the relationship between scientific facts and facts that are not susceptible to verification in a laboratory. This brings me back to my sweeping statement about science and the law. Until we have some scientific understanding of that great inner frontier of knowledge, the brain, which is the basis of knowledge itself, what are we to do? We must, it seems to me, try to find ways to mix the legislators and the scientists and to encourage mutual comprehension of their two disciplines—the discipline of procedural investigation against a background of precedent and the discipline of scientific investigation.

Contrary to the opinion of C. P. Snow, the culture underlying these two disciplines is not two cultures, but it must be admitted that the two most important practical disciplines of that single culture are still largely sealed off from each other. The result is that lawyers and legislators who are used to dealing in the domain of values want to hold adversary proceedings to decide what has already been demonstrated by scientific procedure. And scientists, who use their method, tend to defer or deny the ongoing domain of values and disputed fact, and therefore avoid some of the major issues in our lives. The upshot is confusion, bad will, and a large sense of loss in both communities.

How can we deal with the separate customs of the disciplines of science and the disciplines of government? On the one hand, we do not wish scientific experts or technocrats to manipulate our society or solely to define its values. On the other hand, we cannot accede to the pastoral fantasies of certain antiscientific ideologists, nor can we rest comfortably while an uninformed set of legislators swayed by those ideologies make key decisions on the control of technology that affects our economics, our health, and our lives.

It seems to me that there are short-range and long-range approaches to this problem. The short-range solution has to do with modes of presentation and communication with each other, whereas the long-range solution requires revamping of our educational systems in law and in science.

The short-range problem is one of testimony.[6] Legislators are used to adversary proceedings as a key mode of making decisions in difficult matters. Indeed, in a recent article on controlling technology in a democratic society,[7] Arthur Kantrowitz has suggested that adversary proceedings are the only way in which scientific issues bearing upon society can be democratically handled, and he suggests a form for these proceedings that is novel. In confronting the problem that moral responsibilities can change in response to new facts, he argues that, in most cases, the scientific and moral components can be separated. In an approach to the dangerous susceptibility of the uninitiated legislator to the scientific expert with fixed ideas, he proposes that the Congress set up an Institution for Scientific Judgment, in which the judge is a broadly trained scientist listening to the possibly conflicting claims of more narrowly trained scientific experts. And to handle the problem of public responsibility for the role of these scientists in decision-making processes, he proposes general publication of all of the scientific judgments of this Institution.

The essential difficulty of such proposals for a "Science Court" is to decide who shall conduct the proceedings—the scientists or the lawyers. Resolution of the difficulty can only come by testing various alternatives in real situations. For example, at what level of the judicial structure should the deliberations be aimed? How are the appropriate balances between the judicial aspects and scientific aspects of a given case to be struck? Moreover, without some form of review, how can we be assured that an understanding of the scientific aspects of the reports of this procedure has truly been reached?

I suspect that a single procedure such as Kantrowitz's will not solve all these problems. But a test of various possibilities in the law schools, using various procedures, might give some real indication of how to approach these difficulties. Such "courts" might be held for one major situation (e.g., the problem of recombinant DNA) in three or more different forms in as many schools. A coordinated report on the experiences and the opinions reached and submitted to the National Academy of

Sciences, to appropriate agencies of the Congress, and to the executive branch, might be a reasonable basis for evaluating the practicality of Science Courts. I suspect that a pluralistic approach resulting in a structure that provides the opportunity for different balances of scientists and lawyers in different cases will be the practical outcome of such tests.

Whatever their defects, proposals for such Science Courts have imagination and merit. At least they have the possibility of avoiding simple-minded adversary proceedings carried out in the ignorance of the nature of scientific procedures. In such proceedings, scientists are often asked to present conflicting opinions upon moral issues in a public hearing open to dramatic misinterpretation. Whether it is a problem of the development of a new drug and its safety, or a problem of the hazards of radioactivity to the public versus the energy needs of that public, there is much misunderstanding whenever these proceedings are held in a courtroom atmosphere.

I experienced that misunderstanding when I testified in May, 1974, on certain proposed constitutional amendments to prohibit abortion before the Subcommittee on Constitutional Amendments presided over by Senator Birch Bayh. One of the key questions brought up in these hearings was concerned with when life begins. Under the circumstances of these adversary proceedings, in which some prestigious scientists were arguing that human life began at the magic moment of conception, it was almost impossible to point out that the question was operationally meaningless from the scientific point of view. This point, which proceeded from the great advances of modern molecular biology, was essentially a negative one: there is no scientifically sound way to tell when life or "valuable life" begins. Is a virus living? Is a skin cell which contains all the information to specify a human being less precious than a fertilized egg? These are not well-defined scientific questions. In the atmosphere of the hearings, it was much more difficult to defend this *inability* of science to make certain claims than to say glibly that a fertilized human egg is a human being.

Within a different framework, it would perhaps have been easier to point out that determining the stage of development at which an individual human being appears is essentially a religious and moral question. In the charged climate of the hearings, it was difficult to point out that one of the greatest powers of science is to tell us what we cannot do and what we cannot easily define. I came away from this experience with a strange sense of the enormous and problematic differences in the styles of the disciplines of law and the disciplines of science.

I suspect that if preliminary proceedings had been held by a scientist judge and scientist advocates, with the results reported to the legislators and the people, there would have been much less confusion. I note that moves in this direction have recently been made. In the New York *Times* recently,[8] there was a report that an independently funded group of academic experts is being organized to provide advice and facts, and access to experts for members of the Congress. This Institute for the Congress would be free of federal authority, and apparently the idea already has bipartisan support.

This a promising development, but, as in the case of the Science Court, one still wonders how the results of this group's deliberations will be communicated effectively to the lawmakers. This problem remains, and it is, I believe, solvable only by mutual education, mutual influence, and mutual friendship. To get at this problem, we must proceed with good will to change our education in the two disciplines of science and government, to find ways to mix at least some legislators into our scientific brew (not to give us grants but to share our excitement), and to persuade more scientists or those trained in science to join the polity, not as advisers but in political roles themselves. In the meanwhile, it would not hurt if five or six of our best scientific teachers were to give a crash course to five or six senators, while playing golf with them in the interludes. It certainly would not hurt if, in some of our institutions of scientific training, scientists heard more about the law and problems of ideology as they relate to science.

I know how impractical all of this sounds, and am aware that I have not considered here the exigencies of political life, but I am tempted to ask whether the situation could be worse than it now is under the pressures of so-called political realism? This "realistic" point of view has given rise to demoralization in the National Institutes of Health, senatorial denunciation of research grants on the basis of their titles alone, and a glib and foolish faith on the part of politicians that crash programs will cure cancer. Many scientists, for their part, are actually beginning to yield to the pressure and promise anything in order to get support for their work.

In addition to pursuing their research, it is important for scientists to inculcate in legislators a new view concerning the search for new knowledge and its implementation for public needs. My belief is that this view must support basic inquiry, not as a product or as a cure, but as a freedom, a freedom necessary to the search for discovery, akin to the freedom necessary to imagine a new future. The search, of course, must be practical, as well, but it simply cannot be practical on formula or on demand. Is the production of a particular new drug enhanced by the basic discovery in the nineteenth century of the periodic table of the chemical elements? Not necessarily, but the future of all new drugs depends *absolutely* upon the pervasive usefulness of that basic table to chemists and pharmacologists, even though the table cures no disease. I take as another example my own experience when asked whether a solution to the structure of antibodies ever cured a disease. I cannot say so, but I am convinced it has altered the way we look at disease, has rationalized its parent discipline of immunology, and has provided a broad ground for new discovery in many branches of science.

I shall summarize my position, hopeful that my fellow panelists will correct my extremities and errors.

Science is imagination in the search of the verifiable truth. As such, it is the greatest of man's collective intellectual achievements, for it transcends locale and prejudice. Science is not exhaustive, however; there are other truths. Nevertheless, science must continue to correct the errors of unrestrained ideologies for, whatever one's ideology, ignoring scientific facts is a basically unsound and hazardous procedure. In other words, there is no turning away from our modern acceptance of science and its products and its mechanistic approaches.

The modern heresy is not mechanism and reductionism—it is the encouragement and continued development of two nonintersecting disciplines, one concerned with power over nature, the other with power over men. The heresy is to assume that, from either side, these disciplines are intrinsically at odds with each other or inherently evil.

As I have said, I believe that the local historical agonies we now face will be transcended when, as a result of the search for new knowledge, we understand more of our own biology and the detailed rules of complex, nonlinear evolutionary systems, such as our own brains. Although no utopia will emerge from this understanding, it may help to mend the enormous intellectual schism upon which human practice and human understanding are now based. And, I suppose I should add, that if no utopia will emerge, no dystopia will emerge either. As in the past, our freedom will be threatened more as a result of ideological conflict than as a consequence of scientific interventions.

For the present, nothing is more important than to find new modes of testimony and education, mixing an understanding of ideological necessities with those of scientific practice. In other words, the search for new knowledge must include a stronger interaction between the discipline of law and the discipline of science. Such an interaction requires imagination, good will, and the kind of patience with small failures that is the necessary price of living in a democratic society. The dialogue between scientists and senators is already under way, and it is my belief that the larger enterprise will succeed.

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On Paying for Basic Research

ADOLF W. JANN

LAST YEAR, in accepting Mr. David Rockefeller's most kind invitation to be a panelist at today's symposium, I felt I had to remind him that I am no scientist and have never been personally involved in basic research. My closest connection with the subject is to hold the purse-strings out of which all research is paid for by our company—and, I must admit, I hold those strings rather tightly!

For my part, therefore, let me talk about paying for both basic and applied research, but before doing so, I should perhaps attempt to define what I understand by those terms, because in actual practice the two concepts tend to merge, or at least to overlap at various points, and frequently it becomes extremely difficult to differentiate between the two. To me, *basic* research is the quest for fundamentally new ways of solving problems; in the health-care sector, that means new methods of preventing, curing, or shortening illnesses. Everything else is *applied* research, even if it is so closely connected with basic research as to constitute a continuation of it in a practical way.

The demand for subsidized medical services is unlimited, and in the fields of public health and other welfare projects, successive governments tend to assume more and more obligations, without at first realizing that they are bringing the state to the verge of bankruptcy. When this fact finally becomes clear, they look desperately for ways and means to reduce their health budgets, and basic medical research usually suffers. There is, therefore, a vital need for institutions such as this university and other similar bodies, which are supported mainly by private enterprise, to carry on basic research in those fields where governments are unable or unwilling to provide continuous full support. But we must all recognize that there are limits to what can be achieved without financial assistance from the state.

Unlike governments, which rely on taxation to cover the cost of their health programs, the pharmaceutical industry has to rely on the profits generated by the sale of its products to cover its research expenditure and other costs. The expenditures have risen enormously in recent years, mainly because of the ever-more exacting standards laid down by pharmaceutical control authorities, in particular the Food and Drug Administration, from which many other countries take their cue. It follows from this that great care has to be taken not to exceed budgets, and a large part of the outlay must, of necessity, be channeled into certain specific directions or themes in the hope that new discoveries will not only be beneficial for mankind, but also profit-producing for the industry. These are the facts of life from which there is no escape, and no amount of wishful thinking can change them!

For a long time, many of the researchers in our own laboratories pressed us for freedom to carry out more basic research in conjunction with, and complementary to, their efforts in applied research. They complained that, in the course of their work, they felt hampered because there were too many gaps in their knowledge of fundamental processes to enable them always to proceed with confidence in the further application and development of existing know-how. In this respect, we nonscientists knew they were right, and this, of course, brings us back to the perpetual dilemma of the pharmaceutical industry: where to draw the line between basic and applied research and, when the two are carried on simultaneously, how to control the outlay and keep it within reasonable limits.

After much thought and discussion, Hoffmann-La Roche decided some seven years ago to finance the establishment and subsequent running costs of two independent basic research institutes, one situated here in the United States and devoted to the study of molecular biology; the other in Switzerland, probing deeply into problems and possibilities in the field of immunology. From what I have learned about The Rockefeller University, its aims and *modus operandi* must be, in many respects, similar to those of our two research institutes.

However, because of the financial limitation involved, this is as far as we dare go into purely basic research, although our company, compared with others in the industry, has had more than its fair share of good luck and success in the last twenty years. The scope for basic research is so wide, and the cost of carrying it out so high, that it is a financial burden, the greater part of which can be borne only by the state, directly or indirectly. Even the president of the International Federation of Chemical and General Workers' Unions, whose views rarely coincide with those of the leaders of our industry, does agree with us on this fundamental point. He has a deep-rooted fear that pharmaceutical companies might be tempted to spend too freely on basic research to the prejudice of their financial strength and, ultimately, to the detriment of the interests of the employees for whom his unions claim responsibility.

On the other hand, the pharmaceutical industry has had much experience and success in its applied research programs. The flow to the markets of effective new drugs and improved versions of older, well-tried remedies, has been phenomenal in the postwar years. These drugs have played a big part in holding down health costs by reducing the length of time spent by patients in hospitals or by enabling them to be treated in their own homes. Also, by reducing the period of incapacity through illness, a patient is able to return to work sooner and thus lessen his economic dependence on the state. Nevertheless, when health budgets have to be reduced, it is inevitably the pharmaceutical sector which is expected to bear the brunt of the sacrifice. Nobody dares to suggest lowering the doctors' fees or thoroughly investigating administrative costs, even though these are far higher than the outlay on medicaments. Instead, the attack is concentrated on the one sector which is capable of providing the health service with real economies that could facilitate the funding of its basic research.

I think the successes achieved by the pharmaceutical industry in the postwar years have been due largely to the fact that it is better geared than are the universities or hospitals to carry out specific investigations into limited fields of application. I believe that those of us in industry are more economically minded than those employed by the state in the use to which we put our limited resources. We can also claim that, through elimination of red tape and by building up personal relationships between management and researchers, the latter are encouraged to use their talents more effectively than if they were under state control.

My conclusion, therefore, is that for the future, so far as the division of the responsibility for research is concerned, this has to be shared between the state and industry along the lines dicated by financial considerations. And this can only mean that the state must assume the main responsibility for basic research, while the pharmaceutical industry, within its financial capability, takes care mainly of applied research. Naturally, there is and should continue to be close contact and a continuous exchange of information and personnel between those engaged in research in the hospitals and universities and those who are so engaged in industry. There must be a perpetual dialogue between them.

Here I should give a note of warning. Multinational companies are being criticized worldwide for a variety of nationalistic and political reasons, few of which make economic sense. The pharmaceutical industry, especially, has been singled out by ambitious politicians as a suitable and popular target for attack. This takes many forms, all of which have the effect of reducing revenue.

Before more pharmaceutical companies are forced to curtail or eliminate their research effort entirely for lack of the necessary funds, I hope that responsible governments will have become aware of the importance of having a thriving pharmaceutical industry in their respective countries, and will recognize the great contribution it can continue to make to medical progress on the basis of a living partnership among universities, hospitals, and the industry.

Experiments of Use and Experiments of Light

SIR PETER MEDAWAR

FRANCIS BACON (1674), the first and greatest philosopher of science, drew an important distinction between "experiments of light," intended to enlarge human understanding, and "experiments of use," those aimed directly at the solution of specific practical problems, so increasing still further our power over nature; but Bacon insisted that the power comes from the understanding, and therefore gave unconditional priority to experiments of light.[1]

I propose to illustrate Bacon's thesis from the recent history of immunology. Why immunology? First, because this campus was once, or is now, the scientific home of Karl Landsteiner, Philip Levene, Oswald Avery, Walther Goebel, Merrill Chase, Gerald Edelman, and Henry Kunkel. Moreover, being an immunologist myself, I like to work the conversation round to immunology if I can.

The New Immunology

Everybody knows that the "new immunology" is the great success story of modern medical science, but from the standpoint of the history of ideas, the really interesting thing about immunology is why its growth was so very indolent between the discovery of complement by Bordet in the early years of the century, and the characterization of immunoglobulins made possible by Tiselius's new technique in the thirties.

The reason is that immunology was an applied science dedicated to quick returns and to operations of immediate practical usefulness. Immunology was a matter of skin tests, empirically improvised vaccines, Wasserman reactions, and antisera whose efficacy was never demonstrated by the exacting tests demanded by modern scientific medicine.

The new immunology began when all this unhappy empiricism, pursued as a sideline by bacteriologists, was swept aside by the work of chemists, microbiologists, zoologists, geneticists, and surgeons who reconstructed immunology on an entirely new basis. The pillars of the new immunology were:

1. The biological basis of self-recognition.
2. The molecular basis of specificity and of information transfer in biological systems.
3. The nature and significance of the subdivision of mankind into genetically distinct groups, recognizable as such by immunological methods.

Its Accomplishments

This all sounds very fine, says the Devil's Advocate, but what of it? Just what are the accomplishments of this new immunology? In briefest summary, they are these:

1. The recognition of the huge medical importance of the miscarriages and maladaptations of the immunological process, and the construction thereupon of a rational therapeutics, e.g., the radical cure of immunological deficiency diseases by what Robert Good calls "cellular engineering," with which I couple the possibility of curing radiation injury by bone-marrow transplants.
2. The interpretation and prevention of hemolytic diseases of the newborn.
3. The interpretation of the origin and nature of blood-group polymorphism generally.
4. Recognition of the dual nature of allergies and the means to cope with them.
5. Tissue transplantation.
6. Most important: the new and deeper insight we now have into the natural defense against tumors, and our best prospect of finding a rational therapy.

The Devil's Advocate now turns in confusion to his client for further instructions.

The Scientific Process

The intellectual processes that lead to scientific discovery are not fully understood, but what *is* fully understood is that there is no such thing as a *calculus* of scientific discovery, no cut-and-dried "scientific method," which can be switched on or switched off at will, to solve our medical and scientific problems.

My message to those who are in a position to promote or impede research by granting or withholding funds is that it is no more than unworldly sentimentality and daydreaming to fund the investigation of some enterprise of immediate practical usefulness without making provision for the basic research upon which the solution of the problem will depend. The history of science shows that it is the shrewd, practical-minded, no-nonsense man of affairs who promotes the welfare of institutions like The Rockefeller University, and it is the worldly daydreamer who thinks that he can solve our practical problems along the principles of the retail trade. We need power over the material world, and we need to understand it, but above all we need to understand that the power comes from the understanding. For this reason, I pray that the future of The Rockefeller University may be as glorious as its past.

REFERENCE

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II

CLINICAL INVESTIGATION AND MEDICAL EDUCATION: PRESSURES FOR CHOICE AND CHANGE

Introduction

WILLIAM M. HUBBARD, JR., Chairman

WHETHER THE CURTAILMENT of the traditional missions of medical schools in both education and clinical research is to be understood as a particular difficulty of these institutions, or whether the decline is related to a general disenchantment with the institutions that have supported traditional societal values, is a reasonable question. Whatever the trends of general confidence in social institutions may be, the role of physician and scientist still are regarded as most valued human endeavors. Research directed to the solution of problems of health is perceived now, as it has been for as long as such data have been collected, to be the most valuable area of research effort insofar as human benefit is concerned.

If one compares the support of clinical research to that of the basic sciences in general, it is clear that, relatively, clinical research is favored. If one compares the resources made available for the education of the medical student with those in other fields, it is obvious that this is the most privileged student group in our society. At the outset, then, we deal with a paradox. In relative terms, clinical research and medical education have never been so generously supported. Nevertheless, the criticisms leveled at these endeavors and the rapid diminution in the rate at which support has continued to grow in absolute terms have created the sense of a crisis. It is encouraging to observe that this period of self-examination under harsh criticism comes at the time of the greatest strength and productivity of clinical science and medical education.

There is in history no model in medicine of a completely satisfying synthesis of the competing interests of education, research, and practice. Basic research; clinical research; the education of medical students; graduate, postdoctoral, and continuing education—all have their internal validity, but depend upon the enhancement of practice to the benefit of the public as the ultimate and essential validation. The relationships among these independent, and yet interdependent, elements of biology and medicine have been irregular, erratic, and, for the most part, inefficient. At its best, the relationship should be a dynamic one that is responsive to the changing external environment. Each of the elements of this dynamic system of biomedicine must be self-protective and self-serving to the point of hedonism. At the same time, however, each element must be submissive to the overriding requirement of contributing to an improved human condition, and therefore must operate in a mood of altruism.

Each element must develop a powerful advocacy as well as an effective defense against the competition of adjacent realms. In so-doing, however, it must be sensitive to the requisite well-being of these adjacent realms, and so avoid seeking its own interests to the point at which a neighbor is made a beggar. There should be a conviction in each of these elements that, if it is isolated from the others, it can participate only in a zero-sum game. Obversely, if basic research, all phases of education, applied research, and the development of scientific understanding to the point at which technologies can be placed into improved practice and are, in fact, symbiotic in their relationship, they fulfill the high expectations of the public. By that means, they will gain the nourishing resources that only public purpose can provide. Academic institutions, departments of government, and private industry must begin to participate in a common purpose if this ideal state is to be achieved.

It is distressing to note that the only example of such cooperation is the period of the Second World War. Under the influence of the binding force of war, all elements of the biomedical system prospered individually while reaching a zenith of productivity collectively. Although the National Science Foundation and the National Institutes of Health were intended to perpetuate the work of the Office of Scientific Research and Development and the Office of Naval Research, this high purpose was lost at the outset as interests began to compete, rather than to cooperate. The concern for creative medical education of high quality, independent of specific research or specialty training goals, still remained the realm of the private philanthropist.

Research funds were provided to the detriment of the integrity of the academic institution, and set up a powerful reward system that pitted the federal treasury against the limited budget of a university and its medical school. Research funds were forbidden to participate in the conduct or enhancement of the medical curriculum, even though it was recognized that the growth of research was in and of itself a powerful influence on the context within which that curriculum existed. Clinical departments continued to be supported in largest part by income from patient care. Within the basic science departments there was a strong resistance to having their disciplines obscured within the medical school curriculum by the students' clinical goals. The stature of the basic science department among its peers was surely not related to collaborative effort in the medical school curriculum. The academic reward system and the scientific peer-reward system offered minimal benefits for education of medical students, rather offering these benefits for graduate and postdoctoral education in the basic sciences and in clinical specialty education. With all of the enormous growth in the size and strength of medical schools, it still remained true—as it does today—that the number of graduates from United States' schools did not meet the community demand for physicians. Furthermore, the number of places available for the study of medicine never has been and is not now adequate to satisfy the needs for qualified students.

The holistic quality of biomedical research from the most basic to its application in the technology of practice must be restored. Only those who are custodians of the discrete interests of each of the elements of this system can formulate that resynthesis. The problems of medical education and clinical research exist in this context. It is part of the legendary tradition that we looked for the enemy, and found that it was us.

The Medical Model: Biomedical Science as the Basis of Medicine

DONALD W. SELDIN

VIEWED FROM THE PERSPECTIVE of science, the present period of medicine is an era of great achievement and even greater promise. For more than 2,000 years, physicians ministered to patients by serving as paternalistic counsellors and comforters. In thus providing care, they achieved an almost saintly role in society, and the bond between doctor and patient came to be hallowed as covenental. Nevertheless, until comparatively recently, medicine could provide almost nothing to mitigate sickness. Indeed, where biomedical interventions were instituted, the consequences were usually catastrophic. The grizzly practice of blood-letting comes quickly to mind by way of illustration.

It was the application of physics and chemistry to biology that transmuted medicine into a scientific discipline of impressive sophistication. Diagnostic and therapeutic tools of great power were developed. New areas of medicine were uncovered. Explanatory and predictive theories, providing deep insight into normal and deranged biomedical function, were elaborated.

From the vantage point of medical therapy, perhaps the most dramatic expression of this scientific revolution was the great triumphs in the field of infectious disease. Appropriate drugs have provided curative treatment for pneumococcal pneumonia, meningococcal meningitis, tuberculosis, syphilis. Vaccines can prevent smallpox and poliomyelitis. Some infectious diseases not amenable to drugs or vaccines have been virtually eradicated as a direct consequence of the knowledge of their epidemiology. In all these instances, the cure or prevention of a disease was accomplished by virtue of understanding its cause.

In other instances, full causal explanations are not available.

Partial understanding can result in the alleviation of the consequences of diseases, even though the cause remains. The pathogenesis of high blood pressure is not completely understood. However, the use of drugs which can reduce blood pressure successfully has resulted in relief of suffering and saving of life despite the lack of full causal explanations. Diuretics for edema, phenothiozines for psychoses, pacemakers for heart disease, dialysis and transplantation for kidney disease, respirators for pulmonary disease, and analgesics for pain are representative of the extensive pharmacological, medical, and surgical technology developed to reduce suffering and postpone death.

Perhaps even more heartening than past triumphs is the promise of biomedical science for the future. The enormous power of molecular biology is only now beginning to be extensively applied to the analysis of disease. The revolution in biochemical genetics gives promise for both a deep understanding of a whole variety of disease states, and for experimental approaches to diagnosis and cure. The theoretical triumphs of immunology are only beginning to be applied to the elucidation and treatment of disease. While cancer, heart disease, kidney disease, and stroke are still only dimly understood, we now possess a powerful theoretical framework within which solutions can be sought and with the rational expectation that the quest will be successful.

Despite past triumph and future promise, there has developed over the past decade a rising tide of criticism of medicine in general and medical schools and medical education in particular. This criticism originates not so much in the identification of specific defects in the system of medical education, but rather in a tremendous dissatisfaction with medical care and medical services. The sources of the attack stem from several different directions. On an economic level, the rising costs of medical care have posed serious financial constraints, particularly on the poor. Furthermore, the expenditure on health of more than 8 percent of the gross national product seriously compromises the resources available for such competing social necessities as education, community services, and the like. On a social level, medical care is frequently unavailable or appallingly bad for those living in slums or sparsely populated areas. On a professional level, the physician product of the medical school is regarded as too highly refined a specialist for the kind of primary care so vitally needed. The crucial inference is then drawn that if only medical schools could be appropriately changed, these defects in medical care would vanish. At the same time, it is tacitly assumed that the intricate structure of biomedical science underlying teaching and research would remain unimpaired and even flourish.

In the remarks that follow, the central issue will be an attempt to define the role of the physician in modern medicine. First, I will examine the educational structure of the medical school. Next, I will consider the criticisms of medical schools and their physician-graduates. Finally, I will elaborate a definition of the necessary features of medicine in terms of a medical model.

The Flexnerian Model of the Medical School

The modern medical school is basically a product of the criticisms and recommendations made by Abraham Flexner in his report of 1910. On the basis of a survey of 155 medical schools in the United States and Canada, he concluded that there were too many medical schools, most of them disastrously inadequate. He described them as "mere groups of local practitioners, nominally, if at all, associated with Universities. . . . Wherever and whenever the roster of untitled practitioners rose above half a dozen, a medical school was likely at any moment to be precipitated." He recommended that 120 of the 155 existing schools be closed, and he identified them by name.

His positive recommendations were radical and disturbing. He argued that medical schools should not be keyed to the clinical needs of their communities. Rather, their prime responsibility was the education of students and the transmission and generation of new knowledge. To fulfill those functions, he elaborated a model for the medical school that consisted of the following features: it should be a part of a university; teaching activities should be conducted by a permanent faculty; members of the faculty should be committed not only to the transmission, but also to the generation, of new knowledge. In order to fulfill these primary obligations, he argued, service functions, especially the direct responsibility for patient care, should be severely delimited, else the medical school could not fulfill its educational function as the social agency for the transmission and creation of knowledge. "The moment he [the faculty member] regards his task as that of caring for more and more of the sick, he will cease to discharge his duty to the university—his duty to study problems, to keep abreast of literature, to make his own contributions to service, to train men who can carry on."

Three critical features of the Flexnerian model of the medical school deserve emphasis. First, service functions were undertaken to facilitate the study of disease and the education of students in the understanding and care of the sick, not the direct provision of clinical care for communities. The M.D. degree is a university certification of successful mastery of medical science. Postgraduate training would give direction for the specific use to which that education would eventually be put. Second, the basis of medical education was firmly embedded in the biological sciences, because medicine is, after all, the application of the knowledge of normal and deranged biomedical function to the alleviation of illness. Without scientific training, the medical school graduate could readily become, as I have elsewhere said, an uninformed peddler of scientific gossip. Finally, implicit in Flexner's model is a reductive hierarchy in medicine. The basic disciplines—physiology, biochemistry, pharmacology, and the like—furnish the broad generalizations within an organ or organ system, or link together several organ systems. These generalizations provide a powerful explanatory and predictive framework for the deranged regulatory function characteristic of disease. If medical education is viewed in this framework, it must by definition become dissociated from concerns of a social nature, the solutions of which lie outside those boundaries.

By the 1950s, the Flexnerian model was adopted almost everywhere. Medical schools were firmly incorporated in the university structure; basic biomedical sciences furnished the framework for medicine; service functions were discrete, usually confined to a university hospital and clinic. No longer, as in the past, did medical students feel compelled to go to Europe for high-level education. The current reversed. Students from all over the world flooded to the United States for medical education and training.

The Sources of Discontent

The triumph in biomedical science was simultaneously associated with a growing public discontent with medical services. The crucial inference was then drawn—by the public and their elected representatives, as well as by medical educators and biomedical scientists—that these two circumstances were causally linked. The medical school, with its powerful, biomedical, reductive hierarchy, was responsible for defective medical services, and correction of the former would cure the latter. It is therefore critical to examine the roots of dissatisfaction to ascertain their sources and their relation to the educational and scientific structure of the medical school.

At least four broad arenas of dissatisfaction with medical schools may be distinguished. First, there arose a growing demand for *service functions* far in excess of the requirements of teaching and research, a demand the medical school was not satisfying adequately. Second, it was claimed that medical problems of an *aggregate nature,* problems of pressing social significance, were either ignored or ineptly addressed. Third, the medical school as an educational institution was conceived to be self-serving and inhumane, as medicine came increasingly to be identified with *health.* Fourth, the failure of the *medical-care delivery system* to allocate physicians to locations and disciplines in accord with social needs was attributed largely to an esoteric scientific bias of medical education. All of these issues, it should be noted, arose in the social domain that is the matrix of medical practice.

Service Functions. As originally conceived by Flexner, service functions were sharply delimited. Training programs for physicians were small; ancillary health personnel were few; patient responsibilities were usually confined to the university hospital and its clinic. The medical school today has basically the same general structure and purpose as that envisaged by Flexner—an institution for the education of physicians in medicine. This primary purpose has, however, been expanded and distorted out of all proportion. Enormous programs of predoctoral and postdoctoral education have been instituted in both the broad field of medicine and all specialty areas. The education of ancillary health personnel—nurses, technicians, physicians' assistants—is largely under the supervision of the medical-school faculty. The medical school has become a supplier of personnel, often for the purpose of satisfying important social, but not necessarily educational, needs.

At the same time, the medical school is required to discharge highly demanding service functions in the community—the rehabilitation of sick hospitals, responsibility for clinics and hospitals in impoverished neighborhoods, and for outreach programs to sparsely populated areas. But medical schools are not designed to function in a managerial capacity. The administrative and financial structure of a medical school is fragile; its faculty is appointed and promoted for scholarly achievement, not for business acumen.

Finally, clinical departments are asked to undertake responsibilities for patient care far in excess of the requirements of teaching and research. Often these responsibilities are in locations remote from both medical laboratories and medical students. To insure participation, attractive financial emoluments for such services are permitted.

These heterogeneous obligations have had the effect of disrupting the structure of the medical school as an institution for the education of students in the biomedical sciences. At the same time, the notorious managerial incompetence of medical schools leaves in its wake a trail of unresolved problems that inevitably elicits hostility and frustration on the part of the supposed beneficiaries.

Aggregate Functions. Characteristically, medicine involves a face-to-face encounter between a physician and a patient for the treatment of illness. There is, however, increasing need for social measures to deal with problems of disease. The broad problems of medical insurance, issues of malpractice, the appalling medical problems of the slums are illustrative of issues that have an enormous impact on medicine, yet which cannot be subsumed within the explanatory theories and procedures of biomedical science. These are aggregate problems, and are addressed by such disciplines as economics, sociology, and political science. To be sure, the medical school may have an input into these issues. But its contribution is bound to be subsidiary. Yet so intimidating are the pressures that many medical schools have decorated their faculties with a stray economist or sociologist. Such individuals are usually lost in a medical faculty, however, and tend to blur the image of the institution as an academic structure rooted in biomedical science.

Health. Perhaps the most dramatic manifestation of the dissatisfactions with medicine is symbolized by a linguistic shift in which the terms medicine, medical school, and medical practice are replaced by health, health science center, and health care. The World Health Organization defines health as "a state of complete physical, mental, and social well-being, and not merely the absence of disease or infirmity." Such a definition makes health a virtual synonym for happiness. Health, so defined, has more to do with heredity, environment, social status, or life style than with anything medicine can offer. If taken literally, the definition has seriously coercive social overtones. It may lead to the construal of all types of aberrant behavior as disease. Such medicalization may obscure the fact that the majority of patient complaints do not issue from medical causes (narrowly construed), but rather from deviances from established social and behavioral norms. Alcoholism, drug habits, antisocial behavior, or sexual deviance may have catastrophic consequences for the individual, but they usually constitute problems in social, cultural, and political adjustment, not in medicine.

Even if health could be unambiguously defined, medicine can influence only a tiny fragment of the totality of health problems. Economic, social, and cultural processes have an enormous impact on health. But this influence is exerted through forces over which medicine has no control and cannot alter. The dehumanization and malnutrition resulting from mass poverty is primarily an economic problem. A rise in the standard of living or a social transformation of the slum into a vigorous community would have a far more profound impact on health than any medical discovery. Even those causes for devastating mortality figures that terminate in medical catastrophes—murder, drugs, alcoholism, starvation—have their roots in profound social and cultural disarray. No discovery of medicine—no penicillin, no streptomycin—will have the slightest influence on these tragic social events. Similarly, the rise of alcoholism and a drug culture, the discontent of the affluent with too much leisure, rising divorce rates and weakening of family bonds, the loneliness of the aged—all these may be accompanied by a host of illnesses. So, too, automobile casualties and industrial pollution are major causes of ill health. But the alleviation of all these disturbances lies in the domain of economic, cultural, and social engineering, not of medicine.

Medical Care System. The allocation of physicians and ancillary health personnel is an important determinant of medical care. An overriding national need is perceived to be the allocation, at reasonable cost, of physicians to sparsely populated rural areas and impoverished urban slums. The fee-for-service system seems to insure that physicians go where other physicians are. Clearly, the market mechanism is not allocating resources and personnel in accord with social needs. If we are committed to redress the misallocation of medical personnel, incentives and penalties must be used to alter market forces in accord with our social goals and needs. This will require aggressive intervention at state and federal levels. Again, this is a political decision, not an issue within medical science.

Given these pervasive social determinants, what is the role of the medical school? It cannot transform the slum; it cannot coerce people into norms of behavior they find uncongenial or oppressive; it cannot force people to lose weight, stop smoking, stop drinking, and forsake automobiles; it cannot cajole or force medical students into environments they find unrewarding or unattractive. On the other hand, it has a narrow, but vital, role in the prevention and treatment of illness in specific patients, a responsibility to which no other agency in society is equipped to address itself.

The Medical Model

In Chart I, two functions that describe the role of the physician are listed. The traditional caring role of the physician as counsellor and comforter is here termed a priestly function. Until recently, the relationship has been paternalistic because of the authoritarian posture assumed by the physician and willingly granted him by the patient. Increased participation by the patient in medical decisions may transform the relationship into

CHART I

MEDICINE

Priestly function: Provision of sympathetic care by a physician in a paternalistic relationship with a patient, based on understanding and trust.

Scientific base: Application of the theoretical knowledge incorporated in medical science to the relief of pain, the prevention of disability, and the saving of life in individual patients.

a partnership. Medical knowledge has not been a necessary component of this interaction. Indeed, the priestly function is not unique to the physician. The soothsayer, the medicine man, the various nonmedical healers now widely proliferating, may all be animated with the warmest personal compassion and the noblest social ideals. What distinguishes the physician from all other healers is his scientific base. This is elaborated in detail in Chart II.

The interaction is described as a face-to-face encounter. This automatically distinguishes medicine from public health, certain forms of behavioral and educational therapy, and aggregate disciplines that influence health through social forces. The purpose of the interaction is the treatment of illness conceived as deranged biomedical function. Finally, this intervention involves the application of the conceptual and technical tools of biomedical science.

CHART II

THE MEDICAL MODEL: NECESSARY FEATURES

Interaction: Face-to-face encounter between physician and patient.

Purpose: Relief of pain, prevention of disability, and saving of life by forestalling and treating illness.

Disturbance: Illness viewed as deranged biomedical function.

Intervention: Application of conceptual framework and tools of biomedical science.

Several features of the medical model are noteworthy. First, by insisting that the illness be a biomedical derangement, there is no commitment to include deviant behavior as medical illness. If this should prove to be the case in certain instances, it would be a matter of empirical demonstration through publicly ascertainable criteria. Second, illness, but not necessarily disease, is specified. The physician always treats illness, but his efforts are not necessarily always directed at disease. The two must be distinguished. Diseases, when understood, have causal roots, the elimination of which is curative. But illness need not be conceived as simply collections of discrete disease entities. It is increasingly possible to analyze illness as derangement of regulatory function, the correction of which may restore normal activity, irrespective of the cause. It is this explanatory power of medical science that permits the physician to intervene in countless mundane disturbances, which at times may be life-threatening. Salt depletion, potassium deficiency, hypercalcemia may be serious or even potentially lethal. The correction of such disorders may be curative even when the cause is unknown.

Medicine is a discipline which subserves a narrow but vital arena. It cannot bring happiness, prescribe the good life, or legislate morality. But it can bring to bear an increasingly powerful conceptual and technical framework for the mitigation of that type of human suffering rooted in biomedical derangements. This is a rather impressive achievement, which defines better than anything else the unique role of the physician and the areas of responsibility of the medical school.

Medical Education and Scholarly Inquiry

ROBERT W. BERLINER

THE THREATS TO MEDICAL RESEARCH and education go well beyond that created by the pressure on medical schools to increase their involvement in the delivery of medical care. Very serious problems for the future are being created by a whole series of pressures that are currently being brought to bear on our medical schools. One of these lies in the distinction between health care and medical care. The traditional role of the physician is as the provider of medical care, not as the provider of health care. "Aha!" the critics say. "That is exactly the trouble with medicine; it's not interested in keeping people well—only in trying to cure them when they get sick!" However, the fact is that medicine can do relatively little to keep people healthy. As Dr. Seldin has pointed out, much ill-health derives from economic and social factors far beyond the competence of the physician to remedy.

Another large segment of ill-health derives from the fact that people do things that are dangerous to their health—such as driving too fast, smoking too much, consuming alcohol—and we don't know how to make them stop. "Aha!" say the critics. "That's because medical students spend too much time on the hard sciences and not enough on the behavioral sciences." My response to this is that the behavioral sciences have yet to prove useful in providing acceptable forms of behavior modification. When they do, the critics' accusation may be justified, but it is not now.

Much ill-health relates to noxious influences in the environment. It is certainly a responsibility of medical science to attempt to identify these influences and to lead the way to their elimination. Indeed, public health and preventive medicine have achieved their greatest triumphs in this area. But there is little here for the individual physician, as physician, to contribute except for the occasional brilliant epidemiological insight that leads to the identification of a dangerous environmental factor.

Finally, we have those preventive measures that physicians as individuals can offer to patients. We must note that, once we get past the immunization for infectious diseases, we know precious little about disease prevention. Certainly we should advise patients not to overeat, not to drive too fast, not to smoke cigarettes, and so on, but we shouldn't expect to have much influence, and we don't. We should detect hypertension and treat it when we find it; we should do Pap smears—although, in my opinion, it is not really established that they have had an influence on the outcome of cervical cancer. But let us not delude ourselves or the public that there is much that we can prevent, that early detection has much value in anything but a very limited number of conditions, that it really is worth paying a doctor to keep you well, enticing as that phrase may seem. It may be some day, although it is questionable whether the physician will be the appropriate person to assume that responsibility. I do not think it is practical today.

The appropriate principal role for the physician is, in my opinion, the traditional one: namely, to try to effect the restoration to health of the individual patient. And the appropriate role of medical education is, accordingly, to provide the physician with the background that will maximize his effectiveness in this role.

This means that the prospective physician should acquire a grasp of the medicine of today, not in all its detail, but particularly the principles and the science that give it substance. The physician must be prepared for the drastic changes that will most certainly occur in the future. Most important to his preparation is that he learn to distinguish dogma from truth, assertion from evidence. In exposing the student to the questioning, evidence-seeking attitude, research makes its most important contribution to the process of medical education.

The linkage of medical research to education is threatened by attempts to redirect the careers of physicians by changes in their undergraduate medical education. The tendency is perhaps epitomized in the practice of referring to the medical school experience as the "training" of physicians rather than as education for medicine. We are being pressured to "train" physicians for primary care, rather than to prepare physicians for a career in primary care. Whatever the validity of the currently popular view that American medicine is deficient in primary care, undergraduate medical education is not the appropriate place to remedy it. This is especially true when the preparation for a medical career is interpreted as a dedifferentiation of medicine, a de-emphasis of its scientific content, and an emphasis on the sociological aspect of health and disease. In such a view, the physician-scientist is considered a baleful influence, an inappropriate role-model for the physician-in-training. The student should be shipped off to so-called remote training sites where he will be exposed to preceptorships with those who can and do, rather than be under the influence of those ivory-tower scientists who can't and teach.

As a matter of fact, I believe that most of the pressures for primary care and family practice are based on the nostalgic illusion that things were better in the good old horse-and-buggy days when the physician was thought to have been more sympathetic and attentive. Everyone has in mind that famous picture of the physician sitting helplessly, but sympathetically, by the bedside of the sick child. Many of the older among us had that kind of care and attention. I, for one, would not prefer it over a brisk, businesslike physician with some penicillin, although a little understanding and sympathy would be a welcome bonus. In any case, whatever may be the virtue of a greater emphasis on primary care, medical education should be geared to producing physicians prepared to offer the best that medicine can provide in whatever aspect of medical practice they should subsequently choose to follow.

Then there is the pressure to make medical education responsible for the geographic distribution of physicians. It seems to be assumed that, if medical students are exposed to the best that medical resources can provide, they will become so dependent upon such resources that they will not venture to those places where the resources are not available. It seems to me that the answer to this problem is not to send the students off to remote areas to learn to make do, but rather to effect a reorganization of medical care to maximize the availability of resources more generally. The former alternative will serve the best interests of neither the student nor the communities in which it is hoped they will serve.

The best medical education is that which prepares the student to offer the best that medicine can provide, not to limit his horizons to what is currently available to the medically underprivileged. In that education, an atmosphere of scholarly inquiry is an essential feature.

Education for Medicine in the University

CARLETON B. CHAPMAN

IN ORDER TO ENCOURAGE a climate for useful exchange, it seems to me that a few basic assumptions are in order. The first is that education for medicine in its entirety is of critical importance to our present and future. The second is that there is good reason for concern about the quality of education for medicine, and, in this connection, that quality ought surely to be defined in terms of standards that are genuinely intellectual and professional. And third, we should agree that biomedical research, including the clinical variety, is or ought to be an intrinsic part of the process of education for medicine. We may not all agree as to just how the two should be related, but, for present purposes, that particular point should not be at issue.

With these as given, we come quickly to the vital question: What is wrong with education for medicine? I submit that Dr. Seldin's formulation, correct as far as it goes, is oversimplified and incomplete, and seriously so. He offers two deficiencies. One has to do with the acceptance of responsibility by the medical schools for design and planning of health-care delivery systems that are not, in themselves, basically or even indirectly related to the hard and clinical sciences as classically defined. The other, as I understand it, has to do with alleged departure from the Flexner model and its basic intent. To restore our system to a level of excellence, he implies that the activities of our medical schools in planning or designing delivery systems should be abolished; that we should return to the original Flexner model. One of the other implications seems to be that the classical Flexner-type medical school is not only optimal, but the best that can be devised for our own time. I find myself in disagreement with these conclusions.

The concern of medical schools for improving both the quality of professional services and the delivery system are not of recent origin. But that concern was certainly expanded by the turbulent events and legislation of the 1960s and by the passage of two hastily constructed pieces of legislation during the heady days of the Eighty-ninth Congress. They were the Regional Medical Program Law, originally called the Heart Disease, Cancer, and Stroke Act, and the Comprehensive Health Planning Act. Both followed closely on the legislative watershed that finally bore the title Medicare and Medicaid, and both were overshadowed by it. Legislation having to do with health-care planning, now largely inoperative, failed, and not because the Congress was wrong in turning to academia for assistance in designing a more effective delivery system. It was, rather, because the medical schools were not, in themselves and acting alone, equipped to cope with such a charge. The necessary disciplines and expertise requisite to attack the problem were not all to be found within the confines of the medical school: many of them resided elsewhere in the university. The results were not satisfactory, at least in large measure, because no medical school could suddenly tool up for such a complex and, to an extent, extra-medical obligation without working some damage to its basic *raison d'être*.

And there was another reason. One of the most basic misconceptions was that the medical school should design and *operate* a new system for health-care delivery. That such an effort menaces the primary purposes of academic institutions there can be no doubt, as Dr. Seldin has pointed out. Apart from obvious fiscal reasons, the reason which both Dr. Seldin and Flexner convey is compelling and, indeed, inescapable: the faculty member who, for whatever reason, regards his primary task and duty as caring for more and more of the sick, is less and less able, or inclined, to discharge his obligation to the university. But looking after patients occurs in many settings. There are community clinics, hospital wards, and various types of ambulatory care units, many of them far away, nowadays, from the university campus. There are also private practice units within medical schools, manned exclusively or in large part by full-time medical faculty.

One hears a great deal about full-time faculty who are too heavily engaged in community health activities to permit involvement in clinical research and organized teaching activities. One hears a good deal less about full-time faculty who cannot effectively discharge their academic obligations, including research, because they are engaged in what amounts to a major commitment to private medical care. There seems to be great reluctance to acknowledge that, in some of our medical schools, that commitment is indeed major, for the insidious reason that it generates mammoth personal incomes. This particular feature, and the distorting and limiting effects that inevitably flow from it, is collectively a far greater threat to medical education than is involvement in the analysis of problems of health-care delivery. This feature, more than any other, is likely to return us to the pre-Flexnerian era, if allowed to run unchecked.

Then there is the fact that Medicare and Medicaid are now being administered in such a way as to constitute another major distortion in the medical educational process. This particular problem is bound up with the general question of mounting health-care costs, and the Flexner model, as originally defined, makes no provision whatever for a medical school to concern itself with such things. But are not analyses of, and research on, this urgent topic perfectly proper subjects for attack by the medical schools and their parent universities? Is the question to be answered by the ritualistic assertion that this and other massive medical, social, and economic problems are respectable subjects for study within the university only after time and circumstance have made them matters of purely historical interest?

The quixotic diminution of federal support for clinical research, which has run parallel to the implementation of these various legislative matters, has produced a mixed bag of results, most of them unhappy. But it would be less than forthright not to note the fact that, by the late sixties, as many medical academics admitted privately at the time, the steady increase in research support for medical schools was beginning to produce problems of its own. They included distortion of the organizational and physical structure of the medical school and of the university itself; production of a good deal of inconsequential research along with much that was creditable and some that was monumental;

and the diversion of some young people into areas for which they were ill-suited.

None of these can rationally be said to diminish the enormous achievements of the National Institutes of Health, including what may yet prove to be its crowning achievement, the Peer Review System. That system, in the able hands of the National Institutes of Health, has begun to point the way to the resolution of one of the most pressing social and political problems of our time: How can inevitable conflicts of interest be neutralized, or at least modified, as professional expertise and talent come to bear on public policy and legislation in the public interest? For it cannot be overlooked that the professions—medical, legal, and academic—can confidently be expected to conduct themselves like craft guilds under some important and recurring circumstances. But the problems that were generated by increasingly massive support of research in medical schools, had that support continued unabated, would sooner or later have had to be faced. We would still, ten or so years later, be seriously troubled about deficient quality in the educational process that leads to medicine. Its defects and the seeds of its degradation are more basic, and they antedate the legislative efforts of the Eighty-ninth Congress by many years. Which brings me back to Mr. Abraham Flexner, not to be confused with his brother, Dr. Simon; it also brings the focus onto an early portion of the long sequence leading to the M.D. degree.

Flexner, et seq.

Flexner's main purpose was to impose a university-wide system of education for medicine that would ultimately make a sort of educated elite of the medical profession, chiefly by rendering its members fully literate in bioscience. But less than ten years after his model became the nation's standard, he took alarm. Something had obviously gone very wrong. In 1925, far from expressing satisfaction with the change he had wrought, he said bitterly:

> It is bad enough that all schools alike run a graded four-year course which almost everywhere places upon all students a burden that is excessive and unwise; that schools graduate together those who

> begin together; that all reckon in hours and courses rather than in terms of large, massive subject-accomplishments.[1]

He was obviously grossly disappointed that his report had produced such rigid results and that the emphasis was on masses of descriptive learning, rather than on conceptual mastery. But it was too late to go back. The rigidity and, indeed, the incredible durability of the Flexner model exceeded both his expectations and his noble hope.

The Present Paradox

The Flexner report certainly succeeded in producing a radical revision of education for medicine in the United States. Yet in 1925 he was, in effect, saying that his great work was a failure: it had not succeeded in its main object, which was to render every physician scientifically literate.

But let us agree that Flexner's focus was primarily on an educational exposure of highest intellectual quality, that the same focus is still generally accepted, and that the topic is critical to the considerations that occupy us as we look beyond tomorrow. Then why is it that, at present, virtually no perceptive academic is satisfied with the premedical-preclinical phases of education for medicine, yet all agree that it is difficult or impossible to change them? The reasons usually offered for the persistence of the paradox are that to change would depart from Flexner's lofty intent and would, therefore, degrade quality.

I submit that these reasons are specious, mainly because, far from departing from the Flexner ideal, we have never really implemented it. Education for medicine has not become a pan-university responsibility and interest, and its various elements stoutly resist any move in this direction. The suspicion, and even hostility, that have grown up between the faculties of arts and sciences and those of the medical school have, as much as anything else, defeated the Flexner ideal: ". . . the complete [medical] school in touch with the rest of the university." Instead, the medical school's basic scientists and the scientists in other divisions of the university hold largely to their separate ways, each understanding well enough that bioscience itself moves at its own accelerating pace, but each claiming that separate obligations are so special that neither has much to contribute to the other.

Present-day faculty groupings, precisely to the extent that they place intrauniversity and interdepartmental jurisdictional disputes above genuine academic and intellectual purpose, are themselves responsible for defects in education for medicine. And part of the process of evading serious discussion of primary academic purpose is to resort for purely tactical purposes to the use of such sacrosanct terms as *liberal education, hard sciences, the humanities, soft sciences,* carefully and ingeniously avoiding anything like precise definitions of meaning.

Modern bioscience requires and justifies a very different educational pattern from that which was so effectively espoused by Flexner. We are not, in fact, providing optimal education either for clinical practice or—more important in the present context—for clinical investigation at the premedical and preclinical levels. And one barrier to progress in this regard lies, beyond any doubt, within the university itself.

A good case, in fact, can be made for the proposition that the standard premedical-preclinical sequence conveys only a very mundane and dismally fragmented estimate of modern bioscience to most students heading for medicine.

In recent years, the earnest efforts of some of the nation's best university scientists to remove that which is artificial, and—in actual effect—anti-intellectual, from the interface between premedical and preclinical education have come to very little. This may be due in some measure to justifiable caution in accepting drastic, or even moderate, change. But no one is likely to believe that this is the only reason, or that, in rejecting such proposals outright, faculties are primarily concerned with the student's intellectual progress and welfare.

Whatever extra-university vicissitudes beset education for medicine, the university has now to set its own academic house in order. It should be possible, within the eight years now allotted to the attainment of the M.D. degree, to approach in a modern context the sort of educational exposure and product Flexner really had in mind. They cannot be attained by the introduction of remedial training in areas that belong at the prebaccalaureate level, or by premature and discursive efforts to influence the student opting for the M.D. degree toward heart surgery, research in molecular biology, family practice, mental health, clinical investigation, or primary care. Training for these laudable pursuits is not a viable or logical option prior to the completion of the student's fundamental education, especially in bioscience.

But the whole is a pan-university matter, precisely as Flexner said it was. The ceaseless arguments about whether this or that hard or soft science offering belongs in arts and sciences or in the medical school are not really fundamental; solutions will, in any case, vary from institution to institution. And in citing Abraham Flexner's grand effort, the focus should be on his intent, rather than on the precise preservation of the model he constructed, which, basically, dates to the turn of the century or earlier. Goals, of necessity, have to be less simplistic than Flexner's. One goal is, without apology, to provide abundantly for the gifted student who is capable of becoming a contributing member of an indispensable intellectual élite. The other is to make available, for the larger number of students, a foundation for medical practice and involvement that is superior to that provided by the Flexner model *as actually applied.* The two goals are not separable. If that large number of students opting for practice tends to be more empirical than their research-oriented colleagues, it is patently calculated misperception to say that they have become mere medical soothsayers.

The threats to education for medicine and to its component part, which is clinical investigation, are thus a complicated collection, some arising from social pressures external to the university, and some from intrinsic—even traditional—intrauniversity defects. Beyond tomorrow, the medical sciences and other relevant disciplines will not be defined precisely as they are today, and the medical sciences of today are not the same as those Flexner knew. Yet the total system of education for medicine reflects the latter fact only imperfectly. The system needs to get itself together for the realities of today, then begin to prepare, as well as it is able, for beyond tomorrow.

REFERENCE

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Considerations on the Future of Medical Research

ATTALLAH KAPPAS

THE QUESTION has been posed as to how we can assure first-rank clinical research in the future in light of the extraordinary changes which have taken place in the traditional educational and scientific missions of medical schools during recent years. The question is an important one, because clinical research is the link through which all that science has to offer medicine can be directed against the problems presented by disease in man. It is also an important question because our capacity to provide a satisfactory answer to it lies entirely within our own power—and therefore the extent to which we maintain first-rank clinical research programs in the future will reflect significantly on the ability of the academic medical community to order its own professional life.

Those external forces which have greatly influenced the character of the educational experience offered to medical students—and, indeed, changed dramatically the cultures of many medical schools—have obviously had an impact on the number and perhaps on the quality of young physicians who seek to make their careers in the field of clinical investigation. On the other hand, it is debatable whether the rise of public interest and even passion relating to ethical considerations in clinical investigation has seriously impeded research in man. Certainly, the activities of the clinical researcher must be open to the closest lay scrutiny. At worst, it is not of critical moment that occasionally the scientist's order of research priorities may not be shared by others.

What is especially significant about the question posed is whether we choose to provide the answer to it by ourselves,

Adapted from Dr. Kappas's remarks on March 8, 1976.

whether we have the willingness to focus resources in the field of clinical research, and whether we are able, within present university organizations, to devise administrative arrangements which encourage and facilitate those intimate relationships between basic scientists and clinical scientists out of which the most creative kinds of clinical studies can emerge.

It can properly be said that the term "clinical research" encompasses a very broad range of medical and scientific activities whose ultimate purpose is to understand the normal physiology of man and to make discoveries concerning the mechanisms and treatments of human diseases. But a more specific definition of the term is, I believe, pertinent to this special academic setting. It is that definition which focuses on clinical investigation as an endeavor that takes place at the closest interface between medicine and science, and therefore as an endeavor that has the potential for generating new knowledge, which can flow both toward the practice of medicine and toward the biological and chemical sciences on which the rational practice of medicine can be based. As William Castle has noted in the Lowell Lectures, this special kind of clinical investigation—truly quite "basic" in its essential nature—shares a principal objective of theoretical research as defined by Willard Gibbs, "to find the point of view from which the subject appears in its greatest simplicity."

The history of the institution whose seventy-fifth year we celebrate today is rich with examples of such clinical investigations, as is the history of the medical schools and institutes which many here represent. It is, I think, not necessary to elaborate on such examples. But there are certain defining characteristics of the basic kind of clinical research to which Castle was referring and, in planning for the future, we must keep these characteristics clearly in mind.

First, some reasonably direct focus on disease in man is important in such research. Cori has reminded us that the challenge offered by diabetes mellitus has been one of the greatest stimuli for experimental work in a large number of fields; and one may truly wonder what would be the status of our present knowledge of carbohydrate chemistry and metabolism if this challenge had not existed.

Second, such clinical research fixes on seminal problems of human disease—those illnesses of man whose resolution, even in part, provides important insights into the causes of other human disorders or into the mechanisms of fundamental biological and chemical processes. The reality that medicine and biology are inextricably woven together is nowhere made more evident than in the intensive study of disease in man. It is timely, in the literal as well as in the other meanings of the word, to recall here Alexander Bearn's statement, made in relation to hereditary disease and biochemical genetics, that "In an age which tends to emphasize, and sometimes overemphasize, the debt which clinical medicine owes to the basic sciences it is useful to consider [how the advancement of scientific knowledge of biochemical genetics] has been conspicuously aided by clinical observations."

Third, this basic kind of clinical investigation follows the biological relationships laid down by nature, and not the arbitrary and specialized distinctions of fields devised by man and reflected in the traditional university organization; it utilizes the full array of concepts and techniques of the nonclinical sciences in its probings into the causes of disease and develops new theories and methodologies of great power, as well; it requires of its practitioners the strictest adherence to the moral and ethical precepts of the profession of medicine which we expect of physicians; and it demands, finally, a broad and deep comprehension of the nature of human diseases and how the scientific method may be directed toward an understanding of their causation.

If we wish to assure that first-rank clinical investigation will continue to be carried out in the future, the design of that assurance must be made by ourselves. It cannot be made in Washington or by the society at large; the assurance depends utterly on the values which we impart to medical students and young physicians during their scientific and clinical education; on the worth in which we hold this kind of scholarly enterprise; on the priority with which we allocate funds toward its support; and, finally, on the organizational entities which we devise to encourage the flow of ideas and joint scientific efforts across the necessary, but frequently constraining, academic boundaries which exist in the university.

Of those values with which we must imbue medical students and young physicians, curiosity and an urge to secure a deep and continuing understanding of the biological and chemical sciences must be especially encouraged. A broad knowledge of human disease comes with time and rich clinical experience; but the sense of affection, compassion, and respect for patients, which good physicians possess, must be selected for—it cannot be taken for granted as a necessary accompaniment of high intellect. Here it must be understood that the situation of the sick cannot always be described in scientific terms.

The worth in which we hold the discipline of clinical investigation is critical for its future development within the academic setting. We are well on our way toward a serious polarization of medicine and biology—a process made unseemly by the manner in which differences of viewpoints are ritualized philosophically and by the way dispute substitutes for cooperative dialogue and planning. If we fail to see the importance of the synthesis of science and medicine which can take place in a properly designed program of clinical investigation, we shall lose a great deal of the scholarly ambiance of the university hospital—and we shall lose it precisely in that area of scholarship which directly bridges science to the problems of disease in man.

Finally, we must consider developing new administrative mechanisms within universities to facilitate the free and extensive exchange of ideas across departmental boundaries, such that concerted efforts to resolve some of the intractable and complex problems of human disease—especially of the degenerative disorders of man—can be made. I do not refer here to the directed assault of large groups of scientists on a specific illness, but rather to the clear need for administrative units within universities that will encourage easy communication and collaborative research among individual scientists in different disciplines, including those working in industry, when it suits their common purposes. Such units should encompass educational as well as scientific functions, since they will epitomize for medical students and young physicians the scholarly and creative interaction which can take place between clinical and nonclinical scientists in the appropriate academic setting.

I share the view that the jurisdictional pattern of the traditional university organization presents a considerable difficulty in designing the ideal administrative setting in which this productive interaction can take place. But, in this matter, academic and social needs converge, and the university—that is, we, ourselves—must make suitable adaptations. The problem, while obviously complex, is resolvable; moreover, it is resolvable entirely by members of university faculties. It is an unusual, and exciting, circumstance these days that any single group in society has the full power to solve a major problem affecting itself. We should make haste to solve this particular problem before others take the pleasure of its solution away from us.

III

SCIENCE AND TECHNOLOGY FOR WORLD PROBLEMS: OPPORTUNITIES AND CONSTRAINTS

Introduction

DON K. PRICE

THIS PANEL, unlike the others at this conference, does not have a single principal speaker—all four of our panelists have prepared papers.

As chairman, I have suggested to the panelists that we should try to deal with three broad aspects of the ambitious topic assigned to us. Let me note those three aspects in the order of their increasing difficulty.

First, what research areas in the life sciences or in medical technology offer the greatest promise or threat to the future of human welfare? This question is difficult enough, but it is one into which scientists and physicians and engineers move with great zest and satisfaction.

Second, as we bring about changes in such areas by new scientific advances, how well are our institutions—our educational institutions, our business corporations, our agencies of government—equipped to handle such changes? I suspect that they are not very well equipped to do so, and we must ask how we can improve them, either by changes in the institutions themselves or in our modes of knowledge or technological methods.

The third aspect of our problem is the most elusive and difficult of all: What changes in the basic ideas and beliefs of our civilization have been wrought by the recent changes in the life sciences and in the institutions that must try to cope with them; and what new scientific, philosophical, or religious insights will help the science of tomorrow further human aspirations and the public welfare?

Social Adaptations for Future Instabilities

LORD ASHBY

MY CONTRIBUTION TO THE THEME OF "Opportunities and Constraints" is to invite reflection on one constraint which, I suggest, subsumes most of the others.

Put in its crudest form, science and technology have enabled man to create human ecosystems of great complexity, but they lack the built-in stability characteristic of natural ecosystems.

In natural ecosystems, the evolution of complexity over millennia has been matched by a concomitant evolution of stability. Ecosystems which cannot recover when thrown out of balance do not survive. In a forest or a lake we find an intricate interdependence which is protected and preserved by a network of "homeostatic gyroscopes," which keep the systems in a dynamic equilibrium. The same is true of natural "urban" communities, such as the nests of ants and termites. The citizens of the nest are genetically coded to perform their civic duties. They have no option but to be loyal. Upon this genetic compulsion the stability of the community depends.

By contrast, man-made ecosystems have become astonishingly complex and interdependent without a matching evolution of checks and balances to insure stability. The homeostatic mechanisms to restore a disturbed equilibrum are weak, compared with those in natural ecosystems. The symbiotic relationship between individuals in a community (and between communities) is maintained not by genetic compulsion but by consent. The motives for consent are liable to be weakened in a pluralistic society, due to tensions between two incompatible criteria for

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Lord Ashby was prevented by illness from attending the conference. He submitted these remarks in advance.

natural selection: on the one hand, the interests of the whole social group (extended family, tribe, city, nation) and, on the other hand, the interests of individuals.

As pluralistic societies apply technology to more and more complex organizations, they increase the need for devices to stabilize these organizations—in particular, to get groups (e.g., trade unions, multinational corporations, and nations themselves) to acquiesce in the consequences of interdependence. For example, the symbiosis between nations that own raw materials, such as oil, copper, phosphate, or tungsten, and the nations that need these raw materials is at present very fragile, and is likely to become more so. The homeostatic mechanisms which control markets are too slow and too unpredictable. A similar fragility exists within communities, especially man-made urban ecosystems. Increasing centralization of services has locked citizens into an obligatory symbiosis, yet the citizens are not genetically coded to cooperate. Not only mechanical faults, such as in power supplies or transport, but noncooperation among tiny minorities, such as the people who handle sewage or garbage, can upset the symbiosis and throw a city into confusion.

Throughout most of history, this weakness in man-made ecosystems has been circumvented in one of two ways: a despot imposes on the citizens compulsion which takes the place of genetic coding (the alternatives to conformity to the welfare of the community are jail or death); or social units become decentralized and sufficiently autonomous so a breakdown in one does not affect all the others. In an industrialized and pluralistic democracy, these conditions no longer exist. Free societies will not tolerate despots and affluent nations demand services of such complexity that they have to be centralized.

Research in science and technology is already moving toward fail-safe devices in inanimate mechanisms. A prime need, in my view, is for a corresponding search for fail-safe—or, to be realistic, "fail-minimizing"—devices in human communities. In a world where speed of transport and speed of communication have increased by many orders of magnitude over those prevailing when our democratic institutions were designed, institutions themselves need critical re-examination. To give one example, a charismatic leader can short-circuit the political system which elected him by appealing directly to the masses over radio and television. In order to diagnose and treat these problems, we need to promote research into social biology, social psychology, and social anthropology. The ultimate purpose of that research would be to devise political innovations which fulfil the following conditions.

The innovations must not be utopian, i.e., they must take the human genetic heritage of original sin as given. Even at the rate at which social selection may affect the expression of man's genetic heritage, by applying sanctions against greed and offering rewards for altruism, for instance, the genetic heritage will remain: a propensity for the seven deadly sins. One must distinguish, and not confuse, reflections about the perfectability of man and reflections on how to negotiate with OPEC or how to curb crime in Detroit. Perhaps a useful analogy is the distinction between the destination of a bus and the precise course which it steers through a street. The one is settled in the bus depot. The other—whether to overtake, to stop, to make a detour—cannot be settled at the depot: it is determined by a continuous process of probing and feedback as the bus goes along.

The "probing-and-feedback" process in politics is likely to yield the most useful social results. I doubt whether we know very much about the homeostatic gyroscopes which at present just manage to keep the complex societies of affluent industrial nations in a giddy equilibrium. Conditions of enforced consensus, as under a dictatorship, have been well described; we know less about conditions of voluntary consensus. It occurs during episodes of great danger from an outside source, as in war. It occurs in those comparatively rare circumstances in which the public welfare and the private welfare coincide, as in the use of the pill. It can be reinforced by fear, but the risk-benefit estimates which determine public opinion and, hence, political action, are curiously irrational. Seven thousand people killed on the roads of Britain each year provoke no public reaction; seventy killed in a plane crash shocks the nation.

The aim of the political innovation we seek can be put into one sentence: how to insure that society adapts its behavior in anticipation of some future threat to its stability, and not, as at present, only *in response* to the threat after it has begun to materialize. In working toward this aim, pluralistic democracies have to face ominous questions squarely: Can this be done through public consensus stimulated by education? Or is the only practicable pattern of politics for survival one in which the citizen must surrender more personal freedom to the state?

Material Progress and Global Responsibilities

AURELIO PECCEI

AS A LAYMAN, I am a little puzzled and troubled by our tendency, not here, but generally, to reduce the problems of the world to something which is less representative of the challenge that we have before us. We tend to equate the West—that is, the United States and some countries in Europe—with the world, and to consider the world problems as those which beset us most directly. This is understandable, because it is in keeping with our cultural formation, analytically and nationally oriented, and it is even legitimate. But I wonder if it is not misleading in a society which is ever-more integrated and interdependent at the global level.

I think that we must raise our sights to the real world problems and what science and technology can do about them. We have to make this effort to see things globally—holistically. This is also, I think, the spirit of this institution, The Rockefeller University; the ideas, the tradition, are to look at man as he is now. Today, man is four billion people living on a small planet, and there will be five, six, and more billions in the decades to come. Any meditation about the future shorn from the premise that it will be a future substantially common to so many people, seems to me meaningless. And we cannot isolate our problems from those of the mass.

First, we must understand the mass. Once we have given to our thinking a truly global dimension, probably we will see new sets of questions, of considerations, cropping up. I will mention three clusters of such questions.

The first has to do with the very poor use we are making of the human potential. Of the four billion people in the world, we know that nearly 500 million are permanently hungry. Their capacities are impaired. Half of the adult world population is illiterate. Unemployment can be counted by the tens of millions, and underemployment by hundreds of millions. Robert MacNamara recently said that, among the two billion people served by the World Bank, 800 million—40 percent—are trapped in a condition of life so limited as to prevent the realization of the potential of the genes with which they were born. This is not only a human tragedy; this is not only an explosive threat to civilization, to mankind; it is a gross mismanagement of our most important resource, the human resource. It is also a burden to the rest of mankind. Therefore, when we think about science and technology and what it can do, we have to see whether better human quality may be reached through better health, education, and nutrition, so that a higher potential, a higher capacity of contribution by the majority of mankind, can be obtained.

A second cluster has to do with the key question: whether the adult human being is actually fit to live in the kind of world that we in the most advanced countries, one part of humanity, are busily constructing. Offhand, the first answer should be doubtful. Perhaps it should be in the negative. If we ask ourselves whether the average human being is adapted to live with the new dimensions, speeds, complexities, stresses of our ever-more artificial world, I think that we cannot say yes. Three phenomena, telling by themselves, demonstrate that the present-day human being is not yet adapted to this kind of world. One is underdevelopment. Why underdevelopment? Because many hundreds of millions of human beings are not able to raise their capacities to what is needed. A second is alienation, particularly among the youth, the citizens of tomorrow. In the highest expressions of our civilization, the large cities and the big factories, there is great alienation, rejection, of what we are creating. The third phenomenon is the bomb—a clear indication that we are not able to master our creature, the technology advance. Therefore, a second group of questions should probably deal with our capacity to develop cultural—because it is not a biological question—man to live with the world he is creating.

A third group of questions, which perhaps moves us further ahead, concerns the future. Our society is going to be increasingly technological and complex. Progress cannot be stopped. Are we able, even if we catch up to the runaway problems of today, to follow up in the future? Dr. Edelman has given us an enlivening vision of the possibility that we must know better and, I hope, develop our unique resource, the brain, to solve our problems and to make a better life. Therefore, science and technology, and all our efforts in the future, must be directed chiefly at developing human beings capable of living in the new world that we have created. Lord Ashby has said rightly that human systems have not the quality of natural systems—the homeostatic quality of being able to re-equilibrate themselves when the environment changes. They need the cybernetic, the regulatory, activity of man. But man must be capable of playing that role.

Can man really withstand the new magnitudes, complexities, overcrowding, and the increasing de-naturalization of his man-made world? Can his quality and capacity be groomed to allow him again to be on top of technology? What are the still-unused margins of his brain potential, and how can this be vastly and rapidly developed? Can his nervous system and other capacities, too, be further adapted? How can he be coached in general for the kind of future he is set to father anyway? These questions become vital. According to the answers which can be given to them, and to other, related, questions, mankind's "progress" should be conceived and molded one way or the other, and our present trends checked, corrected, or even stopped.

To summarize—and expand—my concern:

1. Present-day society is forging ahead at great speed while in a state of deep crisis. Nothing could be more dangerous. This is what The Club of Rome termed "the predicament of mankind."
2. The propellant of its mad thrust is modern man's proficiency in developing the material revolutions (industrial, scientific, and technological, the latter also subsuming the others). There are no indications that they will subside in the next decades. There exists, in fact, a great wealth of scientific knowledge yet to be put to practical use by technology.
3. These revolutions have produced immense benefits for man. They have also given him tremendous power, which he has

used to modify drastically his world environment and his condition in it. But he has so far been unable really to control and guide them, becoming in a way estranged from his creature.

4. There is no sign, either, that man is going to master his revolutions in the near future. Their anarchical course is therefore bound to remain so. This constitutes the element of extreme danger in the human predicament. The sense of insecurity and instability creeping everywhere in the world is but a reflection of this state of affairs.
5. At the same time, the poor of the world do not see their lot improving, even in the midst of the unparalleled opportunities offered by techno-scientific-industrial progress. Their revolt is building up into another world revolution: that for a new international (economic) order. While this new revolution will probably overflow from among states to within states, its manifestations are themselves disorderly, unregulated.
6. At this stage, further material progress, purged, it is hoped, from unwanted effects, and a new global economic order, leading, it is also hoped, to both social and political change, are indispensable and inevitable. But they patently are not sufficient for society to redress the situation and become sane and safe again.
7. Henceforth, much more than in the past, the key factor is man himself. All other advances will be beneficial, provided that the quality and capacity of the individual are developed at the same time. If, on the contrary, man the protagonist continues to be divorced from the reality he goes on creating, his predicament will worsen and probably his very fate will be sealed.
8. Society's internal disequilibria, its disharmony with the ecosystems, and most of its other ills are due to the cultural imbalances which have occurred in the human individual throughout the world during our generation. He has acquired his undreamed-of power too suddenly to be able to learn how to use it or to adjust to its consequences. He has thus become a mighty, modernized barbarian. No worthy human goals, perhaps not even survival, can be achieved if his cultural equilibrium is not re-established first—and soon.
9. Therefore, the real question in the years and decades ahead

> are not those with which mankind seems most preoccupied nowadays. They are not whether technological and institutional development can be achieved, but whether the indispensable human development is feasible, and how, and by what means—before it is too late.

I hope, then, as we look forward to the next twenty-five years, or to the next seventy-five years—for those of you who will live so long—that we will think of what this university, this center of excellence, can do. I think that it should look very deeply into the quality and capacity of the human being as a new expression of a multibillion humanity. The issues I have outlined concern the future, and are also within the scope of the medical sciences in a very broad sense, although many other disciplines should be asked to participate in the research and reflections they call for. This, too, is within the province of The Rockefeller University.

Systems Science and Engineering for a Hungry World

WILLIAM O. BAKER

A RECURRING THEME of my associates in our preparation for this discussion is the complexity of applying science and technology for human good and the satisfaction of human needs. A lifetime of many of us has been devoted to that objective, and causes me to agree with them in depth. Nevertheless, we are moved also by how far our efforts to deal with this complexity have carried us. They have carried us forward most especially in engineering and physical sciences, for instance in the field of communications and transport, as Dr. Davis has mentioned, although advances in the conversion of energy, the techniques of warfare, the exploration of space, and numerous other examples also could be given.

To see such effects in the biomedical sciences, one need only look at the prolongation of life, life expectancy at birth, at once a cause of worldwide instabilities such as Lord Ashby has warned us of, and at the same time an easing of the life conditions of scores of millions of human beings. In the last twenty-five years alone in the yet-developing countries, average life has been extended from forty-two to fifty-two years, and infant mortality has dropped from 180 per thousand births to around 120.

But in spite of these dramatic advances in easing the human condition, this part in our discussion is based on the postulate that systems engineering, that is, engineering for the purposeful stabilization of ecosystems, such as those Lord Ashby has described as in natural balance, has been little applied in bioengineering. Specifically, vast opportunities exist in the technology of food and nutrition, which must, in turn, be achieved from a more systematic scientific knowledge of food systems than is now available.

We are barely beginning to adjust, in gathering appropriate information, to even the atmospherics and hydraulics of crop culture. That beginning must be supplemented by sensing temperature, light, moisture, and nutrient conditions, coupled with work in plant genetics. Thereby, our basic yields will continue the gains of the last couple of decades. Indeed, the yield per acre of grains could rise by one-half if, by some of the newer microbial or chemical schemes, we could bring their nitrogen capture up to a quarter of that regularly accomplished by legumes. Over-all, in developing areas, yield would have to grow by 3.5 percent per year to meet food demands already felt in the past decade. Although growth did hit 1.9 percent per year recently, climatic changes threaten that gain. If, indeed, as is asserted, less than 218 million metric tons of grain, out of the world production of 1.2 billion, are produced in the United States, there is still ample worldwide opportunity for improved adaptation to growing conditions. But the world will need 100 million tons more of cereals to withstand hunger. Indeed, the United Nations Food and Agriculture Organization projects a world deficit of 8.5 million tons by 1985.

But these familiar notions of improving yields and spreading knowledge of cultivation are not the main thrusts of my reminder to this sophisticated audience. Rather, I am suggesting particularly that there are vast opportunities in science and engineering, in the immediate and long-range future, first to produce the right, unharmful foods, and second not to waste appalling fractions of those we do produce. Economic analysis, as well as biomedical evaluation, suggest that, if these paths could be developed, profound advances in the total world ecology and economy could result.

Concerning the right foods, evidence ranging from the epidemiology of cancer to the extraordinary studies, in Mexico and elsewhere, of mental retardation from malnutrition calls for a systematics of food-selection principles—a grasp of socio-bio-habituation—which perhaps has not been seriously examined since Biblical times. The situation also urgently demands a similar psychological, sociological, and biological examination of food waste during harvest, transport, distribution, preparation, and eventual consumption.

In the physical sciences, we can describe the propagation of a pulse of radiant energy through nearly every medium and, since Newton's time, the movement of an object being propelled. It is not too much to seek some engineering parameters of the bioenergetic transport of edible matter, beyond the estimate that the world produces about 2,600 calories per capita (3,200 in developed countries). Yet distribution of these foodstuffs is so erratic that at least 460 million people are believed to be malnourished. This is despite the need for only about 2,300 calories per person, with actual production in even the poor countries often averaging about this level!

Studies by the U.S. Army Natick Laboratory, monitored in the past quarter-century by panels of specialists from the National Research Council of the National Academy of Sciences, have indicated the still-vague outlines of how to create and measure food systems. Unlike the ardent "archaeologists" of Tucson, Arizona, who are now systematically measuring the waste, especially food waste, by the people of that city—as much as one-half of the food is wasted—military systems with which Natick has been concerned are closed enough to permit measurement of the disposition of a variety of basic foods. Their ability to support life on the one hand, and their acceptability and preference qualities on the other, have been at least crudely characterized.

Natick Laboratory is one of the few in the world that maintains operations research in a systems-analysis office for food activity. Thus, within the past year or so, studies have been completed, such as that of P. Brandler and co-workers on "The Basic Level of Feeding: A Comparison of Military and Comparable Civilian Food Utilization." Similar studies by Brandler have involved "The Development of Alternative Food Cost Indexes" and "Patterns of Food Utilization in the DOD," Volume I. The work of Bustead, Byrne, and Davis involves some of the closed systems mentioned, including a recently completed study, "An Evaluation of Food Service Systems at Fort Myer, Bolling Air Force Base, and Fort Benjamin Harrison." An even more extensive investigation has involved works of M. M. Davis: "A Nutritional Evaluation of the Experimental Food Service System at Travis Air Force Base, California" and "A Work Analysis of Food Service Personnel at Travis Air Force Base, California." A whole series of similar studies at that locale has provided a microcosmic view of the effective technical parameters and their scientific basis in the amiable and efficient feeding of important (and often hungry!) segments of a population.

We may prefer to forget that in our civilian population we have equally large, effectively contained, similar segments. For example, in 1975, 19 million individuals used food stamps issued under auspices of a single federal department, HEW. Thus, about 9 percent of our population received food as a public bounty, with little technical or sociological knowledge of whether it was well or poorly provided. There is, indeed, strong evidence of extensive waste through purchase of costly and ineffective "nutrients," and other examples contrary to the public interest.

We must seek a new coalition of the physical and engineering sciences with the biosciences in constructing a socioeconomic food technology. In the case of food preservation, experiments at the Natick Laboratory on sterilization by radiation have for a decade demonstrated excellent and economical results for a variety of meats, fruits, and vegetables that are sensitive to various biochemical changes. Dehydration, time-temperature interchanges in preparation, above all, packaging to control diffusion and oxygen exchange and reduction of nitrite preservatives in meats are proper examples of opportunities for major scientific applications to establish this new food and nutrition economy.

Studies at the same laboratory reach over into important animal assays, whose correlation with human reactions are ever more needed. Thus, use of dehydrated food for rats seems to cause an abnormal serum lipid-protein pattern, and a calorie-dense diet seems to elevate total liver lipids and cholesterol. However, moderate exercise, systematically applied, lowers these values to normal in the case of the calorie-dense diet. We cite but a few of these factors, which, of course, are thoughtfully studied in so many university laboratories, to emphasize that there is no national or international system for the regular and effective exchange of new findings with industry and government agencies. Thus, even the most appealing and seemingly efficient food preparation and marketing are attended by a host of bioscientific uncertainties, which should at least be recognized in more than diet and cholesterol-scare fads.

What we must now pursue is a broad characterization of our national and international food usage, so that we can, at least in the next decades, appeal to humanity's essential rationality with knowledge, with engineering data. These must display, on the one hand, the true costs in energy, matter, and environment of the proteins, fats, carbohydrates, and vitamins which we and other societies consume. On the other hand, we must reveal, as much as possible, the physiological and psychological effects of these substances, particularly in relation to the epidemiology of cancer, of obesity, and of other disease vectors. Equally, we should reveal emphatically whatever affirmatives there are in the old cliché, "You are what you eat."

Doubtless, we shall not resolve the great cholesterol controversy, the great vitamin-C polemic, or the lipid liability, but this is not the point at all, except inversely. These issues have indeed absorbed far too much of the scientific and technical effort that should be organized to form a basis for efficient food production and usage. We should build a technology base that compares with that underlying supplies of energy, clothing, even shelter, transport, communication, and national security of the modern era.

The supermarket is indeed a great invention, but its socioeconomic effects on diet and food utilization are virtually unknown. Maybe elementary and high-school training in the use of the supermarket, including arithmetic and such other factors as language and labeling, is as important as training in history and composition. For instance, what effect does a market's colorful display have on the fluctuations in per capita food consumption, which in 1975 went down to the lowest level in seven years? And what caused the highest food consumption in 1972, 103.8 percent of the 1967 average? Meat consumption fell 4 percent in 1975, and now, according to the Agriculture Department Economic Research Service, is expected to rise considerably. Is this good or bad for the general public health and well-being? Per capita consumption of potatoes is expected to decline in 1976. Sugar consumption fell 8 percent last year, presumably because of prices, which have caused a similar decline in coffee consumption since 1962. What kinds of physiological and behavioral effects do these fluctuations represent in a free-market society? In the spring of 1976, the price of cattle, eggs, apples, and lemons declined, whereas that of corn, soybeans, and potatoes rose. Is the average marketing scale an adequate guide for the appropriate biology of food and nutrition?

Indeed, will supposedly free-market economics, coupled with superficial, perhaps child-dominated, preferences, spread around the world in a new economy of nutrition? Nine developing countries, themselves producing food at a higher rate of increase than the United States, nevertheless imported our farm products, moving from $56 million in 1955 to $2.5 billion in 1973. Because we do not understand the bio- and behavioral engineering of food consumption, it is hard to know what such major world shifts mean, such as rough, unrefined, native crops and other natural components of the diet of developing countries are replaced by the bland and specialized cereals, soyas, and other major commodities of export and mass agriculture.

Proper encouragement of food production in the developing countries needs this kind of technical insight. The International Bank for Reconstruction and Development increased funding for agriculture from $484 million between 1961 and 1965 to $3.6 billion between 1971 and 1974. But only a few traces of systems evaluation of these programs, such as the International Corn Research Network, are so far evident. Because corn is the predominant food for calories and protein in South America and Africa and is produced as a major food in 126 countries, exceeded in worldwide importance only by rice, it is helpful that at least some genetic and biochemical attention is being given to corn culture. However, only in 1974 were systematic tests of new varieties undertaken. Our point is that, under present circumstances, the physiological impact may take many years or even generations to ascertain. We know that about twice as much corn or sorghum protein as animal protein seems to be required for equivalent values, but we are really not confident of those values. Two new corn genes developed at Purdue in 1963 contain the two needed additional amino acids, and recent corn derived therefrom has a protein value for human nutrition estimated to be about as high as that of dairy products. But this pushes us quickly to ask what the right level is, and how it should be balanced with other foods and with human preferences.

The challenge to bioscience and engineering posed by the food and nutrition needs of the world has another remarkable attraction. It is that human pathology can often be directly related to eating. We know of a host of classic examples, from straight starvation through blindness from vitamin-A deficiency; anemia due to a lack of iron, riboflavin, and folic acids; goiter from iodine deficiencies; and many others. But, in addition, there is emerging the huge domain of allergy, overstressed in some centers, but nevertheless important in assessing human feeding. Likewise, and of special importance in the developing areas, is the whole realm of toxins from foods that appear innocent, but are spoiled or inhabited by unusual pathogens.

Then there is the contentious issue of additives, which on the one hand greatly extend food usage and availability throughout the world, but are considerable factors in the assertion that carcinogens are especially active in food. Food additives may, of course, have more subtle, possibly allergenic influences. A remarkable possibility of this appears in a recent study by Connors at the University of Pittsburgh, following the early observation of Dr. Benjamin Feingold in California. In a controlled study, Connors concluded that hyperactive children improved significantly, in the judgment of their teachers, when given a diet free of artificial flavors and colors. Although there are an estimated five million such cases of hyperactivity in the country, we do not know what the consequences of the social reaction to a hyperactive childhood may be for adult behavior. The matter is compelling. Of course, the experimental diet shifted the total food pattern considerably, as soda pop, frankfurters, and cake mixes, as well as certain breakfast foods and aspirin were proscribed. While the study is sensitive and thin in data, it does illustrate our theme that, insofar as biomedical sciences and research are intended to improve human well-being, this arena of food and nutrition, with its combination of normal and unhealthy effects, requires a new, systematic pursuit.

Further in this regard, understanding food effects in human societies couples closely with many other biomedical variables. Nutritional needs are affected by a person's developmental history, occupation, current medical state, and therapeutic activity, as well as by many social factors, fashions, and doctrines. It has long been known that infections, such as certain gastrointestinal diseases, markedly shift metabolic activity and thus influence nutritional needs in subtle ways.

Finally, the biosciences provide new pathways to food production, with improved efficiencies that could shift the political stability and, indeed, the economic strength of many nations. As I pointed out earlier, the 40-million tons of nitrogen fixed annually by microorganisms attached to the roots of plants, particularly legumes, are about equal to the present world consumption of fertilizer. However, recent discoveries make possible contrived association of nitrogen-fixing microorganisms with the roots of such tropical grasses as sugar cane, corn, and rice. In addition, these findings imply a better use of fixation by microorganisms that are not legume-associated. If microorganism association of grasses and grains could be raised to about half the 84 kilograms of nitrogen per hectare (75 pounds per acre) achieved by legumes in the United States, the present world grain production would be aided by the equivalent of 30 million tons of nitrogen. This, in turn, is equal to about 112 million tons of high-nitrogen fertilizer, or three times the current world use. This could produce 250 to 300 million tons of grain on acreage already available in the developing countries, although if such fertilizer production were achieved by chemical factories, the capital cost would be about $23 billion.

The whole area of photosynthesis also is capable of scientific and technical advance, involving such matters as carbon-dioxide enrichment, genetic changes, and understanding of the photocatalytic energy-transfer process. Similarly, the control of microorganisms, fungi, and various pests would vastly improve the preservation and storage of vital food crops. It is believed that, in tropical African countries, storage of grains and legumes for twelve months causes an average spoilage of 50 percent or more. These principles of systemic, organic technology can also be applied to livestock, which constitute another major food resource and which consume large quantities of crops in the course of their own function. In the United States, in fact, animals provide two-thirds of the protein consumed, about one-third of the energy, and about 80 percent of the calcium. However, we do not yet apply to their culture the very aspects of nutrition systems engineering that I have urged in earlier comments about human feeding. Serums for livestock disease control, especially of parasitic and viral diseases, are of high urgency, and improved methods of breeding to obtain a greater number of offspring should also be emphasized.

Altogether, the new institutions and strategies for research and development in the biosciences, which have been proposed or remarked upon in this meeting, would seem to find a place in the advance of the world's food and nutrition. The feeding of animals, especially human beings, encompasses all aspects of normal growth and life, including interactions with individual abnormalities. Thus, it seems possible that this realm of activity, which has traditionally involved a rather specialized and compartmentalized acquisition of knowledge, should be guided toward full systems science and engineering, as has been achieved in the simpler case of the physical sciences. It is possible, considering the way the world of people is made nowadays, that major advances in peace, individual comfort, and cultural growth could come from relief of the pressure and threat of hunger. At the same time, many pathologies, perhaps including even cancers and neurogenic diseases, also could be explained and better controlled by application of systems science and engineering.

Several studies of the National Academy of Science/National Research Council have been especially valuable in preparing these comments.

Science and the Developing Countries

MAURICE F. STRONG

I WOULD LIKE to bring you the particular point of view of the developing world, which, I think, is a necessary point of view. In the absence of better-qualified developing-country spokesmen, I will take on that role, having spent a good deal of my life in and with developing countries. As we are discussing global issues under the aegis of an international conference, the point I will make is probably best symbolized by looking around the audience and asking how many developing-country voices are being heard; how many developing-country people are even represented here.

Because I am a layman, I might as well turn this into a virtue and be very unscientific, and present to you in rather telegraphic style a number of points which I will not attempt to substantiate. However, they are clearly open to debate.

The first is "a clearly evident gap" between the developing and the industrialized world that is primarily a gap in science and technology. The statistics of this are known to most of you, except for perhaps one reminder that some 98 percent of research and development expenditures today take place in the developed, or industrialized, world, and some 2 percent in the developing world, a fair percentage of which is carried on by industrialized-country corporations. If it is true that today's research and development produce tomorrow's wealth and productivity, it is obvious that the gap, in purely material terms, is destined to widen and not to narrow. Some individual corporations, as you know, spend more on research and development than is spent by the entire developing world, with the exception of China and perhaps Cuba, for which statistics are not available.

This gap is a threat, a very great threat, to both developing and

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Mr. Strong substituted for Lord Ashby at the conference.

industrialized countries and, most of all, is a very real constraint on our capacity to create an operable, global system of governance. Bridging this gap is essential to the effective functioning of our technological civilization. We talk about global priorities for science, which I won't attempt to list in specific terms; rather, I would like to define them in two broad categories. I believe it is within these categories that our priorities, on a global basis, clearly must be placed.

The first category is what might be called the "outer limits" to man's activities—helping to define and to foresee those risks in which man's activities may be impinging on outer limits, which, in turn, can affect his own survival and well-being. These include environmental risks, such as risks to the ozone; the possibility of nuclear destruction, either through the use of nuclear energy as a source of energy or through the nuclear bomb, mentioned by Aurelio Peccei. It is clear that the identification of such risks, the foreseeing of them, and the kind of collective action required to avoid them, require global action involving the cooperation of that two-thirds of the people of the world who live in the developing world. It is also clear that such other activities as the exploitation of the seabed and the use and control of nuclear energy require a high degree of cooperation by the countries of the developing world.

Of course, the developing countries must have the capacity to participate in these programs if they're going to do so effectively. In the United Nations Environment Program, with the mounting of the Earth-Watch program, the Global Environmental Monitoring System, and the International Referral System, we had to wrestle with this problem. Many representatives of industrialized countries felt we should pursue the more efficient route of setting up a relatively small group of scientists from industrialized countries who were capable of defining all the problems, even of creating a global system. However, we elected to take the longer, slower, more politically difficult route, helping to bring the developing countries into it, identifying people in the developing countries who had the scientific capacity to understand and contribute to it. This is not always easy; it is time-consuming and politically difficult. But, having done it, a commitment has been made to a system that will not work solely on the basis of scientific acceptance, which requires a certain degree of political acceptance. This is one of two central points I want to make: we require the cooperation of the developing countries, and must help them develop the capacity to cooperate.

The other side is that their own development requires much greater scientific and technological capacity in such areas as food production and industrial development. Here we have a most interesting example of the political and social consequences of scientific progress, particularly in the health-care field. We all know that the population explosion, with which the developing countries, first and foremost, must cope, is a direct consequence of the application of science to the field of health care and the corresponding dramatic decrease in death rates, which is now facing the developing countries with an almost Herculean task.

I believe that the two categories in which the priorities for science have to reside on a global scale are, first, avoidance of the kind of global catastrophe which could affect the future of the whole human population, or large segments of it, and second, meeting the very basic human needs of the two-thirds of the population that live in the developing world. These are the two parameters: avoiding ultimate catastrophe and meeting elementary human needs.

Here are a few ideas on how the developing world might be helped to get this capability. The central point is that we need a lateral extension of science as much as we need an in-depth penetration of science into many new problem areas. It is probably true that science and technology today could solve most problems without the participation of the developing-country scientists. The developing countries, however, do not need just the results of science, the application of science; they need science. They need help to develop the scientific way of looking at things; help to develop the institutions; help to develop the respect of science and the respect of scientists in their own community. Education and training, yes, and this has received the most emphasis today, but most developing-country scientists, as you well know, don't work in developing countries. They work in industrialized countries. We need support for institutional development, including regional and special-purpose institutions which help the developing countries to overcome this problem of the political acceptability of science in their own, sometimes rather primitive, political environments. We need support for continuing professional development specifically tied to their home-country institutions, rather than to foreign ones. We need international recognition of developing-country scientists, their work, and their institutions. We need more joint funding of research and development programs, in which developing-country institutions are given a significant piece of the research and development programs concerned.

The dichotomy in developing countries between scientists and political leaders is usually more than simply between the two disciplines mentioned by Dr. Edelman. In the developing countries, it really is a dichotomy between two cultures, and we have to understand that and help them to bridge it. Let me simply mention that we have found the ability to talk across ideologies with the Communist world, largely—not entirely, but largely—through the ability to communicate on the level of science and technology. The ability to communicate with the developing world through scientific and technological processes is absolutely essential, and this is really what we must aim our efforts at. If we cannot construct a dialogue through the language of science and technology with the peoples of the developing world, we cannot expect them to have confidence in the scientific and technological means of finding solutions to the problems we share with them in terms of facing the outer-limits type of risk I have described, and the problems they face in providing the basic essentials of life for their people.

When Science and Technology Stumble, Everyone Suffers

RUTH M. DAVIS

Underlying Assumptions

Humpty Dumpty sat on a wall
Humpty Dumpty took a great fall
All the King's horses and all the King's men
Couldn't put Humpty Dumpty together again

Nursery Rhyme, author unknown

SCIENTISTS AND TECHNOLOGISTS, as well as the fans and critics of science today, must feel a bit like those who surveyed and assessed the fate of Humpty Dumpty in his nursery-rhyme tumble. For indeed the phrase "science has fallen off its lofty perch" has become so common as to be trite, although, fortunately, it does not yet have the universal appeal or acceptance of a nursery rhyme. Also, in keeping with Humpty Dumpty's situation, there is today a valiant, but as yet far from successful, attempt to reduce the disarray of science and technology and "put it back together again" into some sort of coherent whole.

One of the great puzzles of our time is what science and technology should look like, i.e., what do we want of science and technology, so it is impossible to decide how close any of its rescuers have come to reducing the disarray and putting science and technology back together again.

Because of the indeterminancy inherent in any overview of science and technology, certain assumptions have been made which are now overtly stated in order to place some bounds of credibility on what follows. These assumptions are:

ASSUMPTION 1

Science and technology are sufficiently distinct so they may be viewed separately in terms of their goals, the problems besetting them, and their internal characteristics. At the same time, science and technology are so interdependent today that any separate treatment of them in terms of their impact on society and society's means of dealing with them would be artificial and meaningless.

ASSUMPTION 2

Science and technology (henceforth referred to collectively for convenience as science-nology) have taken a great fall in several senses of the phrase. A few noteworthy instances include the following:

1. The public's belief that science is equivalent to certainty has been publicly destroyed. Even though this belief was fallacious, its dramatic overthrow was traumatic to society's need for stability in some aspect of its existence.
2. The public's belief that scientists spoke with one voice on a given "scientific" topic has been dispelled. The reliance of the public on "one-handed science" was obviously honest and real. Senator Muskie's stated annoyance with scientists employing the "on-the-one hand . . . but then . . . on-the-other hand" tactics of ordinary folk turned out to be not only his, but a universal, annoyance: more importantly, the death of this myth caused a universal let-down.
3. Science-nologists have themselves been confounded and confused by what needs to be "justified," "proved," "refuted," "confirmed," "tested," "guaranteed," etc., in order to engage without guilt in science and technology. Here the differences between science and technology are of profound import, as shall be discussed later.
4. Recent products of science-nology have apparently resulted in grave and unforeseen hazards, such as: a) exposure of workers to risk of disease and premature death from vinyl-chloride in factory environments; b) changes in the composition of the ozone layer from supersonic transports and aerosols alike; c) transplantation of foreign genes into bacteria, causing new in-

fectious diseases; and d) accident and injury in the Bay Area Rapid Transit (BART) system from defects in the automated control system, and

5. The seemingly callous and headlong pursuit of funding by science-nologists, regardless of the special interests they would then have to serve as their traditional sources of resources for science-nology diminished in number and quantity.

ASSUMPTION 3

The reliance of society on "good" science and "good" technology as a source of stability and progress is so great as to demand unlimited heroic efforts to get science and technology back together again in a position of influence and strength.

ASSUMPTION 4

Whether or not science and technology have actually fallen on bad times and are, in reality, in difficulty, the public views science and technology as a cause of many of today's problems, and believes that science is meandering in a rather aimless course. As is so often the case, the public perception of a situation may be more important to the outcome than are the realities of the situation.

These assumptions underlie the concerns and the conjectures made in subsequent sections.

Some Historical "Ups and Downs" of Science and Technology

SCIENCE

Science owes some of its ups and downs to its characteristic of not being particularly diplomatic in selecting those groups or entities to which it has permitted itself to be an adversary or ally: for example, religion and war.

Man, as a being of sense, wants his life to make sense. We would all like to believe there is a reason for what we do and that we can exercise some control over what we do. In many instances, man has found this hard to believe unless there is more than what he sees—unless there is an external order and an external life beyond the daily uncertainties of life and death. Religion has, most often throughout history, assumed the responsibility of providing these beliefs.

But so, also, has science. Scientists moved by honest zeal and a reverence for facts have produced "miracles" in the world of everyday life. The price of these miracles of science have come dear, especially in earlier centuries. It pitted scientist against clergy and confused the ordinary person. Scientists were expelled from the church for their findings. Even worse, scientists had to juggle their desires to produce good science, their needs for money, and the need in talking to their benefactors to create as little confusion as possible among science, man, and God. Some of the prefaces and dedications of scientific books in the eighteenth and nineteenth centuries are masterpieces of double talk, signifying this multiple identity of scientists.

One of my treasures, a book by Priestley on *Optics,* written in 1772, is dedicated to the Duke of Northumberland. In this dedication, Priestley valiantly says:

> No branch of science stands in so much need of the aid of the Great as that which is the subject of this work. But on the other hand from no branch of science do they derive greater advantage. For without that knowledge about what is conversant, rank and fortune would be of little value. . . . By this species of knowledge it is that mankind in general are capable of improving their situation in the world, making the most of every advantage it affords; and obviating the inconveniences to which it is liable. These studies also having the words of God for this object are of eminent use to extend the views, and enlarge the comprehension of the human mind. . . . That your Grace may long enjoy the exalted satisfaction of promoting a science so truly worthy of your taste is the sincere prayer of, . . . Joseph Priestley.

As theology and science gradually reached a stable impasse or an acceptable detente, science showed its impractical nature by admitting to war and crises as allies. This was in spite of the fact that, substantively, science is oblivious of international boundaries and transcends in its findings the various artificial clusterings in which people group themselves, e.g., labor unions, midwesterners, diplomats, the press corps, etc.

The National Academy of Sciences was established during the Civil War, and the National Research Council during World War

I. World War I was a war of chemistry. Radar, computers, and nuclear physics were products of World War II. A survey covering the 1939–1940 academic year in thirteen leading universities revealed that the highest total of departmental expenditures in physics for "direct operating expenses of research" was $39,000; in chemistry it was $73,000. By contrast, at the end of World War II, OSRD could report that "it had awarded contracts totaling nearly $117 million to MIT, $83 million to Caltech, $31 million to Harvard and $28 million to Columbia."

The crisis of Sputnik in 1957 caused the Office of Science and Technology, the President's Science Advisory Council (PSAC), and the Office of Science Advisor to the President to be established in the executive office of the president. Sputnik also gave birth to the National Space Program, with its Apollo and Skylab projects and its explorations of outer space.

Science appears to spurt with war and crises. At other times, it suffers more rebuffs than honors. History certainly makes it difficult not to associate science with war and new methods of destruction: scientists are associated with the advice they give to national and military leaders and to their associations with the Big Science of wars and crises. Only medical science appears to have escaped this gross generalization. At the same time, science and scientists make no real attempt to disavow war and crises as allies: government is the biggest spender on science and government spends "biggest" during wars and crises, so the allegiance is understandable. But it certainly makes it tough on scientists who also have an interest in the public good, the public welfare, and the individual citizen to manipulate his way semantically through some of the recent perils of science.

TECHNOLOGY

Technology also has suffered its ups and downs. In his book *Technology in the Ancient World* (1970), Henry Hodges concludes that "nowhere was the rate of technological advance a steady, even upward, climb." There always seemed to be short bursts of technological innovation interspersed with long periods of virtual stagnation. He cites the societies of modern New Guinea and the Amazon as classic examples of apparent permanent stagnation that followed initial early bursts of technological invention.

One of the limitations of technological evolution in antiquity seems to have been imposed by man himself: certain social conditions turned out to be inimical to further technological innovation. Many agree that authoritarian governments aimed at stable social conditions appear to have been those under which there was least technological advance. The reasons varied, but included too-rigid controls by government, too much capital investment in certain technologies to the detriment of others, and the denial of communications in order to maintain the status quo.

The periods of dramatic technological growth in ancient days were seen when a stable society suffered a setback at the hands of one of its less technologically advanced neighbors. Then, if enough remained of the older society's intellectual capability, the intruders or newcomers provided the incentive for new technologies. The intruders were less scientifically trained, they were willing to learn, and they were, obviously, more politically adaptable and ready to accept change. For many centuries, this pattern of technological activity dominated.

SCIENCE, TECHNOLOGY, PATRONAGE, AND CHANGE

In the Middle Ages, science and scientists found a seemingly penoptimal way of survival—patronage. If a scientist could persuade a wealthy patron that what he was doing was exciting, that he was an interesting fellow, and that these two attributes would enhance the prestige of the patron, he could confidently rely on a long-term source of funds.

There was a very useful separation of power in the Middle Ages. In reality, it was more a Separation of Purpose. The utility of a particular scientific objective was not tied to the utility of science in its entirety. One did not have to sell Science to the "System" to sell a small science project to a patron. The purposes of Big Science were clearly separated from the purposes of Little Science. The charm of scientific discovery had not yet been sullied by having to determine its impact.

Of course, not only was the charm of scientific discovery untainted; so also was its translation into practice. Anything can be charming if it does not affect you. Science was as rural as the countryside in those times. Change came very slowly; changes traveled very slowly from village to village and from country to country. Most importantly, there was little recognition that science was a most effective agent for change. Since change is a process to which people always have difficulty adapting, science was spared the unpopularity it often enjoys today. The Middle Ages was the time of isolated scientific achievement with little spread of scientific application.

History provides proof that science allowed itself to be typed as an adversary of religion, even though this may have been unwitting. History also shows war and crises to be great allies of the advance of science. Technology has been shown to flourish when mature, stable societies get "knocked off" by less advanced societies. Wealthy patrons of the Middle Ages, generally disliked by the populace, were the greatest monetary supporters of science. And in this country, government, viewed with mixed feelings by its citizens, is the greatest supporter of science and technology. One could suggest that science and technology have never excelled at being loved or at picking their friends.

Selected Observations on Science and Technology Today

COMMUNICATIONS AND SCIENCE

Today, in contrast with the past, we no longer have the luxury of long lead times between discovery and impact or between event and impact or between cause and effect.

When the telephone was introduced, its use spread slowly from the more wealthy to the less affluent, from the city to the country, and from the United States to other countries. The use of the automobile spread more rapidly. Television spread almost instantaneously, with only the slower growth of electric power holding back the use of television in certain parts of the world. Unfortunately, now that the telephone, automobile, and television have been invented, we all use them.

The existence of the technological triumphs of worldwide communications and transportation makes it possible for all of us to share vicariously the scientific changes impacting on just a few of us. The landing on the moon made everyone equally aware of the technology involved. The first heart transplant became an overnight hope for everyone. The worries of the Aleuts over their underground nuclear blast were simultaneously worries of us all. Communications and transportation have made scientifically induced triumphs and misery anywhere the triumph and misery of everywhere. As a consequence of this universality of sharing all the results of all sciences, one has a perhaps false impression of rapid rates of change in science. What may be happening instead is that a somewhat greater rate of scientific advance has been combined with a much greater communication of information about changes due to science. These two phenomena, when joined, provide the same effect on each of us individually as would a much greater rate of advance of any single science. Perhaps, then, the discontinuity that is attributed to science is actually a triumph of communications.

Indeed, in an "AFIPS-Time" survey of 1970, the responses to the question "All in all, what effect do you think inventions and technology have had on life in the past 25 years—have they made life better, worse or haven't they affected us one way or the other?" were heartening. Eighty-five percent of those asked said that life is better, with 56 percent stating that it is much better.

This finding would indicate that people still admire science, respect it and its scientists and, when really annoyed with it, probably rationalize by saying, "Oh, well, it just had the wrong goals" or "Government was just using science as a pawn—it wasn't the fault of science."

CHANGE AND TECHNOLOGY

In his book *Future Shock,* Alvin Toffler stated that "change is the process by which the future invades our life." Most people who become known as reformers or leaders do this by changing a government, a science, an institution, or a trend. Our admiration of them is due to their success in effecting this change and in so causing a discontinuity in our society. One aspect of this phenomenon which we often overlook, however, is that there are two components to any such change. The first is a change in direction; the other is the rate of change. History generally treats only the change in direction, e.g., going from a dictatorship to a democracy, changing physics through the theory of relativity, or going from an increasing to a decreasing infant mortality rate.

Generally, it is the rate of change that escapes our attention. Yet, it is when we are subjected to too much change in too short a time that we look at change as a discontinuity and a cause of anxiety, rather than of progress.

As a result, there is today increasing interest in exerting conscious and structured efforts to effect change in management, in ways of providing services, in methods of production and manufacture, and in means of communication. People are less and less willing to accept change over which they have exercised no control. They want and believe in their rights of choice over changes which will affect their ways of life.

One of the greatest strains that has resulted from our desire to exercise some control over changes affecting us in our individual lives is that between technology and the public. Technology is seen as an agent of rapid change—a sophisticated agent not too well understood by the ordinary citizen, and therefore an agent to be feared and slowed whenever possible. And, indeed, technology has been perhaps the most effective agent for change in management, manufacturing, services, and communications. It is probably even less understood than are the institutions we have established to handle our many special interests in society and in our economy.

The concern of today is that people may become the victims, rather than the masters, of technology. To escape such a fate, we must decide what we wish for ourselves and apply technology to achieve our goals. But few people excel at changing fantasy into fact. First of all, we generally lack the patience to proceed through all the steps that make fantasy a fact. Second, it usually takes more than our individual resources to get what we want. That means that a lot of us have to agree on the same goals and then apply our collective resources to reach them. Third, different groups of people have conflicting goals and dreams for the future. It should be no surprise, then, that it is sometimes easier to let technologists have their way than to argue them out of their proposals.

The best of all practical worlds occurs when technologists allow themselves to be socially acceptable schizophrenics. First, they think as individual citizens, hating to drive to work or hating the boredom of a production-line job. Then, they assert their technological prowess and think of ways to change their daily nightmare into a real world paradise.

INSTITUTIONAL BARRIERS TO PRACTICING GOOD TECHNOLOGY

Concerns with institutional barriers to practicing good technology are generally expressed in such questions as:

> Why does it take so long for a good idea to spread?
>
> Why was the aerospace industry so successful with instrumentation when the medical community wasn't?
>
> If the police in Ohio use the new mobile radio communications with such good results, why doesn't the governor of Pennsylvania require Pennsylvania police to use them also?
>
> Why do we have to pay for the development of another computerized payroll system? Why don't we use one of the ten we have already paid for in the federal government?
>
> Why does postal service keep getting worse at the same time as we have to pay more for stamps?
>
> Multiphasic health screening is really succesful at Kaiser Permanente in California. Why doesn't it spread to other places?
>
> We have already paid for that equipment-diagnostic technology in the Apollo lunar program at NASA and there are lots of offices for technology transfer. Why doesn't the TV industry use it?

Answers to these questions, and even any intelligent discussion of them, presupposes a rather intimate knowledge of: the technological process, processes of change themselves, the institutions which operate in the various sectors of our economy, the failures occurring in attempts to effect change through technology, and the institutional practices which operate to prevent the spread, i.e., diffusion, of successful technology-induced change.

The spread of successful applications of technology, for example, may occur within the same special-interest community as did the innovation, or it may transit between communities. The problems or barriers associated with the spread of applications appear dependent on which of the above two situations holds. This dependence can be explained logically as resulting from the differences in institutions found in different special-interest communities.

For example, if the objective is to diffuse the successful application of emergency police communication from one state's law enforcement community to another, there will probably be considerable similarity between the institutions for law enforcement between the two states. The barriers to innovation in the second state, i.e., to diffusion of the successful application from the first state, will, it is hoped, be similar to those already overcome successfully.

However, if the objective is to transfer the successful application of emergency police communications technology, to, say, emergency medical services, the situation may be entirely different. The institutions of the medical community are distinct from those of the law-enforcement community. Therefore, the barriers erected to technological innovation will also be different. Whether lessons learned in the law-enforcement community will help to introduce technological change into the medical community is an area in which we have too little experience to allow extrapolation. Presently, then, we do not have either good methodology or enough good case studies to make us consistently effective in the diffusion of technology.

Institutional barriers arise because institutions exist which generate and impose them. The types of institutions to which we refer in this regard are those established to represent the interests of some special group. The United States Government, for example, is the only institution established to represent the interests of the American public. Institutions exist for very obvious reasons, including: mutual aid and education within their constituency; protection of their membership from external pressure; income maintenance for their members; insulation of their members from economic forces not under their direct control; exclusion of incompetents from their ranks; perpetuation of special privileges.

The existence of an institution implies the existence of an organization serving a special-interest group or a "privileged" group. As indicated earlier, the only institution established to serve the public and its special interests is government. Even here, the behavior of the institution, i.e., government, may pose barriers to the progress of planned change.

Institutional barriers to change or to technological diffusion arise principally when the reasons for existence of the institution lead to institutional practices, consequences of which are in conflict with the goals of the change. Examples of such practices come to mind easily when one views an illustrative list of types of institutions found in the United States today. The list of those in the area of public services, for instance, would include: state, municipal and local governments; federal government; labor unions; consumer groups; professional societies; service organizations; religious organizations; informal organizations of "privileged groups"; national commissions and councils; informal "advocacy" groups.

Although widely varying in interest and membership, such institutions do exhibit considerable similarity in their behavior and their characteristics. They are egocentric, with their objectives developed internally to meet the stated needs of their membership. They are intentionally oriented toward survival and perpetuation of the special-interest or privileged group they serve. As a result, they have well-developed internal mechanisms that provide remarkable stability to their operation.

Institutional behavior is dominated by excellent practices for monitoring the "external" world in order to detect threats to their objectives and their membership. This institutional surveillance is accompanied by an institutional ability to adapt so as to employ the best strategy and structure to combat detected threats.

Generally, the institutional strategy for adaptation to insure survival depends on some combination of preventing change which affects them or their membership in a manner deemed adverse to its best interest; controlling the rate of change; recommending alternative changes whose effects would be felt by other institutions; or suggesting changes that would better their membership or themselves.

Institutional survival, in turn, depends on excellent internal communications networks and the production and packaging of institutional products, generally information, to achieve the greatest impact on their membership and on those whom they wish to influence.

A real problem is the present lack of understanding of how to deal with institutional barriers, even when they are identified and their deleterious effects cited. For example, suppose that the American Medical Association was identified as posing the greatest barrier to the humane diffusion of technology to improve the quality and decrease the cost of medical examinations. There is no direct way of influencing the Association's behavior, and one might expect the public to become even more unhappy if this actually were shown to be the case.

Or perhaps the Parent-Teachers Association (PTA) posed the principal barrier to the use of audiovisual and computer-communications technology to accelerate self-paced education in public schools. Again, there is no direct way of influencing the Association's behavior.

The removal or handling of institutional practices tending to produce barriers to technological change presently utilize the practice of providing information or education to the public to arouse their awareness of the problems, to promote their understanding of the issue, and to allow them to exert public pressure for removal of the barriers. This course of action is, of necessity, slow. Nevertheless, it is certainly always preferable to more precipitious action based on inadequate public knowledge.

This is the situation in which we find ourselves today regarding one of the man-made constraints to science-induced change. We must either work with it or replace it with a better means of protecting the rights of individuals and of large societal groups.

Replacing Big Problems with Little Problems

Today, there appears to be national confusion on how to deal with science and technology. National opinion seems to vacillate from contempt to fear to awe. When faced with widespread confusion in an indeterminate situation, mathematicians often employ an intermediate step in obtaining answers: they introduce hypotheses and then try to prove or disprove them. If the situation is sufficiently illogical to defy logic in dealing with hypotheses, conjectures can be introduced instead. What is offered here are conjectures and suggestions intended to replace big problems with little problems.

CONJECTURE 1

A rebellion against traditional science and technology is in process. It is nurtured by a growing awareness of the potential problems posed by advances in science and technology. Among these problems are:

Increases in societal and national discomfort due to an inability to control or pace scientific advance.

Aggravation of existing scarcities of resources, caused principally by applications of science without informed national or local consent.

Changes in existing interdependency relationships among groups and nations, with too little foresight in evidence, and

Creation of "power structures" comprised of those few who, through scientific or financial credentials, can select and control the applications of science and the spread of technology.

Discussion and Suggestions. As individuals and society become fearful of the changes which science is causing, they tend to ignore the beneficial impact of science and its applications. A continual effort must be sustained to acquaint people of all ages with the confirmable good achieved through science.

However, people are rightfully concerned about the sciences and technologies which impact directly on them as individuals. Biological and medical science are examples of such sciences, as are computer, communications, and information sciences.

Directed and objective discussions which focus attention on overt or latent areas of individual concern are essential to allay honest public concerns. Examples of targets of public concern in the field of biological and medical research include:

Genetic engineering: e.g., biomedical research concerned with gene manipulation and the devising of genetic messages.

Population control: e.g., birth control, increasing the life span, selective breeding through biological means, etc.

Behavior modification/control via drugs (pharmaceutical research).

Specialization of intelligent species for alien habitats, such as space or underseas.

Real-time management of body functions via implaced sensors and active-automated controls.

CONJECTURE 2

A principal area of societal discomfort is the correct recognition that the public and its selected or elected representatives cannot, today, adequately predict, pace, or direct science and technology. The public and science will be at odds until this issue is settled to the satisfaction of the majority.

Discussion and Suggestions. Many issues of vital concern to individuals or nations are now being decided by institutions or groups which transcend national governments, which are informally constituted, serve special interests, and are subject to few, if any, traditional national or international governances. In many instances, research or research applications are directly the subject of decisions by such special-interest groups. In other instances, research and/or research application are directly affected by funding available principally from such "supralegal" groups. If this situation continues or becomes more common, the areas, pace, and direction of research may soon be "forever" beyond the control of individuals and their selected or elected representatives.

Examples include:

> The world-wide banking community and computer/communications research directed toward world-wide electronic funds-transfer functions. Decisions being made by this special-interest community are already affecting national abilities to make policy in the national interest.
>
> The medical and biological research communities (e.g., molecular biology and genetic engineering research), one of which—at Asilomar, California in 1975—attempted to develop its own code of ethics governing the conduct of its research, the outcome of which has a profound impact on the future of individuals.

Attention must be directed, with public participation, to resolving conflicts between the unfettered advance of science and the pacing of research with informed consent by providing means for understanding the general conditions required for scientific freedom *and* responsibility; developing criteria and mechanisms for reviewing instances in which scientific freedom has been abridged; and establishing scientific codes of ethics or other mechanisms for discharging scientific responsibility.

One instance of progress in the area is exemplified by the American Association for the Advancement of Science (AAAS), which addressed this issue in *Scientific Freedom and Responsibility* (AAAS, 1975). The issue needs additional attention in order to make highly visible the problems attendant upon simultaneously achieving adequate scientific freedom and responsibility.

Resolving the issue of scientific freedom and responsibility is a necessary *but not* a sufficient condition for predicting, pacing, and directing research and its results. Its companion issue is whether *institutional* freedom and responsibility can be so defined and met as to preclude predictable potential problems bred by research. The relative powerlessness of public groups should be viewed with alarm by the public and its representatives.

Government is the only institution which has the public as its constituency and which serves the public. There has been little success in finding mechanisms to assure the public or its representatives that private institutions can, will, or should assume society-wide responsibilities for research they perform, fund, or apply.

It appears that, as public desire increases to have advances in science and technology understood and paced, as appropriate, by its elected or selected representatives, the actual control of research is passing into the hands of an uncontrollable few. Examples include the support of science by a few oil-rich nations; the concentration of research in particular fields within one or two dominant profit-oriented companies; and the migration of research to nations rich in research freedom because of their lack of environmental or other public-oriented safeguards.

The outcome of this trend should be publicly discussed and actions taken in the best national interest.

CONJECTURE 3

Progress in putting science and technology back together again in the Humpty Dumpty tradition will be thwarted until there is better understanding—by the scientific, legal, and policy-making communities alike—of the differences between "management" of science and "management" of technology.

Discussion and Suggestions. Joseph Agassi has dealt superbly with this topic. As he properly states in *Science in Flux* (1975, pages 336–337) there is a sharp dichotomy "between science whose chief role is to provide testable explanations and eliminate the worst of these, and technology whose chief role is to introduce practical proposals, eliminate the worst of these, and volunteer implementation of the uneliminated. Whereas standards of criticism in science may be raised as much as it is within our reach, one can easily overdo the standards of assurance necessary prior to implementation."

One of the greatest frustrations to scientists and technologists today is the outpouring of legal justifications, guarantees, positive assurances of negative effect, and governmental permissions needed to test, to implement, or to initiate either research or technological diffusion. These safeguards are understandable; they are symbols of the public's fear of uncontrollable scientific-induced change and of the harmful impact of science.

Nevertheless, the existence of such well-motivated, but improperly imposed, safeguards is sure to lead to sluggishness and stagnation in science and technology. Indeed, it may have already.

An urgent public call for national attention to scientific safeguards and to preventive technology should be forthcoming.[1] The problems of bringing stability and balance into the interactions of government, science, technology, special-interest institutions and the individual deserve to be accorded the highest priority world-wide.

Many individuals and groups are worrying about science and technology of the third century of America, and the year 2000. The real challenge is to take today's environment, the individuals living today, the science and technologies now being pursued, and to effect a change into a more desirable environment in the next twenty years. A more productive and happier citizen and a more comfortable society will be our reward.

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IV

FUTURE DIRECTIONS IN BIOMEDICAL RESEARCH

Introduction

PATRICK E. HAGGERTY

THE THEME OF THIS SESSION is "Future Directions in Biomedical Research." Implicit in that title is the optimistic inference that there is a future worth striving for and directions to be sought which will prove to be fruitful in their consequences for man, his society, and his environment.

Even though incomplete and tentative in detail after detail, surely among the more stimulating and disturbing items of knowledge we have gained from intellectual strivings of the sort we have been discussing today are those relating to the development of life on earth, to which we have attached the broadly descriptive title "evolution." Surely, too, and flowing from those items of knowledge, the comprehension of ourselves as interrelated and active parts of our total environment, and our dependence and influence in extraordinarily complex ways on the preservation and enhancement of that environment represent some of our very recent and significant increases in wisdom.

Yet, it seems to me that, in our considerations of and our speculations about evolutionary processes, we tend to do two things:

> We exclude the acts of man—wise and unwise—from our considerations of evolution.
>
> We assume that evolutionary developments, excluding the acts of man, are positive in value, i.e., that the evolutionary system is self-policing for the good of the over-all system.

The latter can be true only if one assumes that, whatever the end product of the natural processes involved, it is the best of all worlds possible at that time. I suggest that if the dinosaur could have a point of view, it would not be that one.

On the first point, when we exclude the acts of man—wise and unwise—we are, in fact, omitting what may well be the most influential evolutionary forces acting in future millenia. Man is a different and much more powerful creature when equipped with the tools he has developed over the past few centuries. He will be an even more different and more powerful creature as his mind is aided and expanded by the inexpensive and increasingly complex elements of electronic logic and memory—elements known to us for a few decades in the data-processing machines we call computers, but which are just now beginning to be dispensed broadly in such still relatively trivial applications as electronic watches and calculators. Man so equipped—his muscles multiplied by his tools, his mobility extended to the boundaries of the planet and beyond, his mind and memory dynamically expanded by electronics—is in a completely different relationship with his environment, for good or bad, from his predecessors. When so equipped, generation after generation, man is, in a very real sense, evolving into a different species. Even though the muscle- and mind-expanding tools themselves are external to his body, the knowledge and skill to use them are within the body. When associated with the creation and operation of institutions to preserve, enhance, and transmit that knowledge and skill to generation after generation, they become a kind of evolutionary development with an impact on man and his environment of the scale we have customarily associated with the passage of eons of time measured in millions of years.

It seems to me that this view of evolution is implicit in the title "Future Directions of Biomedical Research," in the expectations it expresses that some of the more gifted among us will deliberately seek and find "Future Directions" relating to the life within us and thus, inevitably, to the life that encompasses and surrounds us!

In that sense, we are especially fortunate that the topic is being discussed by Dr. Lewis Thomas, now president and chief executive officer of the Memorial Sloan-Kettering Cancer Center, but also a researcher and practitioner in neurology, pathology, pediatrics, and education. I can best communicate why he is so eminently suited to view this topic by quoting from some of his own writings.

Many of you, I am sure, have experienced the delight of reading his beautifully written and stimulating essays collected under the title *The Lives of a Cell.* Here are a few paragraphs from one of those essays:

> We have become, in a painful, unwished-for way, nature itself. We have grown into everywhere, spreading like a new growth over the entire surface, touching and affecting every other kind of life, *incorporating* ourselves. The earth risks being eutrophied by us. We are now the dominant feature of our own environment. Humans, large terrestrial metazoans, fired by energy from microbial symbionts lodged in their cells, instructed by tapes of nucleic acid stretching back to the earliest live membranes, informed by neurons essentially the same as all the other neurons on earth, sharing structures with mastodons and lichens, living off the sun, are now in charge, running the place, for better or worse.
>
> Or is it really this way? It could be, you know, just the other way around. Perhaps we are the invaded ones, the subjugated, used.
>
> Certain animals in the sea live by becoming part-animal, part-plant. They engulf algae, which then establish themselves as complex plant tissues, essential for the life of the whole company. I suppose the giant clam, if he had more of a mind, would have moments of dismay on seeing what he has done to the plant world, incorporating so much of it, enslaving green cells, living off the photosynthesis. But the plant cells would take a different view of it, having captured the clam on the most satisfactory of terms, including the small lenses in his tissues that focus sunlight for their benefit; perhaps algae have bad moments about what they may collectively be doing to the world of clams.
>
> With luck, our own situation might be similar, on a larger scale. This might turn out to be a special phase in the morphogenesis of the earth when it is necessary to have something like us, for a time anyway, to fetch and carry energy, look after new symbiotic arrangements, store up information for some future season, do a certain amount of ornamenting, maybe even carry seeds around the solar system. That kind of thing. Handyman for the earth.
>
> I would much prefer this useful role, if I had any say, to the essentially unearthly creature we seem otherwise on the way to becoming. It would mean making some quite fundamental changes in our attitudes toward each other, if we were really to think of ourselves as indispensable elements of nature. We would surely become the environment to worry about the most. We would discover,

in ourselves, the sources of wonderment and delight that we have discerned in all other manifestations of nature. Who knows, we might even acknowledge the fragility and vulnerability that always accompany high specialization in biology, and movements might start up for the protection of ourselves as a valuable, endangered species. We couldn't lose.*

*From *The Lives of a Cell* by Lewis Thomas, copyright © 1973 by the Massachusetts Medical Society, reprinted by permission of The Viking Press.

Future Directions in Biomedical Research

LEWIS THOMAS

We use the hybrid term "biomedical" science as shorthand to describe the whole inquiry that underlies modern medicine, and there are two good reasons for doing this. One is, of course, that it is biological science that most of us in medicine are betting on for the future, and it therefore seems natural to attach the words biology and medicine together to name the enterprise. The second reason, to face it squarely, is that there really isn't enough medical science to enable the term to stand alone, by itself.

This is not yet a widely enough acknowledged fact. Indeed, there is a popularly held opinion which takes precisely the opposite view: that medicine, all by itself, has come a great distance, maybe nearly its full achievable distance, just within our lifetimes; that we now know almost everything knowable, and can do pretty much everything that we're ever going to be able to do. It is even said that medicine has become too scientific, that it is being damaged by the harmful effects of all its sciences, and that the technology resulting from the science has become unendurably expensive.

If you believe this, you are entitled to think that perhaps it is time now to call a halt to inquiry, stop the science, and settle down to apply today's store of knowledge with more intelligence, so that more people can be benefited, and with equity. Clearly, if the science of medicine has moved as far as it is likely to go, there must be something appallingly wrong with the way we are delivering its benefits, for there is still a formidable roster of incapacitating and fatal diseases, and people seem to be dying from these at about the same rate as twenty-five years ago. Why is this so? Is it because we have not learned how to apply today's information? Or is it because there is something wrong with the information?

And now, in the midst of this argument, new voices are being raised in an effort to simplify the whole problem of disease by blaming it, simply, on *wrong living*. Suddenly, hygiene has been rediscovered. If you want to avoid heart disease, eat less animal fat and ride your bicycle. Hypertension is a result of social stress. Cancer is totally and comprehensively explained by external contaminants in the environment; get rid of these and thus be rid of cancer. Live a more sensible life, get plenty of sleep and a good breakfast, give up smoking and drinking, eat less, and you can stretch out your life by eleven or twelve extra years.

Preventive medicine is being urged on us from all sides, as though we'd never heard of it, nor ever hankered for it to become, some day, a reality. And if you fail to prevent disease, through some unspecified oversight, then early detection is the thing; if you can check the progress of glaucoma or cervical cancer by early detection, why not do the same for coronary disease, arthritis, diabetes, stroke, and all the rest? This has become the public expectation, and it is our misfortune not to have been sufficiently candid about the impossibility of such an expectation, at this state of our knowledge.

I had better say, right now, that I have no quarrel with those who propose a new "holistic" approach to health, and I devoutly hope they are right in their prediction that very substantial improvements in the quality and duration of life can be brought about by making changes in the life habits of the population at large. My trouble is that I carry around a list of about 20 diseases, taken straight from the pages of the U.S. Vital Statistics Report; they are the 10 leading causes of death in this country, and the 10 most common causes of serious, incapacitating illness. I cannot really make a connection between most of the items on the list and what, for want of a better term, we call hygiene. Mind you, the diseases on my list are not exotic or esoteric; to be concerned and worried about them is not, in my view, to be biased by "disease-orientation," as is sometimes said. These are the principal problems of modern medicine; they are the illnesses that kill or injure most people; they are what many of the people who come to doctors' offices or to clinics are really worried about, and need reassurance that they don't have. And I do not know for sure, with today's level of scientific information, what we can do to prevent or cure them. There are, to be sure, a lot of good guesses, but I am talking now about scientific proof. Do we really know that changing the diet of the American people will eliminate coronary occlusion? And if so, do we know how to change the diet, and to what? Some of us think so, but it is not yet a matter of scientific proof. Except for cancer of the lung, can we really prevent cancer in human beings? I doubt it. Do we know how to prevent the vascular manifestations of diabetes? Stroke? Chronic hypertension? Senile dementia? Schizophrenia?

We must be careful, in my opinion, not to make promises about preventive medicine—as we should have been (but weren't) about curative medicine in the past quarter-century.

For, if the truth be told, we are still at a very early, primitive stage in the development of medical science. There is nothing disparaging about this statement. On the contrary, it ought to provide a source for the greatest optimism about the future. It is not that the science has not been getting anywhere, or is stuck somehow; there are the most convincing sorts of evidence that it is moving, and getting ready to move faster and more productively. But it has to be said that it is just at its beginnings, and most of its new world still lies ahead.

The greatest single accomplishment in medical science, to date, is in the field of infectious disease. There have been a few others, as we shall see, but none is on the great scale of the achievements in infection. This has grown to the stature of a full-fledged, proper science, with both basic and applied fields of real power, encompassing a range of inquiry extending from molecular virology to the treatment of lobar pneumonia and the prevention of measles.

How did this largely satisfactory state of affairs come to pass? Was it, as is sometimes said, the sudden transformation of this branch of medicine into a high technology by the quite accidental discovery of penicillin in the 1930s, followed, more or less automatically, by the development of all the other chemotherapeutic agents now at hand? And if so, why did this happen so asymmetrically, without being accompanied by any corresponding, comparable transformation of other fields of medicine? How could we have been so lucky with infection and, at the same time, remained so unlucky with heart disease, cancer, stroke, schizophrenia, arthritis, diabetes, chronic nephritis, and all the rest?

The answer, of course, is that the science of infection did not begin with the discovery of penicillin. As it happened, infection was the first field to begin moving out of dogma and empiricism and into genuine experimental science, and it had a long head start on all the other branches of medicine. It began more than 60 years earlier, around 1875. This date marks the real beginning of the science of medicine.

Penicillin did not simply drop into our laps in the mid-1930s, nor did sulfanilamide. These agents, and their successors, could not have been dreamt of, in the 1930s or now, had it not been for the preceding 60 years of steady, intense, and often brilliant basic research, which established, first off, that there were such things as microbes and microbial diseases, and then succeeded in sorting out the various infectious diseases by name, so that we knew with certainty which ones were caused by which bacteria or virus. This astonishing body of work, launched by Pasteur and carried through by Koch, Behring, Metchnikoff, Theobald Smith, Roux, Bordet, Ehrlich, and other illustrious scientists of the early twentieth century, represents a landmark advance in human affairs. Without that work, we would not have a glimmer as to the etiology of lobar pneumonia, tuberculosis, syphilis, or scarlet fever, and we would still be thinking that typhoid and typhus fever were variants of the same mysterious disease. We would not be able to imagine what to do with penicillin or streptomycin even if we found them in labeled bottles.

It took a long time, more than half a century, and a great deal of hard work by several generations of fundamental scientists. As the work proceeded, the phenomenon of immunity came into view, around 1890, and the existence of antibodies, complement, phagocytosis, and the special gift of specific immunization against diphtheria, pertussis, tetanus, and other infectious agents were recognized. Landsteiner's work with chemical haptens established the molecular specificity of the immune reaction. By the 1920s, Avery, Heidelberger, Cole, Goebel, and others had delineated the specific polysaccharides of pneumococci, and the new field of immunochemistry was well launched. The various types of Group-A hemolytic streptococci were identified by Lancefield and her associates, and the relationship of streptococcal infection to rheumatic fever was uncovered in 1933 by Coburn.

With the general exception of immunization technique, there was very little "pay-off" during most of this long period of basic research. By the mid-1930s, some cases of pneumococcal pneumonia was being effectively treated with specific antisera, but that was about the sum of the technology. The arsenicals, mercury and bismuth, were in wide use for syphilis, but at such high cost in time and hazard that I doubt we would find them acceptable today, even if we had no other means of treatment. Tuberculosis was essentially untreatable, as were typhoid, brucellosis, and infections by streptococci and staphylococci.

In retrospect, it was a very lucky thing for all of us that the work went on, even though it must have seemed, from time to time, like a waste of effort, an enterprise that was uncovering a great deal of information for which there did not appear to be any practical usefulness. What kept it going, I suppose, was the lucky fact that the problems in infection were of such intense interest, as biological puzzles, to the investigators of that day. Also, I imagine, there must have been a generally shared hunch, among the scientists, that sooner or later something was bound to turn up that could be used against human disease. Nevertheless, looking back on the record of events, it took a long time, and it must have been a frustrating period to live through, for the investigators and their sponsors, and for the physicians who were waiting anxiously for something of practical value.

Anyway, by the late 1940s, the field was well established as an applied science. Some of the major infections which plagued us all before then have literally vanished since, and most of the others have come under effective control. There are still important unsolved problems, most conspicuously the lack of any technology for the treatment of virus infections, but the prospects for the near-term future seem bright enough, and the work goes on. The story is nowhere near over. There are enticing problems all over the place: the slow viruses and their possible role in degenerative diseases of the central nervous system; the possibility that undetected infectious agents may be responsible for rheumatoid arthritis and disseminated lupus; the role of viruses in immune-complex diseases, perhaps including chronic nephritis; the virus-cancer problem.

For all of these new problems, and for each of the other infection problems already solved, the common feature which has distinguished them as a class from the other unsolved problems in medicine is the known existence of a central, fundamental mechanism of disease. The relative success achieved by the field of infectious disease to date is due to the discernibility of this crucial participant in disease mechanism—the microbe—accessible, available for manipulation and experimentation.

By the 1950s, when the major programs of the National Institutes of Health were being organized, there were no comparable insights into the inner mechanisms of the other great diseases of human beings. In contrast to the infectious diseases, the research on these other problems had to be started virtually from scratch, with nothing at all to compare with the storehouse of banked knowledge available for infectious disease. Even now, for some of the most important ones, including heart disease, cancer, and stroke, there is nothing equivalent to the handhold on an inner mechanism of disease that was provided by the recognizability of microbial agents in the late nineteenth century. There has, in short, been a lot of catching up to do.

There is another conspicuous difference between infection and the other disease problems—the difference in the cost of caring for them. Any illness requiring six weeks or more of hospitalization, with the full services of today's diagnostic laboratories as indispensable items for proper care, and sometimes with intra-abdominal or thoracic surgery as an essential measure, represents a catastrophic illness in economic terms. Without antibiotics, typhoid fever and lobar pneumonia would surely be in this class; they were the most commonplace of all illnesses in the wards of our city hospitals earlier in the century, and each episode of such a disease would cost, by today's standards, something more than $10,000, at the least. Today, with a decisively effective technology for turning off or preventing infections of this kind, the cost is measurable in cents, rather than dollars. The same generalization applies to tuberculosis. Instead of sanatoriums, periods of bedrest measured in years, cyclic fads of mountain air, sunlight, ocean voyages, Arizona, Saranac, massive surgery to collapse or remove the infected lung tissue, and all the rest, the whole disease now can be eliminated in a few days by a short course of drugs that cost a few dollars. Tuberculosis meningitis was one of the master diseases when I was a medical student; it was common, and it had a mortality of 100 percent, no less. It is now rare, and quickly and easily cured.

It is often said that, as medicine becomes more of a science, the costs of care become higher and higher, but the truth is just the opposite. When the science is really far enough advanced so that the resulting technology can deal directly and decisively with an underlying disease mechanism, the costs go down. The more effective the medical technology, the simpler it is, and the cheaper. The cost is at its highest, and the technology at its most complex, when we are only halfway along.

We are only halfway, or less than that distance, in our understanding of the causative mechanisms in heart disease, cancer, stroke, nephritis, arthritis, schizophrenia, and the others, and what we have for therapy is, correspondingly, a halfway technology, costing enormous sums of money and involving high complexity. The coronary care unit and open-heart surgery for coronary disease illustrate the dilemma. We do not really have a clear understanding of the mechanisms of coronary occlusion; it may have something to do with the diet of Western man, although this is, as I have asserted, by no means a matter of scientific proof. Even so, even if you concede this speculation, there is still no real insight into the determining events that affect the arterial wall, and no way of getting at these events, either to prevent them or turn them around. In this circumstance, the best we can do is wait until the disease has occurred and the damage to the heart has been inflicted, and then to use technology for coping with the results of the disease. There is no way out of this dilemma until we have gained more information about the disease. We are as compelled by our professional obligation to do whatever can be done as we were in the days when iron lungs and limb braces were the only technology available for poliomyelitis.

Hypertension may be moving us, at least temporarily, into a similar situation. It is now known that anti-hypertensive drugs have some beneficial effects on the final outcome of essential hypertension. But the benefit is by no means total; not all patients are protected against coronary occlusion or stroke or renal failure, nor can all types of complications be prevented. Moreover, the drug treatment, to be at all effective, must be for a lifetime, and it requires a degree of patient compliance amounting to ardor if it is to be maintained optimally. To detect and then treat all eligible hypertensives—ten million or more in this country—will be an enormously expensive task and an uncertain one, at best. But the real difficulty is that the elevation of blood pressure is a manifestation of the disease, certainly not in itself a cause, and the underlying mechanisms which are responsible, in a primary sense, for hypertension remain entirely unknown. Perhaps, in a better world, when we have learned enough about the walls of arteries, we will have a clearer view of the central factors governing essential hypertension, and other, more decisive measures for preventing or terminating the disease will come to hand.

Renal failure, usually the result of chronic glomerulonephritis or pyelonephritis, provides another example. At today's level of understanding, all we can do is wait until the damage to kidney tissue has run its full course and then try to replace the lost organs by the use of dialysis machines or kidney transplantation. And there is no way out of this, as things stand today; no one can really say that it is too expensive to do these things, when individual, young human lives are involved. The only conceivable hope for the future, both for the patients and the societal agencies which must meet the costs, is in more research into the underlying mechanisms of chronic nephritis.

My last example is cancer. Here, we are in possession of a variety of halfway technologies, some relatively effective, some totally ineffective, but all directed at the existing, already established cancer tissue. We have to deal with the disease after it has become a threat to life, and our only workable approach is an attempt to destroy the existing cancer tissue by surgery, radiation, or chemotherapy, maybe soon by immunotherapy as well. But we do not yet have the kind of fundamental information about the underlying process of neoplasia that will permit us to turn the cancer cell around, or to prevent the transformation in the first place. When we reach this stage—and I am confident that it is within reach, sooner or later—we will find the measures needed for coping with cancer to be much simpler, and less costly, than those we must rely on today.

I do not know anyone now involved in cancer research at the fundamental level who is not highly optimistic for the long-term future of this problem, and I sense that my colleagues in cardiovascular disease are equally hopeful for the new lines of research on their problems.

In short, these are busy times for the students of human disease, and a good many of the mysteries are beginning to look penetrable. I can assure you, from a first-hand personal experience in the matter, that there has not been a time like this in the past forty years. Indeed, fifteen years ago, when the biological revolution was just getting under way, things were still quiet and relatively inactive in medicine. Now, new information is coming in cascades, and is filled with meaning and astonishment for all of us. And it should not need mentioning that the greatest part of this information has come from laboratories engaged in the fundamental biological sciences—from the fields of immunology, bacteriophage and microbial genetics, cell biology, membrane structure and physiology, neurophysiology, and molecular biology.

Moreover, it is my belief that we are just at the beginning. During the past quarter-century we have built in this country an unprecedented system for biological and medical science, due in large part to the evolution of that most extraordinary of all scientific institutions, the National Institutes of Health. It is in the nature of new information, in whatever field, that it leads by a sort of catalysis to the continuing production of still more new information, and that is what has been happening in biomedical science. It is simply inconceivable to me that the kinds of insight we are now obtaining, at more and more profound levels of understanding, into the form and function of living tissues, cells, and the smallest parts of cells, will end with nothing more than an appreciation of the normal state of living. It is, in my view, an absolute certainty that we will also come to an understanding of disease mechanisms, at the same profound level. The great advance in medicine, which has occurred just within the past quarter-century, is in the knowledge that there are no longer any incomprehensible, unapproachable diseases.

For these reasons, I have a high degree of confidence for the future of medicine. My only uncertainty in the matter comes from an anxiety that maybe we won't keep at it, or won't be enabled to keep at it, or will only keep at one part and not the other. The last is my deepest worry—that we or our masters may decide that disease-oriented research is all right to do, and worth supporting, but that fundamental biological science is something else, a luxury too costly or too frivolous, and that kind of decision could turn the whole process off. We have already learned, from the experience of the last decade, that these cannot be regarded as two separate kinds of science; they operate marvelously well together if they are given any sort of institutional encouragement; they feed on each other and feed each other. In the end, we will perhaps all come to agree that disease is itself the most fundamental and intrinsically fascinating of all biological problems, but at the moment I recognize this as my own personal bias, and acknowledge that my colleagues in nonmedical fields of biology are not quite so sure. Nevertheless, I can feel a sort of consensus these days, among the best of the professionals, that disease research and pure biology for its own sake are intimate and interdependent parts of the same larger field, and that, I can assure you, is a new phenomenon in science.

What is likely to come of this, in the best of possible worlds? Eventually, if all goes reasonably well, nothing less than the control of human disease—if not the outright elimination of disease, at the least a technological capacity to turn it around and govern it when it occurs.

This does not mean as much as it sounds like meaning. It has nothing at all to do with death, beyond the prevention of premature death. No matter how skilled we become at controlling or abolishing the last of our major diseases, we will still die, and probably die by the same, unalterable genetic clock as always. We will still grow old, although aging will not be the incapacitating and humiliating disorder that it is for most of us, sooner or later, these days.

Most of all, it needs saying that the prospect is in no large sense Utopia. To be free of disease is, by itself, no assurance of happiness. We will still be as vulnerable as ever to the disorders of our society, and if our social structures come unhinged we will be back where we started, plagued by all the diseases ever recorded, including all the old infections. It will be a delicate balance, needing constant attention.

Finally, the benefits that I have been discussing will have very little effect on that 75 percent of calls on the doctor that are said not to concern organic disease, except to the extent that there will be less anxiety about particular diseases and, perhaps as a result, fewer calls for reassurance. In this connection, it is worth recalling that when poliomyelitis came under control there was a dramatic decrease in the demand on the time of pediatricians, not because polio was all that common, but because great numbers of parents no longer required the assurance that their febrile child was not coming down with the disease. When cancer vanishes, as I believe it will, people will not live in the kind of constant apprehension it engenders today.

So things will be significantly better, and the health-care system will be very much less a drain on the public purse. But not Utopia. We will still have our other anxieties, our neuroses, our fears of meaninglessness, our problems with each other. We will still be compelled to stare at famine and death on our television screens, trying to think up new excuses. Coping once and for all with organic disease will not solve any of these, but perhaps it is safe to say that we will be somewhat better at constructing a workable society if we are at least physically healthy. Given enough time and patience, and enough good luck in the science, that objective, limited as it may be, is within our grasp.

V

THE PURSUIT OF EXCELLENCE

Introduction

FREDERICK SEITZ

OUR INSTITUTION was created seventy-five years ago by the grandfather of our next speaker in order to fill a serious gap in the spectrum of research needed in our country. This step was taken at a time when our country finally had awakened, almost overnight, to the realization that one great cultural breach remained between the new world and western Europe—the appreciation of scientific research. The breach was marked in the United States by two defects: first, by the lack of opportunities available for pursuing scientific investigation; second, by the absence of institutions capable of preparing scholars for scientific leadership.

At that time, the standards of judgment in our country did not permit one to distinguish reliably between the good and the best in matters of intellectual creativity. However high the hopes of our founder may have been for the future of The Rockefeller Institute for Medical Research, he was prepared to accept a modest level of achievement, if fate so willed. But he did expect that the new institute would do its best, and that it would at least provide a model for other new institutions that might in time prove to be even more creative.

Through a combination of sound instinct, good judgment, wise advice, and solid financial support, Mr. Rockefeller was able to create an institution that soon joined the ranks of the greatest on an international level. From the start, our institution was in a position to place emphasis upon quality, rather than quantity. It set a few major goals—focusing on the most difficult problems in the sciences related to medicine. It strove to reach these goals by the maximum effort that the human and material resources available at the time permitted. That is still our mission here and that is still what we try to achieve.

In the intervening years, our national outlook has changed in many ways. With these changes, there has been a tendency to favor equitable distribution, large size, and other factors related more to quantity than to quality. Public monies often tend to be spread too broadly when a problem becomes of general concern, whether that problem is cancer or the cost and availability of energy. To a degree, this trend is both understandable and necessary, related as it is to the desire to insure equal opportunities for all citizens in more and more areas of human endeavor. Yet those of us who have had the privilege of spending our lives in the fields of science know, as do those in other fields, that the key to progress most often lies in backing the abilities of the unusually gifted person. In many situations, the combined labor of a group of average workers cannot replace the contribution of a single exceptionally gifted and inspired individual.

Therefore it is an honor to introduce a good friend of science and the arts, and a devoted spokesman for the highest standards around the world. David Rockefeller volunteered to speak today, even before our program had been developed, because he feels strongly about the need to emphasize the pursuit of excellence.

The Pursuit of Excellence: Some Reflections on the Wealth of Nations

DAVID ROCKEFELLER

THERE MUST BE more than a grain of presumption in my selecting excellence as the subject of a talk on an occasion as auspicious as this one, in an environment as prepossessing as that of this University, and before an audience as distinguished as the one I address here.

Yet it seems to me that many of us tend to be derelict in neglecting to express more firmly and more frequently our appreciation of excellent achievement or to reaffirm our faith in the ideal of excellence. Like many of you, I was brought up with the idea that excellence was necessarily the goal we must seek to achieve; if we failed, it could permissibly be for lack of talent, but never for want of effort. Indeed, it was one of the assumptions of my earliest childhood—like love of country and faith in a divine Being—that excellence would remain an unshakeable goal for all humanity.

In recent times, however, there have been signs that the unshakeable is growing shaky. Whether excellence is or should be a firm and unyielding part of our social commitment, or whether the time has come for excellence to give way to such expediencies as satisfying expanded social or economic demands, has become the subject of open and widespread debate. I would like on this occasion to address myself to that question.

But first let me explain what I mean by excellence. To my mind the term denotes, first, those high peaks of intellectual accomplishment, of insights about humanity and the universe, of creative and artistic achievement—to which the best among us aspire. Second, I think of excellence in terms of the standards that are set and that society accepts as a result of those high achievements. And third, I think of excellence as the pinnacle toward which we reach out in our work and our own contributions to humankind—in short, the best that each of us is capable of accomplishing.

To my knowledge, no responsible voice has yet been raised to vilify excellence, either by my definitions or any other. The denigration is rather more subtle. It may take the form of opposing intellectualism on the ground that it is a kind of snobbery, and so belittle rare gifts or significant achievement—whether artistic, scientific, economic, or intellectual—and raise in their place the mean or average in what amounts to a kind of idolatry of mediocrity.

Now, anti-intellectualism is neither new nor unexpected in a functioning democracy like our own. Indeed, by tracing back at least to the age of Jackson, we find it has been nourished by successive waves of popular support. The impact of anti-intellectualism has been felt often in our political life and, though perhaps less often, in our popular culture and in catering to the least-common denominators in our systems of education and of job standards and requirements. This is not at all surprising, because intellectualism presupposes the recognition of a small, select group with exceptional gifts—an elite, if you will. A democratic ideal that reveres the common touch may, at the same time, distrust the exceptional or the elite, whether it be an elite of patrimony, of wealth, or of special gifts, such as intellect.

But in the past, behind the resentment and envy of the exceptional and the outstanding, there was always a deep-seated admiration, however grudging, for great capability and accomplishment. That high regard was part of the unspoken compact that existed in a society held together by a sense of rationality, by an understanding of the relationship between means and ends, and by a conviction that the fruits of excellence are enjoyed in some degree by all.

Now, however, there are mounting signs of a fundamental breach between the democratic ideal of equality and the respect that has traditionally been accorded to excellence. One reason may be that the notion of equality is being broadened. Where once it was sufficient, if not always easy, to work toward equality of opportunity and equality before the law, the newly emerging standards ask us to reach for absolute equality, or equality of human condition. Theoretically, this is an admirable goal. But if conflict arises between excellence and such egalitarianism—as it might in such fields as education, scientific inquiry, or the assignment of a democracy's more challenging tasks to the best-suited and best-equipped—it is too often excellence that is asked to give way.

Not only is this a matter of political expediency and a tendency of the popular mind; it has become a philosophy with many proponents who themselves possess no small intellectual gifts. The philosophers of equality quite properly extol justice and fair dealings in human affairs, but they insist that fairness can be served only by more equal distribution of all the fruits of human endeavor. By these lights, excellence cannot accrue special rewards, which is, in itself, an inequity.

There is, to be sure, something admirable and attractive in the desire to level the conditions under which men exist. In fact, in our time we have sought to achieve greater equity in those conditions, and we have succeeded to a degree that would have seemed incredible to earlier generations. But the danger lies in stretching the application of equity to the point at which incentives for accomplishment are greatly diminished. All human experience indicates that lower incentives are accompanied by lower efficiency, so that the eventual outcome is a more even distribution of poverty and deprivation, rather than more substantial material gains for the poor and underprivileged. Surely the redistribution of poverty cannot be the goal of the most egalitarian among us. The challenge must be to increase productivity and efficiency to the point at which we produce substantially more with substantially less. And this presents us with the imperative of social invention, which offers limitless opportunities for excellence.

But greater production of goods and services is not enough by itself to meet the requirements of the human spirit. There must be a balance between material and spiritual values. On a visit to China not long ago, our party had the opportunity to visit a ceramics factory that had been in existence for some 700 years. On display was a cabinet full of exquisite work made in the past. But the products being turned out now were mass-produced, uninspired, and totally lacking in artistic value. We were told that individually fashioned works were inconsistent with the aim of making ceramics available to the largest number of people. Indeed, during the ten-day visit to China, I could find no encouragement of individual independence of thought or creativity. Perhaps this was inevitable, at least temporarily, in a country where so many had been living below the poverty line. But if the policy were to persist, the consequences for a civilization which, in the past, has achieved great pinnacles of excellence in many fields could be grave.

It is true that here in the United States we, too, have sacrificed quality in certain instances in order to produce larger and larger quantities of things our people need or want. For example, while the United States was concentrating energies on increasing steel production from 15,000 tons a year at the end of the Civil War to more than 10 million tons by 1890, there were few fundamental improvements in the steel-making process itself. That great old steel-master, Andrew Carnegie, said simply, "Pioneering don't pay." Similarly, Henry Ford revolutionized auto-making by concentrating on a uniform, no-frills, mass-produced automobile for the broadest stratum of society.

But surely the real point is that excellence and the capacity to meet essential human needs efficiently are not in conflict. Both goals have been pursued in this country for 200 years. The Hemingways and the Frank Lloyd Wrights have coexisted with the Fords and Carnegies. Lowell was the surname of both a great textile manufacturer and a great poet. We have demonstrated that creativity, genius, and excellence are to be found in all walks of human endeavor. What may well have been the greatest contribution this country has made to the welfare of the modern world is its healthy respect for talent and what it can accomplish wherever it may be found.

President John Quincy Adams, himself a man devoted to science and the arts, said that "hiding in the earth the talent committed to our charge would be treachery to the most sacred of trusts." Much more recently, Alfred North Whitehead echoed the thought by observing, "In the conditions of modern life. . . , the race which does not value trained intelligence is doomed." And just seventy years ago, Williams James said, "The world . . . is only beginning to see that the wealth of a nation consists more than in anything else in the number of superior men that it harbors."

As the world has opened up, as travel becomes swifter and communication easier, it is not surprising to find mounting discontent and dissatisfaction among the less-developed nations and among the impoverished third of the people on our planet. One of our problems is that we have yet to find a way to communicate to these nations and these people some of the economic truths that many of our own people have difficulty in perceiving. Wealth is not static, not simply a pool of capital or even a great stock of natural resources. Rather, the wealth of nations is dynamic and constantly developing; it rests less in what has been produced than in the ability to continue producing. And its essence lies in the human minds that create, that explore, that design, and that alter the earth to fit human needs.

In many ways, the United States had all the appearances of an underdeveloped nation during most of the first 100 years of its existence. The frontier remained unconquered; the infrastructure was underdeveloped; there was heavy reliance on imports of finished goods; the economy was chronically short of capital. But in other, more important, ways—the strong drive for public education, a deep concern with technological advancement, an eagerness to develop and market its resources—this country was one of the more developed nations of its time.

Development, in the final analysis, is primarily an internal process, and some of the more perceptive minds in the developing nations are aware of this. "No nation, no matter how rich," said Egypt's former planning minister, Ismail Abdullah, "can develop another country." What he might have added is that people—capable, effective, excellent people—make up the most important of all national or international resources. Without that resource, no nation can make its way or retain its position in the modern world.

John Gardner has noted that our society, recognizing that motivation is the engine that powers performance, has made performance the key to status and to upward mobility. In this way, excellence is given external recognition and approval, along with internal satisfaction. And in order to use its own human resources most effectively, a society needs to establish the environment in which excellence can flourish.

That is not to say that excellence can have only one kind of background, or any single source of expression. If you compare the civilization that produced Versailles with that which made possible the writings of Plato, the French society at the time of Cézanne, or the American society at the time of Edison, you find they represent a great diversity of moral and intellectual climates. On the one hand, outstanding performance is given individual social recognition and acclaim, as for great books or great paintings; on the other, there is the anonymity of the builders of Chartres Cathedral, or of Persepolis, which were built over centuries by artists, artisans, and craftsmen who served their societies, but were not heralded as individuals. "Each honest calling," James B. Conant once said, "has its own élite, its own aristocracy based upon excellence of performance."

In any event, regardless of motivation, there are those peaks of excellence against which society can measure itself. The literary work of Goethe, Milton, Dante, and Shakespeare; the paintings of Michelangelo, Velasquez, and Rembrandt; the scientific insights of Newton, Galileo, Faraday, and Einstein; physical monuments such as the Parthenon and the Taj Mahal—these stand out in the civilizations that produced them, and endure through the ages that follow.

"From the altar of the past, carry the fire, not the ashes," a Frenchman, Joseph Fouché, once said. A great civilization has to kindle that fire to produce those achievements of enduring quality that surmount the pettiness of their own time and place and cross epochs in their significance for mankind.

During a period like the Renaissance, when excellence was nurtured and venerated, it was certainly easier to produce a flowering of great art. Could that or something like it happen in China today? Modern China has made tremendous strides in feeding and clothing the great masses of its people better than they have been fed or clothed before in history. That is a major achievement. But it would appear that part of the price they have paid has been to forego, and even downgrade, individual freedom and creativity. What are the implications if such a policy were to persist? Who can say? But perhaps somewhere in China there are still men and women, working unobstrusively and alone, striving to keep alive that nation's great traditions of artistic and scientific endeavor. Let us hope so. In the past, even when official attitudes or popular sentiment have discouraged excellence, there always seem to be individuals or institutions to keep candles burning in dark corners—the monasteries that lit the torch of civilization in the Dark Ages, or the medieval guilds that upheld and raised the standards of craftsmanship.

I am not suggesting that the Dark Ages are upon us again in China or closer to home. But at a time when the values of excellent performance are brought into question, it is especially fortunate that some institutions in our society continue to prize outstanding effort and performance. This is precisely the part that, in my view, institutions like The Rockefeller University must continue to play, and that is why I prize so highly my long and close association here.

I see this institution as one with the highest standards of excellence, as well as one that exercises moral leadership in its own communities of endeavor. The record speaks eloquently of the peaks that have already been achieved here. From these laboratories, for instance, came proof that DNA transmits hereditary information and that animal cancers can be caused by a virus. Here were developed a way to preserve whole blood, making blood banks possible, and the first chemical description of immunoglobulin, one of the body's key defenses against disease. These findings and others like them have bestowed great benefits on mankind—a prime result of excellence.

Excellent performance is the natural outgrowth of freedom, and of the latitude that free people are given. This is a truth implicit with many people here at The Rockefeller University, and many more elsewhere who are exploring the outer boundaries of knowledge. For science and scientific discovery are especially jealous of the freedom to search, to question, to doubt, and to explore to the very limits of what can be known, or even beyond. Norbert Wiener once said that "science is a way of life which can only flourish when men are free to have faith."

This University thus personifies the international character of excellence, the quest of superior people to explore the unexplored and, as a by-product, to receive recognition through benefits to all mankind. Excellence has a contagion that one feels in the University's atmosphere, and that crosses lines of age and position. I am not sure whether it is fact or apocryphal that my grandfather once said to those working at the time in The Rockefeller Institute: "Don't be in a hurry to produce anything practical. . . . You, here, explore and dream." In any case, it is undeniably the spirit in which he established and endowed the institution.

In our drive to produce more and more for a better life for the many, we in this country helped to empty the reservoirs of ideas supplied by people in other lands. We could no longer say, with Carnegie, that "pioneering don't pay," because clearly somebody's pioneering *did* pay, and the time came when *we* had to pioneer and to fill the reservoirs with new inventions and discoveries. It was an unfamiliar role, but one we quickly assumed with our customary drive to do more better than had ever been done before. That is why it is important to have institutions like this one, with the means, the talent, and the dedication to risk money, equipment, time, and often the best years of people's lives.

The Rockefeller University is more than simply a research center. In the very best sense, it is also a teaching institution. And by the best sense, I mean that faculty and students here work together, explore together, draw from each other. Any great teaching institution must give much to its students, but it must also demand much from them. In education, particularly, excellence demands a sense of discipline and adherence to the highest standards. For a great many years, our educational system has been so obsessed with meeting what is regarded as the needs and desires of the students that it has downgraded its own important role in meeting the needs and desires of society, which, in the last analysis, is the goal of a student approaching maturity. Excellence, after all, is not parochial; it is what the judgment of generations holds it to be. When society accepts the importance of high standards, individuals will raise their own personal standards.

In this, the 200th year of our national independence, it seems appropriate to recall that many of the problems we face today were those that troubled our founding fathers when they were bringing the nation into being. They were admirably equipped for the task. Our country's emerging leaders were themselves an intellectual elite—the scholars and the scientists, the students of government and of society; in short, the most competent people of their time. They sought, even as we do today, to find the right balance between greater elitism and greater democracy, between equity and reward for achievement. The Constitutional Convention rang with debates between the elitists and the egalitarians.

The government they shaped incorporated both objectives. And the nation to which they gave birth has, over two centuries, expanded equality of opportunity and equality under law, even while it honored and nurtured excellence. In recognizing that true elitism is nonexclusive and nonlimiting, we have been able to open the gates of excellent achievement and, at the same time, to drop the barriers to social mobility for a great many who were once poor, once little-educated, once newcomers to our shores, once even slaves.

Among the still-enduring premises on which they built a nation destined to become the richest and strongest in the world were these:

1. A recognition of the sanctity and importance of the individual.
2. A safeguard of property rights, which has acted as a spur to individual initiative.
3. A sense of materialism and pragmatism, along with a faith in the expansion of what is possible.
4. A regard for divine Providence, and for the moral guidance that Judeo-Christian principles have given the Western world for thousands of years.
5. A belief in the highest degree of individual freedom, consistent with a well-ordered society.

There was no specific mention of excellence in any of the expressions of these ideas, yet excellence is locked into the fiber of all of them.

Of course, some of the basic equations have changed drastically since the days when America was young. The vast new lands that were opened up made land cheap and human labor dear. A natural consequence was an upgrading of the value of human efforts and, by the same token, of individual human beings and the contributions they could make. Now no great new tracts of land are being opened; the earth's wealth and its resources are being used up faster than they are being discovered; but the number of people on earth continues to grow factorially. Does this mean we are now about to devalue people, their freedom, and what they contribute?

I would hope not, and so would you. But I am far from wholly reassured by what I see taking place. Today there seems to me to be a constricting of essential freedoms—in our economic lives, in our professional practices, and in many forms of individual expression. I think the losses of liberty have been great, and I fear the gains are small. Yet, freedom is intrinsic to the human spirit, and is deeply ingrained in the American tradition. If we are to be great, it is only the finest efforts of men and women working in an atmosphere of freedom that will make us so. Any civilization that is to leave a worthwhile heritage to the future must create works of enduring quality, whether tangible or abstract. That means we must seek to bring excellence into all of our lives, persistently, tirelessly. It is difficult to see how a viable free society can have any other goal.

BIOGRAPHIES

Biographies

LORD ASHBY, fellow of Clare College, Cambridge University, is widely known for his many contributions to experimental plant biology and has held faculty appointments at the universities of London, Bristol, Sydney (Australia), and Manchester. He also has served as master of Clare College, president, chancellor, and vice-chancellor of Queen's University, Belfast, and vice-chancellor of Cambridge. From 1970 to 1973, he chaired the Royal Commission on Environmental Pollution. Lord Ashby is a fellow of the Royal Society.

WILLIAM O. BAKER is president of Bell Laboratories, Inc., and vice chairman of The Rockefeller University Board of Trustees. Prior to becoming president, Dr. Baker, as research vice president of Bell Laboratories, was responsible for programs that produced the laser, superconducting magnets and the strongest permanent magnets, new computer graphics, and millimeter wave communications. In 1975, he received the Gold Medal of the American Institute of Chemists and the Mellon Institute Award.

ROBERT W. BERLINER is dean and professor of physiology and medicine of the Yale University School of Medicine. Between 1968 and 1973, he served as director of laboratories and clinics and then as deputy director for science at the National Institutes of Health. A leader in renal physiology research, Dr. Berliner has published widely and has won a number of awards for his contributions to the fields of medicine and health.

CARLETON B. CHAPMAN is president of The Commonwealth Fund and has a wide background in medicine as clinician, educator, and administrator. Among posts he has held are professor of medicine at the University of Texas Southwestern Medical School, dean of Dartmouth College, and staff member and consultant at a number of hospitals. He is a former president of the American Federation for Clinical Research and of the American Heart Association.

RUTH M. DAVIS has been director of the Institute for Computer Sciences and Technology of the National Bureau of Standards since its founding in 1972. Dr. Davis is also vice chairman and chairman-elect of the National Advisory Council of the Electric Power Research Institute.

She has lectured at many colleges and universities and is the author of some fifty articles. In 1973 she received the Rockefeller Public Service Award for Professional Accomplishment and Leadership.

GERALD M. EDELMAN is Vincent Astor Professor of The Rockefeller University and in 1972 shared the Nobel Prize in Physiology or Medicine. His major research interests are in the fields of immunology and protein chemistry. In 1969, Dr. Edelman announced that his research group had deciphered for the first time the structure of gamma globulin, a chemical substance that defends the body against disease. His memberships include the National Academy of Sciences and the American Academy of Arts and Sciences.

PATRICK EUGENE HAGGERTY is general director of Texas Instruments Incorporated. Other posts he holds include chairman of the Board of Trustees of The Rockefeller University, director of A. H. Belo, and member of the executive committee of the Trilateral Commission. Mr. Haggerty is also a member of the National Academy of Engineering and of the Business Council. He has served on the President's Science Advisory Committee and the Board of Governors of the U.S. Postal Service.

PHILIP HANDLER has been president of the National Academy of Sciences since 1969 and is James B. Duke Professor of Biochemistry at the Duke University School of Medicine. The recipient of many honors and awards, Dr. Handler is a former chairman of the National Science Board of the National Science Foundation and was a member of the President's Science Advisory Committee. He is co-author of *Principles of Biochemistry* and editor of *Biology and the Future of Man*.

WILLIAM M. HUBBARD, JR. is the president of The Upjohn Company. He was on the faculty at New York University College of Medicine from 1950 to 1959, when he became dean of the University of Michigan Medical School. He was named professor of internal medicine in 1964, and in 1968 he became the first director of the University's Medical Center. He is a member of the National Science Board of the National Science Foundation.

ADOLF WALTER JANN is the president and managing director of F. Hoffmann-La Roche & Co., Ltd., Basle, Switzerland. He joined the company in 1957 as vice chairman of the board. Prior to that he had served as secretary of the Swiss Bankers Association and general manager of the Union Bank of Switzerland. In 1972, he was awarded an honorary degree in medicine by the University of Basle.

ATTALLAH KAPPAS is professor and physician-in-chief of The Rockefeller University Hospital and head of the University's metabolism-pharmacology laboratory. His area of research includes studies of the porphyrias and other genetic liver diseases, and disorders related to hereditary and acquired abnormalities of the heme pathway. He received a Burroughs Wellcome Fund Special Award in Clinical Pharmacology in 1973 and a Distinguished Service Award in Medical Sciences from the University of Chicago in 1975.

SIR PETER MEDAWAR is head of the Division of Surgical Sciences, Clinical Research Centre, Harrow, England. In 1960, he shared the Nobel Prize in Physiology or Medicine for his work on acquired immunological tolerance. From 1962 to 1971, he was director of the National Institute for Medical Research, London. A fellow of the Royal Society, Sir Peter has been awarded its Royal and Copley Medals. He is the author of five books.

AURELIO PECCEI is the chairman of the board and former president of Italconsult S.p.A., Rome. He has also been a member of the management committee of the Fiat Company and president and chief executive officer of the Olivetti Company. In 1968, Dr. Peccei founded The Club of Rome, devoted to the study of environmental problems. His interest in major world problems also is reflected by his membership on the boards of the Atlantic Institute for International Affairs, the World Wildlife Fund, and the Centre d'Etudes Industrielles, among others.

DON K. PRICE is dean of the Kennedy School of Government of Harvard University. He also has held posts as associate director and vice president of the Ford Foundation and as deputy chairman of the Research and Development Board, U.S. Department of Defense. From 1959 to 1961, Dr. Price was a member of the President's Advisory Committee on Government Organization. In 1965, he was awarded the Faculty Prize of the Harvard University Press for his book, *The Scientific Estate*.

DAVID ROCKEFELLER is chairman of the board of directors and chief executive officer of The Chase Manhattan Bank, N.A., and of The Chase Manhattan Corporation. He was chairman of the Board of Trustees of The Rockefeller University from 1950 to 1975 and is now chairman of the executive committee. Mr. Rockefeller has gained a worldwide reputation as a spokesman for the business community. He also has been a leader in many public and private projects, reflecting his interests in international, governmental, civic, and cultural affairs.

FREDERICK SEITZ is president of The Rockefeller University. Before assuming that post, he had been president of the National Academy of Sciences for seven years. Dr. Seitz began his career as a physicist, specializing in the theory of solids and in nuclear physics. From 1964 to 1965, he was dean and vice president for research at the University of Illinois. His honors and awards include the National Medal of Science and the Karl Taylor Compton Medal for distinguished statesmanship in science.

DONALD WAYNE SELDIN is William Buchanan Professor and chairman of the Department of Internal Medicine at University of Texas Southwestern Medical School. He is also chief of medical service at Parkland Memorial Hospital, Dallas, and a highly regarded consultant on national medical problems and clinical research. Dr. Seldin is a fellow of the Royal Society of Medicine and the American Academy of Arts and Sciences, and a member of the Institute of Medicine of the National Academy of Sciences.

MAURICE F. STRONG is chairman of the board and president of Petrol-Canada, a new government corporation created to oversee Canada's energy program. Before that, he had been executive director of the United Nations Environment Program. From 1966 to 1970, Mr. Strong was with the Canadian government, first as director-general of the External Aid Office and then as president, as well as chairman, of the Canadian International Development Board. Before joining the government, he had extensive experience in business and public affairs. He is a founding member of The Rockefeller University Council.

LEWIS THOMAS is president and chief executive officer of the Memorial Sloan-Kettering Cancer Center and professor of pathology and medicine at Cornell University Medical College. His interest in the underlying mechanisms of disease has led to research in neurology, pathology, pediatrics, internal medicine, and education. He is a member of the National Academy of Sciences. His book, *The Lives of a Cell, Notes of a Biology Watcher,* won a National Book Award.